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Natural language and natural selection

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Abstract: Many people have argued that the evolution of the human language faculty cannot be explained by Darwinian natural selection. Chomsky and Gould have suggested that language may have evolved as the by-product of selection for other abilities or as a consequence of as-yet unknown laws of growth and form. Others have argued that a biological specialization for grammar is incompatible with every tenet of Darwinian theory – that it shows no genetic variation, could not exist in any intermediate forms, confers no selective advantage, and would require more evolutionary time and genomic space than is available. We examine these arguments and show that they depend on inaccurate assumptions about biology or language or both. Evolutionary theory offers clear criteria for when a trait should be attributed to natural selection: complex design for some function, and the absence of alternative processes capable of explaining such complexity. Human language meets these criteria: Grammar is a complex mechanism tailored to the transmission of propositional structures through a serial interface. Autonomous and arbitrary grammatical phenomena have been offered as counterexamples to the position that language is an adaptation, but this reasoning is unsound: Communication protocols depend on arbitrary conventions that are adaptive as long as they are shared. Consequently, language acquisition in the child should systematically differ from language evolution in the species, and attempts to analogize them are misleading. Reviewing other arguments and data, we conclude that there is every reason to believe that a specialization for grammar evolved by a conventional neo-Darwinian process.

Keywords: biology of language; evolution; grammatical theory; language; language acquisition; language universals; natural selection; origin of language; psycholinguistics

> Language could not have begun in the form it was said to have taken in the first recorded utterance of Thomas Babbington Macaulay (the infant Lord Macaulay): Once when he was taken out, his hostess accidently spilled hot tea on him. The little lad first bawled his head off, but when he had calmed he said in answer to his hostess' concern, "Thank you, Madam, the agony is sensibly abated."
> — P. B. and J. S. Medawar

1. Introduction

All human societies have language. As far as we know, they always did; language was not invented by some groups and spread to others like agriculture or the alphabet. All languages are complex computational systems using the same basic kinds of rules and representations, with no notable correlation with technological progress: The grammars of industrial societies are no more complex than the grammars of hunter-gatherers; Modern English is not an advance over Old English. Within societies, individual humans are proficient language users regardless of intelligence, social status, or level of education. Children are fluent speakers of complex grammatical sentences by the age of three, without benefit of formal instruction. They are capable of inventing languages that are more systematic than those they hear, showing resemblances to languages that they have never heard, and they obey subtle grammatical principles for which there is no evidence in their environments. Disease or injury can make people linguistic savants while severely retarded, or linguistically impaired with normal intelligence. Some language disorders are genetically transmitted. Aspects of language skill can be linked to characteristic regions of the human brain. The human vocal tract is tailored to the demands of speech, compromising other functions such as breathing and swallowing. Human auditory perception shows complementary specializations toward the demands of decoding speech sounds into linguistic segments.

This list of facts (see also Pinker 1989a) suggests that the ability to use a natural language belongs more to the study of human biology than human culture; it is a topic like echolocation in bats or stereopsis in monkeys, not like writing or the wheel. All modern students of language agree that at least some aspects of language are due to species-specific, task-specific biological abilities, though of course there are radical disagreements about specifics. A prominent position, outlined by Chomsky (1965; 1980a; 1981; 1986; 1988a), Fodor (1983), Lenneberg (1964;

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1967), and Liberman (Liberman et al. 1967; Liberman & Mattingly 1989), is that the mind is composed of autonomous computational modules – mental faculties or "organs" – and that the acquisition and representation of language is the product of several such specialized modules.

It would be natural, then, to expect everyone to agree that human language is the product of Darwinian natural selection. The only successful account of the origin of complex biological structure is the theory of natural selection, the view that the differential reproductive success associated with heritable variation is the primary organizing force in the evolution of organisms (Darwin 1859; see Bendall 1983 for a contemporary perspective). But, surprisingly, this conclusion is controversial. Noam Chomsky, the world's best-known linguist, and Stephen Jay Gould, the world's best-known evolutionary theorist, have repeatedly suggested that language may not be the product of natural selection, but a side effect of other evolutionary forces such as an increase in overall brain size and constraints of as yet unknown laws of structure and growth (e.g., Chomsky 1972; 1982a; 1982b; 1988a; 1988b; Gould 1987a; Gould & Piattelli-Palmarini 1987). Recently, Massimo Piattelli-Palmarini (1989), a close correspondent with Gould and Chomsky, has done the field a service by formulating a particularly strong version of their positions and articulating it in print. Premack (1985; 1986) and Mehler (1985) have expressed similar views.

In this target article, we will examine this position in detail and will come to a very different conclusion. We will argue that there is every reason to believe that language has been shaped by natural selection as it is understood within the orthodox "synthetic" or "neo-Darwinian" theory of evolution (Mayr 1982). In one sense our goal is incredibly boring. All we argue is that language is no different from other complex abilities such as echolocation or stereopsis, and that the only way to explain the origin of such abilities is through the theory of natural selection. One might expect our conclusion to be accepted without much comment by all but the most environmentalist of language scientists, as indeed it is by such researchers as Bickerton 1981, Liberman & Mattingly 1989, Lieberman 1984, and, in limited respects, by Chomsky himself in some strands of his writings.[1] On the other hand, when two such important scholars as Chomsky and Gould repeatedly urge us to consider a startling contrary position, their arguments can hardly be ignored. Indeed, these arguments have had a strong effect on many cognitive scientists, and the nonselectionist view has become the consensus in many circles.

Furthermore, a lot is at stake if our boring conclusion is wrong. We suspect that many biologists would be surprised at the frequent suggestion that the complexity of language cannot be explained through natural selection. For example, Chomsky has made the following statements:

> [An innate language faculty] poses a problem for the biologist, since, if true, it is an example of true 'emergence' – the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization (1972, p. 70).
>
> It is perfectly safe to attribute this development [of innate mental structure] to "natural selection," so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation for these phenomena (1972, p. 97).
>
> Evolutionary theory is informative about many things, but it has little to say, as of now, of questions of this nature [e.g., the evolution of language]. The answers may well lie not so much in the theory of natural selection as in molecular biology, in the study of what kinds of physical systems can develop under the conditions of life on earth and why, ultimately because of physical principles (1988a, p. 167).
>
> It does seem very hard to believe that the specific character of organisms can be accounted for purely in terms of random mutation and selectional controls. I would imagine that the biology of a 100 years from now is going to deal with the evolution of organisms the way it now deals with the evolution of amino acids, assuming that there is just a fairly small space of physically possible systems that can realize complicated structures. . . . Evolutionary theory appears to have very little to say about speciation, or about any kind of innovation. It can explain how you get a different distribution of qualities that are already present, but it does not say much about how new qualities can emerge (1982a, p. 23).

If findings coming out of the study of language forced biologists to such conclusions, it would be big news.

There is another reason to scrutinize the nonselectionist theory of language. If a current theory of language is truly incompatible with the neo-Darwinian theory of evolution, one could hardly blame someone for concluding that it is not the theory of evolution that must be questioned, but the theory of language. Indeed, this argument has been the basis of critiques of Chomsky's theories by Bates et al. (1989), Greenfield (1987), and Lieberman (1984; 1989a), who are nonetheless strange bedfellows with Chomsky in doubting whether an innate generative grammar could have evolved by natural selection. Because we are impressed both by the synthetic theory of evolution and by the theory of generative grammar, we hope that we will not have to choose between the two.

In this article, we first examine arguments from evolutionary biology about when it is appropriate to invoke natural selection as an explanation for the evolution of some trait. We then apply these tests to the case of human language, and conclude that language passes. We examine the motivations for the competing nonselectionist position and suggest that they have little to recommend them. In the final section, we refute the arguments that have claimed that an innate specialization for grammar is incompatible with the tenets of a Darwinian account and thus that the two are incompatible.

2. The role of natural selection in evolutionary theory

Gould has frequently suggested that evolutionary theory is in the throes of a scientific revolution (e.g., Eldredge & Gould 1972; Gould 1980). Two cornerstones of the Darwinian synthesis, adaptationism and gradualism, are, he argues, under challenge. Obviously, if strict Darwinism is false in general it should not be used to explain the origin of language.

2.1. Nonselectionist mechanisms of evolutionary change

In a classic paper, Gould and Lewontin (1979) warn against "naive adaptationism," the inappropriate use of adaptive theorizing to explain traits that have emerged for other reasons (see also Kitcher 1985a; Lewontin 1978). The argument is illustrated by an analogy with the mosaics on the dome and spandrels of the San Marco basilica in Venice:

> Spandrels – the tapering triangular spaces formed by the intersection of two rounded arches at right angles . . . are necessary architectural by-products of mounting a dome on rounded arches. Each spandrel contains a design admirably fitted into its tapering space. An evangelist sits in the upper part flanked by the heavenly cities. Below, a man representing one of the four biblical rivers . . . pours water from a pitcher in the narrowing space below his feet.
>
> The design is so elaborate, harmonious, and purposeful that we are tempted to view it as the starting point of any analysis, as the cause in some sense of the surrounding architecture. But this would invert the proper path of analysis. The system begins with an architectural constraint: the necessary four spandrels and their tapering triangular form. They provide a space in which the mosaicists worked; they set the quadripartite symmetry of the dome above.
>
> Such architectural constraints abound, and we find them easy to understand because we do not impose our biological biases upon them. . . . Anyone who tried to argue that the structure [spandrels] exists because of [the designs laid upon them] would be inviting the same ridicule that Voltaire heaped on Dr. Pangloss: "Things cannot be other than they are . . . Everything is made for the best purpose. Our noses were made to carry spectacles, so we have spectacles. Legs were clearly intended for breeches, and we wear them." . . . Yet evolutionary biologists, in their tendency to focus exclusively on immediate adaptation to local conditions, do tend to ignore architectural constraints and perform just such an inversion of explanation (pp. 147–49).

Unconvincing adaptationist explanations, which Gould and Lewontin compare to Kipling's "Just-so stories," are easy to find. In the Science and Technology section of the Boston Globe in March 1987, an article noted that the number of teats in different mammals ought to correspond not to the average litter size but to the largest litter size that can occur for that species within some bound of probability. Because humans ordinarily bear single children but not infrequently have twins, we have an explanation for why humans have two breasts, not one. The author did not discuss the possibility that the bilateral symmetry that is so basic to the mammalian body plan makes the appearance of one-breasted humans rather unlikely.

Gould and Lewontin describe a number of nonadaptationist mechanisms that they feel are frequently not tested in evolutionary accounts: genetic drift, laws of growth and form (such as general allometric relations between brain and body size), direct induction of form by environmental forces such as water currents or gravity, the effects of accidents of history (which may trap organisms in local maxima in the adaptive landscape), and "exaptation" (Gould & Vrba 1982), whereby new uses are made of parts that were originally adapted to some other function or of spandrels that had no function at all but were present for reasons of architecture, development, or history. They point out that Darwin himself had this pluralistic view of evolution, and that there was an "unfairly maligned" nonadaptationist approach to evolution, prominent in continental Europe, that stressed constraints on "Baupläne" (architectural plans) flowing from phyletic history and embryological development. This body of research, they suggest, is an antidote to the tendency to treat an organism as a bundle of traits or parts, each independently shaped by natural selection.

2.2. Limitations on nonselectionist explanations

The Gould and Lewontin argument could be interpreted as stressing that because the neo-Darwinian theory of evolution includes nonadaptationist processes, it is bad scientific practice not to test them as alternatives to natural selection in any particular instance. However, they are often read as having outlined a radical new alternative to Darwin, in which natural selection is relegated to a minor role. Though Gould and Lewontin clearly eschew this view in their paper, Gould has made such suggestions subsequently (e.g., Gould 1980), and Piattelli-Palmarini (1989, p. 1) has interpreted it as such when he talks of Darwinian natural selection being replaced by "a better evolutionary theory (one based on 'exaptation')." The reasons why we should reject this view were spelled out clearly by Williams (1966), and have been amplified recently by Dawkins (1983; 1986).

The key point that blunts the Gould and Lewontin critique of adaptationism is that *natural selection is the only scientific explanation of adaptive complexity.* "Adaptive complexity" describes any system composed of many interacting parts where the details of the parts' structure and arrangement suggest design to fulfill some function. The vertebrate eye is the classic example. The eye has a transparent refracting outer cover, a variable-focus lens, a light-sensitive layer of neural tissue lying at the focal plane of the lens, a diaphragm whose diameter changes with illumination level, muscles that move it in precise conjunction and convergence with those of the other eye, and elaborate neural circuits that respond to patterns defining edges, colors, motion, and stereoscopic disparity. It is impossible to make sense of the structure of the eye without noting that it appears as if it were designed for the purpose of seeing – if for no other reason than that the man-made tool for image formation, the camera, displays an uncanny resemblance to the eye. Before Darwin, theologians, notably William Paley, pointed to the eye's exquisite design as evidence for the existence of a divine designer. Darwin showed how such "organs of extreme perfection and complication" could arise from the purely physical process of natural selection.

The essential point is that no physical process other than natural selection can explain the evolution of an organ like the eye. The reason for this is that structures that can do what the eye does are extremely low-probability arrangements of matter. By an unimaginably large margin, most objects defined by the space of biologically possible arrangements of matter cannot bring an image into focus, modulate the amount of incoming light, respond to the presence of edges and depth boundaries, and so on. The odds that genetic drift, say, would result in the fixation within a population of just those genes that would give rise to such an object are infinitesimally small, and such an event would be virtually a miracle. This is also true of the other nonselectionist mechanisms outlined by Gould and Lewontin. It is absurdly improbable that some general law of growth and form could give rise to a functioning vertebrate eye as a byproduct of some other trend such as an increase in size of some other part. Likewise, one need not consider the possibility that some organ that arose as an adaptation to some other task, or a spandrel defined by other body parts, just happened to have a transparent lens surrounded by a movable diaphragm in front of a light-sensitive layer of tissue lying at its focal plane. Natural selection – the retention across generations of whatever small, random modifications yield improvements in vision that increase chances of survival and reproduction – is the only physical process capable of creating a functioning eye, because it is the only physical process in which the criterion of being good at seeing can play a causal role. As such, it is the only process that can lead organisms along the path in the astronomically vast space of possible bodies leading from a body with no eye to a body with a functioning eye.

This argument is obviously incomplete, as it relies on the somewhat intuitive notion of "function" and "design." A skeptic might accuse the proponent of circularity, asking why a lump of clay should not be considered well designed to fulfill the function of taking up exactly the region of space that it in fact takes up. But the circle can be broken in at least three ways. First, biologists need posit far fewer functions than there are biological systems; new functions are not invented for each organ of each organism. Furthermore, each legitimate function can be related via a direct plausible causal chain to other functions and – critically –to the overall function of survival and reproduction. Finally, convergent evolution and resemblance to human artifacts fulfilling the same putative function give independent criteria for design. But regardless of the precise formulation of the modern argument from design (see, e.g., Cummins 1984), it is not controversial in practice. Gould himself readily admits that natural selection is the cause of structures such as the vertebrate eye, and he invokes the criterion of engineering design, for example, to rescue Darwinism itself from the charge of circularity (Gould 1977a). Presumably, this is why Gould and Lewontin concede that they agree with Darwin that natural selection is "the most important of evolutionary mechanisms."

What, then, is the proper relation between selectionist and nonselectionist explanations in evolution? The least interesting case involves spandrels that are not involved in any function or behavior, such as the redness of blood, the V-shaped space between a pair of fingers, the hollow at the back of a knee, the fact there are a prime number of digits on each limb, and so on. The mere presence of these *epiphenomenal spandrels*, that play no direct role in the explanation of any species-typical behavior or function, says nothing about whether the structures that they are associated with were shaped by selection. There are as many of them as there are ways of describing an organism that do not correspond to its functional parts.

Much more important are cases where spandrels are modified and put to use. However, in such cases of *modified spandrels*, selection plays a crucial role. Putting a dome on top of four arches gives you a spandrel, but it does not give you a mosaic depicting an evangelist and a man pouring water out of a pitcher. That would *really* be a miracle. To get the actual mosaic you need a designer. The designer corresponds to natural selection. Spandrels, exaptations, laws of growth, and so on can explain the basic plans, parts, and materials that natural selection works with – as Jacob (1977) put it, nature is a tinkerer, not an engineer with a clean drawing board. The best examples of structures produced entirely by nonadaptationist mechanisms are generally one-part or repetitive shapes or processes that correspond to simple physical or geometric laws, such as chins, hexagonal honeycombs, large heads on large bodies, and spiral markings. But, as Darwin stressed, when such parts and patterns are modified and combined into complex biological machines fulfilling some delicate function, these subsequent modifications and arrangements must be explained by natural selection.

The real case of evolution without selection consists of the use of *unmodified spandrels*. Gould (1987a) describes a kind of wading bird that uses its wings primarily to block reflections on the surface of water while looking for fish. The possibility that some useful structure is an unmodified spandrel is the most interesting implication of the Gould-Lewontin argument, because Darwinian natural selection would really play no role. Note, though, that unmodified spandrels have severe limitations. A wing used as a visor is a case where a structure designed for a complex engineering task that most arrangements of matter do not fulfill, such as controlled flight, is exapted to a simple engineering task that many arrangements of matter do fulfill, such as screening out reflections (we are reminded of the paperweight and aquarium depicted in *101 Uses for a Dead Computer*). When the reverse happens, such as when a solar heat exchanger is retooled as a fully functioning wing in the evolution of insects (Kingsolver & Koehl 1985), natural selection must be the cause.

We are going over these criteria for invoking natural selection in such detail because they are so often misunderstood. We hope we have made it clear why modern evolutionary biology does *not* license Piattelli-Palmarini's conclusion that "since language and cognition probably represent the most salient and the most novel biological traits of our species, . . . it is now important to show that they may well have arisen from totally extra-adaptive mechanisms." And Piattelli-Palmarini is not alone. In many discussions with cognitive scientists, we have found that adaptation and natural selection have become dirty words. Anyone invoking them is accused of being a naive adaptationist, or even of "misunderstanding evolution."

Worst of all, such a person is open to easy ridicule as a Dr. Pangloss telling Just-so stories! (Premack's 1986 reply to Bickerton, 1986, is typical.) Given the uncontroversially central role of natural selection in evolution, this state of affairs is unfortunate. We suspect that many people have acquired much of their knowledge of evolutionary theory from Gould's deservedly popular essays. These essays present a view of evolution that is vastly more sophisticated than the nineteenth-century versions of Darwin commonly taught in high schools and even colleges. But Gould can easily be misread as fomenting a revolution rather than urging greater balance within current biological research, and his essays do not emphasize the standard arguments for when it is appropriate, indeed necessary, to invoke natural selection.

Also lurking beneath people's suspicions of natural selection is a set of methodological worries. Isn't adaptationism fundamentally untestable, hence unscientific, because adaptive stories are so easy to come by that when one fails, another can always be substituted? Gould and Lewontin may be right in saying that biologists and psychologists have leapt too quickly to unmotivated and implausible adaptationist explanations, but this has nothing to do with the logic of adaptationist explanations per se. Glib, unmotivated proposals can come from all kinds of theories. To take an example close to home, the study of the evolution of language attained its poor reputation precisely because of the large number of silly *non*adaptationist hypotheses that were proposed. For instance, it has been argued that language arose from mimicry of animal calls, imitations of physical sounds, or grunts of exertion (the infamous "bow-wow," "ding-dong," and "heave-ho" theories).

Specific adaptationist proposals are testable in principle and in practice (see Dennett 1983; Kitcher 1985a; Maynard Smith 1984; Mayr 1982; Sober 1984; Williams 1966.) Supplementing the criterion of complex design, one can determine whether putatively adaptive structures are correlated with the ecological conditions that make them useful, and, under certain circumstances, one can actually measure the reproductive success of individuals possessing them to various degrees (see, e.g., Clutton-Brock 1983). Of course, the entire theory of natural selection may be literally unfalsifiable in the uninteresting sense that elaborations can always rescue its empirical failings, but this is true of all large-scale scientific theories. Any such theory is supported to the extent that the individual elaborations are mutually consistent, motivated by independent data, and few in number compared to the phenomena to be explained.[2]

Indeed, one could argue that it is nonadaptationist accounts that are often in grave danger of vacuity. Specific adaptationist proposals may be unmotivated, but they are within the realm of biological and physical understanding, and often the problem is simply that we lack the evidence to determine which account within a set of alternative adaptive explanations is the correct one. Nonadaptationist accounts that merely suggest the possibility that there is some hitherto-unknown law of physics or constraint on form – a "law of eye-formation," to take a caricatured example –are, in contrast, empty and nonfalsifiable.

2.3. Two issues that are independent of selectionism

There are two other issues that Gould includes in his depiction of a scientific revolution in evolutionary theory. It is important to see that they are largely independent of the role of selection in evolutionary change.

2.3.1. Gradualism. According to the theory of "punctuated equilibrium" (Eldredge & Gould 1972; Gould & Eldredge 1977), most evolutionary change does not occur continuously within a lineage, but is confined to bursts of change that are relatively brief on the geological time scale, generally corresponding to speciation events, followed by long periods of stasis. Gould has suggested that the theory has some very general and crude parallels with approaches to evolution that were made disreputable by the neo-Darwinian synthesis, approaches that go by the names of "saltationism," "macromutations," or "hopeful monsters." (e.g., Gould 1981). He is emphatic, however, that punctuated equilibrium is "a theory about ordinary speciation (taking tens of thousands of years) and its abrupt appearance at low scales of geological resolution, not about ecological catastrophe and sudden genetic change" (Gould 1987b, p. 234). Many other biologists see evolutionary change in an even more orthodox light. They attribute the sudden appearance of fully formed new kinds of organisms in the fossil record to the fact that speciation typically takes place in small, geographically isolated populations. Therefore, transitional forms, even if evolving over very long time spans, are unlikely to appear in the fossil record until they reinvade the ancestral territory; it is only the invasion that is sudden (see, e.g., Ayala 1983; Dawkins 1986; Mayr 1982; Stebbins & Ayala 1981). In any case, it is clear that evolutionary change is gradual from generation to generation, in full agreement with Darwin. Thus, Piattelli-Palmarini (1989, p. 8) expresses a common misunderstanding when he interprets the theory of punctuated equilibrium as showing that "many incomplete series in the fossil record are incomplete, not because the intermediate forms have been lost *for* us, but because they simply never existed."

Once again, the explanation of adaptive complexity is the key reason one should reject nongradual change as playing an important role in evolution. An important Darwinian insight, reinforced by Fisher (1930), is that the only way for complex design to evolve is through a sequence of mutations with small effects. Although it may not literally be impossible for an organ like the eye to emerge across one generation from no eye at all, the odds of this happening are unimaginably low. A random large leap in the space of possible organic forms is astronomically unlikely to land an organism into a region with a fully formed functioning eye. Only a hill-climbing process, with each small step forced in the direction of forms with better vision, can guide the lineage to such a minuscule region of the space of possible forms within the lifetime of the universe.

None of this is to deny that embryological processes can result in quite radical single-generation morphological changes. "Homeotic" mutations causing slight changes in the timing or positioning of epigenetic processes can result in radically new kinds of offspring, such as fruit flies with legs growing where their antenna should be, and it is possible that some speciation events may have begun with such large changes in structure. There is a clear sense, however, in which such changes are still gradual, because they only involve a gross modification or duplication of existing structure, not the appearance of a new kind of structure (see Dawkins 1983).

2.3.2. Exaptation. Exaptation is another process that is sometimes discussed as if it were incompatible both with adaptationism and with gradualism. People often wonder whether each of the "numerous, successive, slight modifications" from an ancestor lacking an organ to a modern creature enjoying the fully functioning organ leads to an improvement in the function, as if it should if the necessary evolutionary sequence is to be complete. Piattelli-Palmarini cites Kingsolver and Koehl's (1985) study of qualitative shifts during the evolution in insects of wings that are ineffective for flight below a certain size but effective as solar heat exchange panels precisely within that range. (The homologies among parts of bat wings, seal flippers, horse forelimbs, and human arms are a far older example.) Nevertheless, such exaptations are still gradual and are still driven by selection; there must be an intermediate evolutionary stage at which the part can subserve both functions (Mayr 1982), after which the process of natural selection shapes it specifically for its current function. Indeed, the very concept of exaptation is similar to what Darwin called "preadaptation," which played an important role in his explanation of "the incipient stages of useful structures."

Furthermore, it is crucial to understand that exaptation is merely one empirical possibility, not a universal law of evolution. Gould is often quoted as saying "We avoid the excellent question, What good is 5 percent of an eye? by arguing that the possessor of such an incipient structure did not use it for sight" (1977b, p. 107). (Of course, no ancestor to humans literally had 5 percent of a human eye; the expression refers to an eye that has 5 percent of complexity of a modern eye.) In response, Dawkins (1986, p. 81) writes: "An ancient animal with 5 percent of an eye might indeed have used it for something other than sight, but it seems to me at least as likely that it used it for 5 percent vision. . . . Vision that is 5 percent as good as yours or mine is very much worth having in comparison with no vision at all. So is 1 percent vision better than total blindness. And 6 percent is better than 5, 7 percent better than 6, and so on up the gradual, continuous series." Indeed, Darwin (1859) sketched out a hypothetical sequence of intermediate forms in the evolution of the vertebrate eye, all with counterparts in living organisms, each used for vision.

In sum, the positions of Gould, Lewontin, and Eldredge should not be seen as radical revisions of the theory of evolution, but as a shift in emphasis within the orthodox neo-Darwinian framework. As such they do not invalidate gradual natural selection as the driving force behind the evolution of language on a priori grounds. Furthermore, there are clear criteria for when selectionist and nonselectionist accounts should be invoked to explain some biological structure: complex design to carry out some reproductively significant function, versus the existence of a specific physical, developmental, or random process capable of explaining the structure's existence. With these criteria in hand, we can turn to the specific problem at hand: the evolution of language.

3. Design in language

Do the cognitive mechanisms underlying language show signs of design for some function in the same way that the anatomical structures of the eye show signs of design for the purpose of vision? What are the engineering demands on a system that must carry out such a function? And are the mechanisms of language tailored to meet those demands? We will suggest that language shows signs of design for the communication of propositional structures over a serial channel.

3.1. An argument for design in language

Humans acquire a great deal of information during their lifetimes. Because this acquisition process occurs at a rate far exceeding that of biological evolution, it is invaluable in dealing with causal contingencies of the environment that change within a lifetime, and provides a decisive advantage in competition with other species that can only defend themselves against new threats in evolutionary time (Brandon & Hornstein 1986; Tooby & deVore 1987). There is an obvious advantage in being able to acquire such information about the world second-hand: By tapping into the vast reservoir of knowledge accumulated by some other individual, one can avoid having to duplicate the possibly time-consuming and dangerous trial-and-error process that won that knowledge. Furthermore, within a group of interdependent, cooperating individuals, the states of other individuals are among the most significant things in the world worth knowing about. Therefore, communication of knowledge and internal states is useful to creatures who have a lot to say and who are on speaking terms. (In section 5.3, we discuss evidence that our ancestors were such creatures.)

Human knowledge and reasoning, it has been argued, is couched in a "language of thought" that is distinct from external languages such as English or Japanese (Fodor 1975). The propositions in this representational medium are relational structures whose symbols pertain to people, objects, and events, the categories they belong to, their distribution in space and time, and their causal relations to one another (Jackendoff 1983; Keil 1979). The causal relations governing the behavior of other people are understood as involving their beliefs and desires, which can be reconsidered as relations between an individual and the proposition that represents the content of that belief or desire (Fodor 1975; 1987).

This makes the following kinds of contents worthy of communication among humans. We would want to be able to refer to individuals and classes, to distinguish among basic ontological categories (things, events, places, times, manners, and so on), to talk about events and states, distinguishing the participants in the event or state according to role (agents, patients, goals), and to talk about the intentional states of ourselves and others. Also, we would want the ability to express distinctions of truth value, modality (necessity, possibility, probability, factivity), to comment on the time of an event or state, including both its distribution over time (continuous, iterative, punctate) and its overall time of occurrence. One might also demand the ability to encode an unlimited number of predicates, arguments, and propositions. In addition, it would be useful to be able to use the same propositional content within different speech acts; for instance, as a question, a statement, or a command. Superimposed on all of this we might ask for an ability to focus or to put into the background different parts of a proposition, so as to tie the speech act into its context of previously conveyed information and patterns of knowledge of the listener.

The vocal-auditory channel has some desirable features as a medium of communication: It has a high bandwidth, its intensity can be modulated to conceal the speaker or to cover large distances, and it does not require light, proximity, a face-to-face orientation, or tying up of the hands. It is essentially a serial interface, however, lacking the full two-dimensionality needed to convey graph or tree structures and typographical devices such as fonts, subscripts, and brackets. The basic tools of a coding scheme using such a channel are an inventory of distinguishable symbols and their concatenation.

Thus, grammars for spoken languages must map propositional structures onto a serial channel, minimizing ambiguity in context, under the further constraints that the encoding and decoding be done rapidly, by creatures with limited short-term memories, according to a code that is shared by an entire community of potential communicants.

The fact that language is a complex system of many parts, each tailored to mapping a characteristic kind of semantic or pragmatic function onto a characteristic kind of symbol sequence, is so obvious in linguistic practice that it is usually not seen as worth mentioning. Let us list some uncontroversial facts about substantive universals, the building blocks of grammars that all theories of universal grammar posit, either as an explicit inventory or as a consequence of somewhat more abstract mechanisms.

Grammars are built around symbols for *major lexical categories* (noun, verb, adjective, preposition) that can enter into rules specifying telltale surface distributions (e.g., verbs but not nouns generally take unmarked direct objects), inflections, and lists of lexical items. Together with *minor categories* that characteristically co-occur with the major ones (e.g., articles with nouns), the different categories are thus provided with the means of being distinguished in the speech string. These distinctions are exploited to distinguish basic ontological categories such as things, events or states, and qualities (see, e.g., Jackendoff 1983; 1990).

Major phrasal categories (noun phrase, verb phrase, and so forth) start off with a major lexical item, the "head," and allow it to be combined with specific kinds of affixes and phrases. The resulting conglomerate is then used to refer to entities in our mental models of the world. Thus, a noun like *dog* does not itself describe anything but it can combine with articles and other parts of speech to make noun phrases, such as *those dogs, my dog,* and *the dog that bit me,* and it is these noun phrases that are used to describe things. Similarly, a verb like *hit* is made into a verb phrase by marking it for tense and aspect and adding an object, thus enabling it to describe an event. In general, words encode abstract general categories and only by contributing to the structure of major phrasal categories can they describe particular things, events, states, locations, and properties. This mechanism enables the language user to refer to an unlimited range of specific entities while possessing only a finite number of lexical items (see, e.g., Bloom 1989; Jackendoff 1977).

Phrase structure rules (e.g., "X-bar theory" or "immediate dominance rules") force concatenation in the string to correspond to semantic connectedness in the underlying proposition, and thus provide linear cues of underlying structure, distinguishing, for example, *Large trees grow dark berries* from *Dark trees grow large berries* (see, e.g., Gazdar et al. 1985; Jackendoff 1977.)

Rules of *linear order* (e.g., "directional parameters" ordering heads, complements, and specifiers, or "linear precedence rules") allow the order of words within these concatenations to distinguish among the argument positions that an entity assumes with respect to a predicate, distinguishing *Man bites dog* from *Dog bites man* (see Gazdar et al. 1985; Travis 1984).

Case affixes on nouns and adjectives can take over these functions, marking nouns according to argument role and linking noun with predicate even when the order is scrambled. This redundancy can free up the device of linear order, allowing it to be exploited to convey relations of prominence and focus, which can thus mesh with the necessarily temporal flow of attention and knowledge acquisition in the listener.

Verb affixes signal the temporal distribution of the event that the verb refers to (aspect) and the time of the event (tense); when separate aspect and tense affixes co-occur, they are in a universally preferred order (aspect closer to the verb; Bybee 1985). Given that man-made timekeeping devices play no role in species-typical human thought, some other kind of temporal coordinates must be used, and languages use an ingenious system that can convey the time of an event relative to the time of the speech act itself and relative to a third, arbitrary reference time (thus, we can distinguish between *John has arrived, John had arrived* (*when Mary was speaking*), *John will have arrived* (*before Mary speaks*), and so on; Reichenbach 1947). Verb affixes also typically agree with the subject and other arguments, and thus provide another redundant mechanism that can convey predicate-argument relations by itself (e.g., in many Native American languages such as Cherokee and Navajo) or that can eliminate ambiguity left open by other mechanisms (distinguishing, e.g., *I know the boy and the girl who like chocolate* from *I know the boy and the girl who likes chocolate*).

Auxiliaries, which occur either as verb affixes (where they are distinguished from tense and aspect affixes by proximity to the verb) or in one of three sentence-peripheral positions (first, second, last), convey relations that have logical scope over the entire proposition (mirroring their peripheral position) such as truth value, modality, and illocutionary force (see Steele et al. 1981).

Languages also typically contain a small inventory of phonetically reducible morphemes – *pronouns* and other *anaphoric elements* – that by virtue of encoding a small set of semantic features such as gender and humanness, and being restricted in their distribution, can convey patterns of coreference among different participants in complex relations without the necessity of repeating lengthy definite descriptions (e.g., as in *A boy showed a dog to a girl and then he/she/it touched him/her/it/herself*). (See Chomsky 1981; Wexler & Manzini 1984.)

Mechanisms of *complementation* and *control* govern the expression of propositions that are arguments of other propositions, using specific complementizer morphemes signaling the periphery of the embedded proposition and indicating its relation to the embedding one, and licensing the omission of repeated phrases referring to participants playing certain combinations of roles. This allows the expression of a rich set of propositional attitudes within a belief-desire folk psychology, such as *John tried to come, John thinks that Bill will come, John hopes for Bill to come, John convinced Bill to come*, and so on. (See Bresnan 1982).

In *wh*-movement (as in *wh*-questions and relative clauses) there is a tightly constrained co-occurrence pattern between an empty element (a "trace" or "gap") and a sentence-peripheral quantifier (e.g., *wh*-words). The quantifier-word can be specific as to illocutionary force (question versus modification), ontological type (time, place, purpose), feature (animate/inanimate), and role (subject/object), and the gap can occur only in highly constrained phrase structure configurations. The semantics of such constructions allow the speaker to fix the reference of, or request information about, an entity by specifying its role within any proposition. One can refer not just to any dog but to *the dog that Mary sold ____ to some students last year;* one can ask not only for the names of just any old interesting person but specifically *Who was that woman I saw you with ____?* (See, e.g., Chomsky 1981; Gazdar et al. 1985; Kaplan & Bresnan, 1982.)

And this is only a partial list, focusing on sheer expressive power. One could add to it the many syntactic constraints and devices whose structure minimizes memory load and the likelihood of pursuing local garden paths in speech comprehension (e.g., Berwick & Weinberg 1984; Berwick & Wexler 1987; Bever 1970; Chomsky & Lasnik 1977; Frazier et al. 1983; Hawkins & Cutler 1988; Kuno 1973; 1974), or to ease the task of analysis for the child learning the language (e.g., Morgan 1986; Pinker 1984; Wexler & Culicover 1980). On top of that, there are the rules of segmental phonology that smooth out arbitrary concatenations of morphemes into a consistent sound pattern that juggles demands of ease of articulation and perceptual distinctness; the prosodic rules that disambiguate syntax and communicate pragmatic and illocutionary information; the articulatory programs that achieve rapid transmission rates through parallel encoding of adjacent consonants and vowels; and on and on. Language seems to be a fine example of "that perfection of structure and coadaptation which justly excites our admiration" (Darwin 1859, p. 26).

As we write these words, we can hear the swelling chorus: "Pangloss! Just-so stories!" Haven't we just thought up accounts about functions post hoc after examining the structure? How do we know that the neural mechanisms were not there for other reasons, and that once they were there they were just put to various convenient uses by the first language users, who then conveyed their invention to subsequent generations?

3.2. Is the argument for language design a just-so story?

First of all, there is nothing particularly ingenious, contorted, or exotic about our claims for substantive universals and their semantic functions. Any one of them could have been lifted out of the pages of linguistics textbooks. It is hardly the theory of evolution that motivates the suggestion that phrase-structure rules are useful in conveying relations of modification and predicate-argument structure.

Second, it is not necessarily illegitimate to infer both special design and adaptationist origins on the basis of function itself. It all depends on the complexity of the function from an engineering point of view. If someone told you that John uses *X* as a sunshade or a paperweight, you would certainly be hard-pressed to guess what *X* is or where *X* came from, because all sorts of things make good sunshades or paperweights. But if someone told you that John uses *X* to display television broadcasts, it would be a very good bet that *X* is a television set or is similar in structure to one, and that it was designed for that purpose. The reason is that it would be vanishingly unlikely for something that was not designed as a television set to display television programs; the engineering demands are simply too complex.

This kind of reasoning is commonly applied in biology when high-tech abilities such as bat sonar are discovered. We suggest that human language is a similar case. We are not talking about noses holding up spectacles. Human language is a device capable of communicating exquisitely complex and subtle messages, from convoluted soap opera plots to theories of the origin of the universe. Even if all we knew was that humans possessed such a device, we would expect that it would have to have rather special and unusual properties suited to the task of mapping complex propositional structures onto a serial channel, and an examination of grammar confirms this expectation.

Third, arguments that language is designed for the communication of propositional structures are far from logical truths. It is easy to formulate, and reject, specific alternatives. For example, it is occasionally suggested that language evolved as a medium of internal knowledge representation for use in the computations underlying reasoning. But although there may be a languagelike representational medium – "the language of thought," or "mentalese" (Fodor 1975) – it clearly cannot be English, Japanese, and so on. Natural languages are hopeless for this function: They are needlessly serial, rife with ambiguity (usually harmless in conversational contexts, but unsuited for long-term knowledge representation), complicated by alternations that are relevant only to discourse (e.g., topicalization), and cluttered with devices (such as phonology and much of morphology) that make no contribution to reasoning. Similarly, the facts of grammar make it difficult to argue that language shows design for "the expression of thought" in any sense that is substantially distinct from "communication." If "expression" refers to the mere externalization of thoughts, in some kind of monologue or soliloquy, it is an unexplained fact that language contains mechanisms that presuppose the existence of a listener, such as rules of phonology and phonetics (which map sentences onto sound patterns, enhance confusable phonetic distinctions, disambiguate phrase structure with intonation, and so on.) and pragmatic devices that encode conversational topic, illocutionary force, discourse antecedents, and so on. Furthermore, people do not express their thoughts in an arbitrary private language (which would be sufficient for pure "expression"), but have complex learning mechanisms that acquire a language highly similar in almost every detail to those of other speakers in the community.

Another example of the empirical nature of specific arguments for language design appears when we examine the specific expressive abilities that are designed into language. They turn out to constitute a well-defined set, and do not simply correspond to every kind of information that humans are interested in communicating. So although we may have some a priori intuitions regarding useful expressive capacities of grammar, the matter is ultimately empirical (see, e.g., Jackendoff 1983; 1990; Pinker 1989b; Talmy 1983; 1988), and such research yields results that are specific enough to show that not just any intuition is satisfied. Grammar is a notoriously poor medium for conveying subtle patterns of emotion, for example; facial expressions and tones of voice are more informative (Ekman & Friesen 1975; Etcoff 1986). Although grammars provide devices for conveying rough topological information such as connectivity, contact, and containment, and coarse metric contrasts such as near/far or flat/globular, they are of very little help in conveying precise Euclidean relations: A picture is worth a thousand words. Furthermore, human grammar clearly lacks devices specifically dedicated to expressing any of the kinds of messages that characterize the vocal communication systems of cetaceans, birds, or nonhuman primates, such as announcements of individual identity, predator warnings, and claims of territory.

Finally, Williams (1966) suggests that convergent evolution, resemblance to man-made artifacts, and direct assessments of engineering efficiency are good sources of evidence for adaptation. Of course, in the case of human language, these tests are difficult in practice: Significant convergent evolution has not occurred; no one has ever invented a system that duplicates its function (except for systems that are obviously parasitic on natural languages such as Esperanto or signed English), and most forms of experimental intervention would be unethical. Nonetheless, some tests are possible in principle, and this is enough to refute reflexive accusations of circularity.

For example, even the artificial languages that are focussed on very narrow domains of content and that are not meant to be used in a natural on-line manner by people, such as computer languages or symbolic logic, show certain obvious parallels with aspects of human grammar. They have needed means of distinguishing types of symbols, predicate argument relations, embedding, scope, quantification, and truth relations, and they solve these problems with formal syntactic systems that specify arbitrary patterns of hierarchical concatenation, relative linear order, fixed positions within strings, and closed classes of privileged symbols. Of course, there are vast dissimilarities, but the mere fact that terms like "language," "syntax," "predicate," "argument," and "statement" have clear meanings when applied to artificial systems, with no confusion or qualification, suggests that there are nonaccidental parallels that are reminiscent of the talk of diaphragms and lenses when applied to cameras and eyes. As for experimental investigation, in principle one could define sets of artificial grammars with and without one of the mechanisms in question, or with variations of it. The grammars would be provided or taught to pairs of communicators – formal automata, computer simulations, or college sophomores acting in conscious problem-solving mode – who would be required to convey specific messages under different conditions of speed, noise, or memory limitations. The proportion of information successfully communicated would be assessed and examined as a function of the presence and version of the grammatical mechanism, and of the different conditions putatively relevant to the function in question.

3.3. Language design and language diversity

A more serious challenge to the claim that grammars show evidence of good design may come from the diversity of human languages (Maratsos 1988). Grammatical devices and expressive functions do not pair up in one-to-one fashion. For example, some languages use word order to convey who did what to whom; others use case or agreement for this purpose and reserve the use of word order to distinguish topic from comment, or do not systematically exploit word order at all. How can one say that the mental devices governing word order evolved under selection pressure for expressing grammatical relations if many languages do not use them for that purpose? Linguistic diversity would seem to imply that grammatical devices are very general-purpose tools. And a general-purpose tool would surely have a very generalized structure, and thus could be a spandrel rather than an adapted machine. We begin by answering the immediate objection that the existence of diversity, for whatever reason, invalidates arguments for universal language design; at the end of the section we offer some speculations as to why there should be more than one language to begin with.

First of all, the evolution of structures that serve not one but a small number of definite functions, perhaps to different extents in different environments, is common in biology (Mayr 1982). Indeed, though grammatical devices are put to different uses in different languages, the possible pairings are very circumscribed. No language uses noun affixes to express tense or elements with the syntactic privileges of auxiliaries to express the shape of the direct object. Such universal constraints on structure and function are abundantly documented in surveys of the languages of the world (e.g., Bybee 1985; Comrie 1981; Greenberg 1966b; Greenberg et al. 1978; Hawkins 1988; Keenan 1976; and Shopen 1985). Moreover, language universals are visible in language history, where changes tend to fall into a restricted set of patterns, many involving the introduction of grammatical devices obeying characteristic constraints (Kiparsky 1976; Wang 1976).[3]

But accounting for the evolution of a language faculty permitting restricted variation is only important on the most pessimistic of views. Even a rudimentary grammatical analysis reveals that surface diversity is often a manifestation of minor differences in the underlying mental grammars. Consider some of the supposedly radical typological differences between English and other languages. English is a rigid word-order language; in the Australian language Warlpiri the words from different logical units can be thoroughly scrambled and case markers are used to convey grammatical relations and noun modification. Many Native American languages, such as Cherokee, use few noun phrases within clauses at all, and express grammatical relations by sticking strings of agreement affixes onto the verb, each identifying an argument by a set of features such as humanness or shape. Whereas "accusative" languages like English collapse subjects of transitive and intransitive sentences, "ergative" languages collapse objects of transitives with subjects of intransitives. Whereas English sentences are built around obligatory subjects, languages like Chinese are oriented around a position reserved for the discourse topic.

However, these variations almost certainly correspond to differences in the extent to which the same specific set of mental devices is put to use, but not to differences in the kinds of devices that are put to use. English has free constituent order in strings of prepositional phrases (*The package was sent from Chicago to Boston by Mary; The package was sent by Mary to Boston from Chicago*, and so on). English has case, both in pronouns and in the genitive marker spelled *'s*. It expresses information about arguments in verb affixes in the agreement marker *-s*. Ergativity can be seen in verb alternations like *John broke the glass* and *The glass broke*. There is even a kind of topic position: *As for fish, I like salmon*. Conversely, Warlpiri is not without phrasal syntax. Auxiliaries go in second position (not unlike English, German, and many other languages). The constituents of a noun phrase must be contiguous if they are not case-marked; the constituents of a finite clause must be contiguous if the sentence contains more than one. Pinker (1984) outlines a theory of language acquisition in which the same innate learning mechanisms are put to use in different extents in children acquiring "radically" different languages.

When one looks at more abstract linguistic analyses, the underlying unity of natural languages is even more apparent. Chomsky has quipped that anything you find in one language can also be found in every other language, perhaps at a more abstract level of representation, and this claim can be justified without resorting to Procrustean measures. In many versions of Chomsky's government-binding theory (1981), all noun phrases must be case marked; even those that receive no overt case-marking are assigned "abstract" case by an adjacent verb, preposition, or tense element. The basic order of major phrases is determined by the value of a language-varying parameter specifying the direction in which case assignment may be executed. So in a language like Latin, the noun phrases are marked with morphological case (and can appear in any position), whereas in a language like English, they are not so marked, and must be adjacent to a case-assigner such as a verb. Thus, overt case-marking in one language and word order in another are unified as manifestations of a single grammatical module. And the module has a well-specified function: In the terminology of the theory, it makes noun phrases "visible" for the assignment of thematic roles such as agent, goal, or location. Moreover, word order itself is not a unified phenomenon. Often, when languages "use word order for pragmatic purposes," they are exploiting an underlying grammatical subsystem, such as stylistic rules, that has properties very different from those governing the relative order of noun phrases and their case-assigners.

Why is there more than one language at all? Here we can only offer the most tentative of speculations. For sound-meaning pairings within the lexicon, there are two considerations. First, one might suppose that speakers need a learning mechanism for labels for cultural innovations such as *screwdriver*. Such a learning device is then sufficient for all vocabulary items. Second, it may be difficult to evolve a huge innate code. Each of tens of thousands of sound-meaning correspondences would have to be synchronized across speakers, but few words could have the nonarbitrary antecedents that would have been needed to get the standardization process started (i.e., analogous to the way bared fangs in preparation for biting evolved into the facial expression for anger). Furthermore, the size of such a code would tax the time available to evolve and maintain it in the genome in the face of random perturbations from sexual recombination and other stochastic genetic processes (Tooby & Cosmides, in press a; Williams 1966). Once a mechanism for learning sound-meaning pairs is in place, the information for acquiring any particular pair, such as *dog* for dogs, is readily available from the speech of the community. Thus the genome can store the vocabulary in the environment, as Tooby and Cosmides (1989) have put it.

For other aspects of grammar, one might get more insight by inverting the perspective. Instead of positing that there are multiple languages, leading to the evolution of a mechanism to learn the differences among them, one might posit that there is a learning mechanism, leading to the development of multiple languages. That is, some aspects of grammar might be easily learnable from environmental inputs by cognitive processes that may have been in existence prior to the evolution of grammar, for example, the relative order of a pair of sequenced elements within a bounded unit. For these aspects there was no need to evolve a fixed value, and they are free to vary across communities of speakers. In section 5.2.3, we discuss a simulation of evolution by Hinton and Nowlan (1987) that behaves in a way that is consistent with this conjecture.

3.4. Language design and arbitrariness

Piattelli-Palmarini (1989) presents a different kind of argument: Grammar is not completely *predictable* as an adaptation to communication, therefore it lacks design and did not evolve by selection. He writes, "Survival criteria, the need to communicate and plan concerted action, cannot account for our *specific* linguistic nature. Adaptation cannot even begin to explain any of these phenomena." Frequently cited examples of arbitrary phenomena in language include constraints on movement (such as subjacency), irregular morphology, and lexical differences in predicate-argument structure. For instance, it is acceptable to say *Who did John see Mary with?*, but not *Who did John see Mary and?; John broke* *the glass* but not *John breaked the glass; John filled the glass with milk,* but not *John poured the glass with milk.* The arguments that language could not be an adaptation take two forms: (i) language could be better than what it is, and (ii) language could be different from what it is. We show that neither form of the argument is valid, and that the facts that it invokes are perfectly consistent with language being an adaptation and offer not the slightest support to any specific alternative.

3.4.1. Inherent tradeoffs. In their crudest form, arguments about the putative functionlessness of grammar run as follows: "I bet you can't tell me a function for Constraint *X;* therefore, language is a spandrel." But even if it could be shown that one part of language had no function, that would not mean that all parts of language had no function. Recall from section 2.2 that many organs contain modified spandrels, but this does not mean that natural selection did not assemble or shape the organ. Worse, Constraint *X* may not be a genuine part of the language faculty but just a description of one aspect of it, an epiphenomenal spandrel. No adaptive organ can be adaptive in every aspect, because there are as many aspects of an organ as there are ways of describing it. The recent history of linguistics provides numerous examples where a newly discovered constraint is first proposed as an explicit statement listed as part of a grammar, but is then shown to be a deductive consequence of a much wider ranging principle (see, e.g., Chomsky 1981; Freidin 1978). For example, the ungrammaticality of sentences like *John to have won is surprising,* once attributed to a filter specifically ruling out [NP-to-VP] sequences, is now seen as a consequence of the Case Filter. Although one might legitimately wonder what good "*[NP-to-VP]" is doing in a grammar, one could hardly dispense with something like the Case Filter.

Because the mere appearance of some nonoptimal feature is inconclusive, we must examine specific explanations for why the feature exists. In the case of the nonselectionist position espoused by Piattelli-Palmarini, there is none: not a hint of how any specific aspect of grammar might be explained, even in principle, as a specific consequence of some developmental process or genetic mechanism or constraint on possible brain structure. The position gains *all* its support from the supposed lack of an adaptive explanation. In fact, we will show that there is such an explanation, well-motivated both in evolutionary theory and in linguistics, so the support disappears.

The idea that natural selection aspires toward perfection has long been discredited in evolutionary theory (Williams 1966). As Maynard Smith (1984, p. 290) has put it, "If there were no constraints on what is possible, the best phenotype would live forever, would be impregnable to predators, would lay eggs at an infinite rate, and so on." Tradeoffs among conflicting adaptive goals are a ubiquitous limitation on optimality in the design of organisms. It may be adaptive for a male bird to advertise his health to females with gaudy plumage or a long tail, but not to the extent that predators are attracted or flight is impossible.

Tradeoffs of utility within language are also unavoidable (Bolinger 1980; Slobin 1977). For example, there is a conflict of interest between speaker and hearer. Speakers want to minimize articulatory effort and hence tend toward brevity and phonological reduction. Hearers want to minimize the effort of understanding and hence desire explicitness and clarity. This conflict of interest is inherent to the communication process and operates at many levels. Editors badger authors into expanding elliptical passages; parsimonious headline writers unwittingly produce *Squad Helps Dog Bite Victim* and *Stud Tires Out.* Similarly, there is a conflict of interest between speaker and learner. A large vocabulary allows for concise and precise expression. But it is only useful if every potential listener has had the opportunity to learn each item. Again, this tradeoff is inherent to communication; one man's jargon term is another's *mot juste.*

Clearly, any shared system of communication is going to have to adopt a code that is a compromise among these demands, and so will appear to be arbitrary from the point of view of any one criterion. There is always a large range of solutions to the combined demands of communication that reach slightly different equilibrium points in this multidimensional space. Slobin (1977) points out that the Serbo-Croatian inflectional system is "a classic Indo-European synthetic muddle," suffixing each noun with a single affix from a paradigm full of irregularity, homophony, and zero-morphemes. As a result, the system is perfected late and with considerable difficulty. In contrast, the Turkish inflectional system is semantically transparent, with strings of clearly demarcated regular suffixes, and is mastered by the age of two. When it comes to production by an adult who has overlearned the system, however, Serbo-Croatian does have an advantage in minimizing the sheer number of syllables that must be articulated. Furthermore, Slobin points out that such tradeoffs can be documented in studies of historical change and borrowing. For example, changes that serve to enhance brevity will proceed until comprehension becomes impaired, at which point new affixes or distinctions are introduced to restore the balance (see also Samuels 1972). A given feature of language may be arbitrary in the sense that there are alternative solutions that are better from the standpoint of some single criterion. But this does not mean that it is good for nothing at all!

Subjacency – the prohibition against dependencies between a gap and its antecedent that spans certain combinations of phrasal nodes – is a classic example of an arbitrary constraint (see Freidin & Quicoli 1989). In English you can say *What does he believe they claimed that I said?* but not the semantically parallel *What does he believe the claim that I said?* One might ask why languages behave this way. Why not allow extraction anywhere, or nowhere? The constraint may exist because parsing sentences with gaps is a notoriously difficult problem and a system that has to be prepared for the possibility of inaudible elements anywhere in the sentence is in danger of bogging down by positing them everywhere. Subjacency has been held to assist parsing because it cuts down on the set of structures that the parser has to keep track of when finding gaps (Berwick & Weinberg 1984). This bonus to listeners is often a hindrance to speakers, who struggle with resumptive pronouns in such clumsy sentences as *That's the guy that you heard the rumor about his wife leaving him.* There is nothing "necessary" about the precise English version of the constraint or about the small sample of alternatives allowed within natural language. But by settling in on a particular subset of the range of possible compromises between the demands of expressiveness and parsability, the evolutionary process may have converged on a satisfactory set of solutions to one problem in language processing.

3.4.2. Parity in communications protocols. The fact that one can conceive of a biological system being different from the way it actually is says nothing about whether it is an adaptation (see Mayr 1983). No one would argue that selection was not the key organizing force in the evolution of the vertebrate eye just because the compound eyes of arthropods are different. Similarly, pointing out that a hypothetical Martian language could do passivization differently is inconclusive. We must ask how well supported specific explanations are.

In the case of features of human language structure that could have been different, again Piattelli-Palmarini presents no explanations at all and relies entirely on the putative inability of natural selection to provide any sort of motivated account. But there does exist such an account: The nature of language makes arbitrariness of grammar itself part of the adaptive solution of effective communication *in principle*.

Any communicative system requires a coding protocol that can be arbitrary as long as it is shared. Liberman and Mattingly (1989) call this the requirement of *parity;* we can illustrate it with the (coincidentally named) "parity" settings in electronic communication protocols. There is nothing particularly logical about setting your printer's serial interface to the "even," as opposed to the "odd," parity setting. Nor is there any motivation to set your computer to odd as opposed to even parity. But there is every reason to set the computer and printer to the *same* parity, whatever it is, because if you don't, they cannot communicate. Indeed, standardization itself is far more important than any other adaptive feature possessed by one party. Many personal computer manufacturers in the 1980s boasted of the superior engineering and design of their product compared to the IBM PC. But when these machines were not IBM-compatible, the consequences are well known.

In the evolution of the language faculty, many "arbitrary" constraints may have been selected simply because they defined parts of a standardized communicative code in the brains of some critical mass of speakers. Piattelli-Palmarini may be right in claiming that there is nothing adaptive about forming yes-no questions by inverting the subject and auxiliary as opposed to reversing the order of words in the sentence. But given that language must do one or the other, it is highly adaptive for each member of a community of speakers to be forced to learn to do it the same way as all the other members. To be sure, some combination of historical accidents, epiphenomena of other cognitive processes, and neurodevelopmental constraints must have played a large role in the breaking of symmetry that was needed to get the fixation process running away in one direction or another. But it still must have been selection that resulted in the convention then becoming innately entrenched.

The requirement of parity operates at all levels of a communications protocol. Within individual languages the utility of arbitrary but shared features is most obvious in the choice of individual words: There is no reason for you to call a dog *dog* rather than *cat* except for the fact that everyone else is doing it, but that is reason enough. Saussure (1959) called this inherent feature of language "*l'arbitraire du signe,*" and Hurford (1989a), using evolutionary game theory, demonstrates the evolutionary stability of such a "Saussurean" strategy whereby each learner uses the same arbitrary signs in production that it uses in comprehension (i.e., that other speakers use in production). More generally, these considerations suggest that a preference for arbitrariness is built into the language acquisition device at two levels. It only hypothesizes rules that fall within the (possibly arbitrary) set defined by universal grammar, and within that set, it tries to choose rules that match those used by the community, whatever they are.

The benefits of a learning mechanism designed to assess and adopt the prevailing parity settings become especially clear when we consider alternatives, such as trying to get each speaker to converge on the same standard by endogenously applying some rationale to predict form from meaning. There are many possible rationales for any form-meaning pairing, and that is exactly the problem – different rationales can impress different speakers, or the same speakers on different occasions, to different degrees. But such differences in cognitive style, personal history, or momentary interests must be set aside if people are to communicate. As mentioned, no grammatical device can simultaneously optimize the demands of speakers and hearers, but it will not do to talk in Serbo-Croatian and demand that one's listeners reply in Turkish. Furthermore, whenever cognition is flexible enough to construe a situation in more than one way, no simple correspondence between syntax and semantics can be used predictively by a community of speakers to "deduce" the most "logical" grammatical structure. For example, there is a simple and universal principle dictating that the surface direct object of a causative verb refers to an entity that is "affected" by the action. The principle by itself is unusable, however. When a girl puts boxes in baskets she is literally affecting both: The boxes are changing location and the baskets are changing state from empty to full. One would not want one perceiver interested in the boxes to say that she is *filling boxes* while another interested in the baskets describes the same event as *filling baskets;* no one would know what went where. However, by letting different verbs idiosyncratically select different kinds of entities as "affected" (e.g., *place the box/*basket* versus *fill the basket/*box*), and forcing learners to respect the verbs' wishes, grammar can allow speakers to specify different kinds of entities as affected by putting them in the direct object position of different verbs, with minimal ambiguity. This is presumably why different verbs have different arbitrary syntactic privileges (Pinker 1989b), a phenomenon that Piattelli-Palmarini (1989) describes at length. Even iconicity and onomatopoeia are in the eye and ear of the beholder. The ASL (American Sign Language) sign for "tree" resembles the motion of a tree waving in the wind, but in Chinese Sign Language it is the motion of sketching the trunk (Newport & Meier 1985). In the United States, pigs go "oink"; in Japan, they go "boo-boo."

3.4.3. Arbitrariness and the relation between language evolution and language acquisition. The need for arbitrariness has profound consequences for understanding the role of communicative function in language acquisition and language evolution. Many psychologists and artificial intelligence researchers have suggested that the structure of grammar is simply the solution that every child arrives at for the problem of how to communicate with others. Skinner's reinforcement theory is the strongest version of this hypothesis (Skinner 1957), but versions that avoid his behaviorism and rely instead on general cognitive problem-solving abilities have always been popular within psychology. [See *BBS* special issue on the work of B. F. Skinner *BBS* 7(4)1984.] Both Skinner and cognitive theorists such as Bates et al. (1989) explicitly draw parallels between the role of function in learning and evolution. Chomsky and many other linguists and psycholinguists have argued against functionalism in ontogeny, showing that many aspects of grammar cannot be reduced to being the optimal solution to a communicative problem; rather, human grammar has a universal idiosyncratic logic of its own. More generally, Chomsky has emphasized that people's *use* of language does not tightly serve utilitarian goals of communication but is an autonomous competence to express thought (see, e.g., Chomsky 1975). If communicative function does not shape language in the individual, one might conclude, it probably did not shape language in the species.

We suggest that the analogy that underpins this debate is misleading. It is not just that learning and evolution need not follow identical laws, selectionist or otherwise. (For example, as Chomsky himself has stressed, the issue never even comes up in clearer cases like vision, where nobody suggests that all infants' visual development is related to their desire to see or that visual systems develop with random variations that are selected by virtue of their ability to attain the child's goals.) In the case of language, the arguments of section 3.4.2 suggest that language evolution and language acquisition not only *can* differ but that they *must* differ. Evolution has had a wide variety of equivalent communicative standards to choose from; there is no reason for it to have favored the class of languages that includes Apache and Yiddish, but not Old High Martian or Early Vulcan. This flexibility has been used up, however, by the time a child is born; the species and the language community have already made their choices. Children cannot learn just any useful communicative system; nor can they learn just any natural language. They are stuck with having to learn the particular kind of language the species eventually converged upon and the particular variety the community has chosen. Whatever rationales may have influenced these choices are buried in history and cannot be recapitulated in development.

Moreover, any code as complex and precise as a grammar for a natural language will not wear its protocol on its sleeve. No mortal computer user can induce an entire communications protocol or programming language from examples; that's why we have manuals. This is because any particular instance of the use of such a protocol is a unique event accompanied by a huge set of idiosyncratic circumstances, some relevant to how the code must be used, most irrelevant, and there is no way of deciding which is which. For the child, any sentence or set of sentences is compatible with a wide variety of very different grammars, only one of them correct (Chomsky 1965; 1975; 1980; 1981; Pinker 1979; 1984; Wexler & Culicover 1980). For example, without prior constraints, it would be natural to generalize from input sentences like *Who did you see her with?* to **Who did you see her and?*, from *teethmarks* to **clawsmarks*, from *You better be good* to **Better you be good?* Children have no manual to consult, and presumably that is why they need innate constraints. [See also Lightfoot: "The Child's Trigger Experience: Degree = 0 Learnability" *BBS* 12(2)1989 and Crain: "How Grammars Help Language Learners" *BBS* (forthcoming).]

So we see a reason why functionalist theories of the evolution of language can be true while functionalist theories of the acquisition of language can be false. From the very start of language acquisition, children obey grammatical constraints that afford them no immediate communicative advantage. To take just one example, one- and two-year-olds acquiring English obey a formal constraint on phrase structure configurations concerning the distinction between lexical categories and phrasal categories and, as a result, avoid placing determiners and adjectives before pronouns and proper names. They will use phrases like *big dog* to express the belief that a particular dog is big, but they will never use phrases like *big Fred* or *big he* to express the belief that a particular person is big (Bloom 1990). Children respect this constraint despite the limits it puts on their expressive range.

Furthermore, despite unsupported suggestions to the contrary among developmental psychologists, many strides in language development afford the child no locally discernible increment in communicative ability (Maratsos 1988; 1989). When children say *breaked* and *comed*, they are using a system that is far simpler and more logical than the adult combination of a regular rule and 150 irregular memorized exceptions. Such errors do not reliably elicit parental corrections or other conversational feedback (Brown & Hanlon 1970; Morgan & Travis in press). There is no deficit in comprehensibility; the meaning of *comed* is perfectly clear. In fact, the child's system has greater expressive power that the adult's. When children say *hitted* and *cutted*, they are distinguishing between past and nonpast forms in a manner that is unavailable to adults, who must use *hit* and *cut* across the board. Why do children eventually abandon this simple, logical, expressive system? They must be programmed so that the mere requirement of conformity to the adult code, as subtle and arbitrary as it is, wins over other desiderata.

The requirement that a communicative code have an innate arbitrary foundation ("universal grammar," in the case of humans) may have analogues elsewhere in biology. Mayr (1982, p. 612) notes that

> Behavior that serves as communication, for instance courtship behavior, must be stereotyped in order not to be misunderstood. The genetic program controlling such behavior must be "closed," that is, it must be reasonably resistant to any changes during the individual life cycle. Other behaviors, for instance, those that control the choice of food or habitat, must have a

certain amount of flexibility in order to permit the incorporation of new experiences; such behaviors must be controlled by an "open" program.

In sum, the requirement for standardization of communication protocols dictates that it is better for nature to build a language acquisition device that picks up the code of the ambient language than one that invents a code that is useful from a child's eye view. Acquiring such a code from examples is no mean feat; many grammatical principles and constraints must accordingly be hardwired into the device. Hence, even if the functions of grammatical devices play an important role in evolution, they may play no role in acquisition.

4. Arguments for language being a spandrel

Given that the criteria for being an adaptation appear to be satisfied in the case of language, we can examine the strength of the competing explanation that language is a spandrel, as suggested by Gould, Chomsky, and Piattelli-Palmarini.

4.1. The mind as a multipurpose learning device

The main motivation for Gould's specific suggestion that language is a spandrel is his frequently stated position that the mind is a single general-purpose computer. For example, as part of a critique of a particular theory of the origin of language (Parker & Gibson 1979), Gould writes (1979, p. 386):

> I don't doubt for a moment that the brain's enlargement in human evolution had an adaptive basis mediated by selection. But I would be more than mildly surprised if many of the specific things it now can do are the product of direct selection "for" that particular behavior. Once you build a complex machine, it can perform so many unanticipated tasks. Build a computer "for" processing monthly checks at the plant, and it can also perform factor analyses on human skeletal measures, play Rogerian analyst, and whip anyone's ass (or at least tie them perpetually) in tic-tac-toe.

The analogy is somewhat misleading. It is just not true that you can take a computer that processes monthly checks and use it to play Rogerian analyst; someone has to reprogram it first. Language learning is not programming: Parents provide their children with sentences of English, not rules of English. We suggest that natural selection was the programmer.

The analogy could be modified by imagining some machine equipped with a single program that can *learn from examples* to calculate monthly checks, perform factor analyses, and play Rogerian analyst, all without explicit programming. Such a device does not now exist in artificial intelligence and it is unlikely to exist in biological intelligence. There is no psychologically realistic multipurpose learning program that can acquire language as a special case, because the kinds of generalizations that must be made to acquire a grammar are at cross-purposes with those that are useful in acquiring other systems of knowledge from examples (Chomsky 1972; 1975; 1980a; 1986; Pinker 1979; 1984; Wexler & Culicover 1980). The gross facts about the dissociability of language and other learned cultural systems, listed in the first paragraph of this paper, also belie the suggestion that language is a spandrel of any general cognitive learning ability.

4.2. Constraints on possible forms

The theory that the mind is an all-purpose learning device is of course anathema to Chomsky (and to Piattelli-Palmarini), making it a puzzle that they should find themselves in general agreement with Gould. Recently, Gould (1989a) has described some common ground. Chomsky, he suggests, is in the Continental tradition of trying to explain evolution by structural laws constraining possible organic forms. For example, Chomsky writes:

> In studying the evolution of mind, we cannot guess to what extent there are physically possible alternatives to, say, transformational generative grammar, for an organism meeting certain other physical conditions characteristic of humans. Conceivably, there are none – or very few – in which case talk about evolution of the language capacity is beside the point. (1972, pp. 97–98).

> These skills [e.g., learning a grammar] may well have arisen as a concomitant of structural properties of the brain that developed for other reasons. Suppose that there was selection for bigger brains, more cortical surface, hemispheric specialization for analytic processing, or many other structural properties that can be imagined. The brain that evolved might well have all sorts of special properties that are not individually selected; there would be no miracle in this, but only the normal workings of evolution. We have no idea, at present, how physical laws apply when 10^{10} neurons are placed in an object the size of a basketball, under the special conditions that arose during human evolution. (1982b, p. 321)

> In this regard [the evolution of infinite digital systems], speculations about natural selection are no more plausible than many others; perhaps these are simply emergent physical properties of a brain that reaches a certain level of complexity under the specific conditions of human evolution. (1988b; p. 22 in ms.)

Although Chomsky does not literally argue for any specific evolutionary hypothesis, he repeatedly urges us to consider "physical laws" as possible alternatives to natural selection. But it is not easy to see exactly what we should be considering. It is certainly true that natural selection cannot explain all aspects of the evolution of language. But is there any reason to believe that there are as yet undiscovered theorems of physics that can account for the intricate design of natural language? Of course, human brains *obey* the laws of physics, and always did, but that does not mean that their specific structure can be *explained* by such laws.

More plausibly, we might look to constraints on the possible neural basis for language and its epigenetic growth. But neural tissue is wired up by developmental processes that act in similar ways all over the cortex and, to a lesser degree, across the animal kingdom (Dodd & Jessell 1988; Harrelson & Goodman 1988). In different organisms it has evolved the ability to perform the computations necessary for pollen-source communication, celestial navigation, Doppler-shift echolocation, stereopsis, controlled flight, dam-building, sound mimicry, and face recognition. The space of physically possible neural systems thus can't be all *that* small, as far as specific computational abilities are concerned. And it is most unlikely that laws acting at the level of substrate adhesion molecules and synaptic competition, when their effects are projected upward through many levels of scale and hierarchical organization, would automatically result in systems that accomplish interesting engineering tasks in a world of medium-sized objects.

Changes in brain quantity could lead to changes in brain quality. But mere largeness of brain is neither a necessary nor a sufficient condition for language, as Lenneberg's (1967) studies of nanencephaly and craniometric studies of individual variation have shown. Nor is there reason to think that if you simply pile more and more neurons into a circuit or more and more circuits into a brain that computationally interesting abilities would just emerge. It seems more likely that you would end up with a very big random pattern generator. Neural network modeling efforts have suggested that complex computational abilities require extrinsically imposed design or numerous richly structured inputs during learning or both, any of which would be inconsistent with Chomsky's suggestions.

Finally, there may be direct evidence against the speculation that language is a necessary physical consequence of how human brains can grow. Gopnik (1990; in press) describes a syndrome of developmental dysphasia whose sufferers lack control of morphological features such as number, gender, tense, and case. Otherwise, they are intellectually normal. One 10-year-old boy with this disability earned the top grade in his mathematics class and is a respectable computer programmer. This shows that a human brain lacking components of grammar, perhaps even a brain with the capacity of discrete infinity, is physically and neurodevelopmentally possible.

In sum, there is no support for the hypothesis that language emerges from physical laws acting in unknown ways in a large brain. Although there are no doubt *aspects* of the system that can only be explained by historical, developmental, or random processes, the most likely explanation for the complex structure of the language faculty is that it is a design imposed on neural circuitry as a response to evolutionary pressures.

5. The process of language evolution

For universal grammar to have evolved by Darwinian natural selection it is not enough that it be useful in some general sense. There must have been genetic variation among individuals in their grammatical competence. There must have been a series of steps leading from no language at all to language as we now find it, each step small enough to have been produced by a random mutation or recombination, and each intermediate grammar useful to its possessor. Every detail of grammatical competence that we wish to ascribe to selection must have conferred a reproductive advantage on its speakers, and this advantage must be large enough to have become fixed in the ancestral population. And there must be enough evolutionary time and genomic space separating our species from nonlinguistic primate ancestors.

There are no conclusive data on any of these issues. This, however, has not prevented various people from claiming that each of the necessary postulates is false. We argue that what we do know from the biology of language and evolution makes each of the postulates quite plausible.

5.1. Genetic variation

Lieberman (1984; 1989) claims that the Chomskyan universal grammar could not have evolved. He writes:

> The premises that underlie current "nativist" linguistic theory . . . are out of touch with modern biology. Ernst Mayr (1982), in his definitive work, *The Growth of Biological Thought*, discusses these basic principles that must structure any biologically meaningful nativist theory. . . . [one of the principles is:] Essentialistic thinking (e.g., characterizing human linguistic ability in terms of a uniform hypothetical universal grammar) is inappropriate for describing the biological endowment of living organisms. (1989, pp. 203–5)
>
> A true nativist theory must accommodate genetic variation. A detailed genetically transmitted universal grammar that is identical for every human on the planet is outside the range of biological plausibility. (1989, 223)

This is part of Lieberman's argument that syntax is acquired by general-purpose learning abilities, not by a dedicated module or set of modules. But the passages quoted above contain a variety of misunderstandings and distortions. Chomskyan linguistics is the antithesis of the kind of essentialism that Mayr decries. It treats such disembodied interindividual entities as "The English Language" as unreal epiphenomena. The only scientifically genuine entities are individual grammars situated in the heads of individual speakers (see Chomsky 1986 for extended discussion). True, grammars for particular languages, and universal grammar, are often provisionally idealized as a single kind of system. But this is commonplace in systems-level physiology and anatomy; for example, the structure of the human eye is always described as if all individuals shared it and individual variation and pathology are discussed as deviations from a norm. This is because natural selection, while feeding on variation, uses it up (Ridley 1986; Sober 1984). In adaptively complex structures in particular, the variation we see does not consist of qualitative differences in basic design, and this surely applies to complex mental structures as well (Tooby & Cosmides, in press).

Also, contrary to what Lieberman implies, there does exist variation in grammatical ability. Within the range that we would call "normal," we all know some individuals who habitually use tangled syntax and others who speak with elegance, some who are linguistically creative and others who lean on cliches, some who are fastidious conformists and others who bend and stretch the language in various ways. At least some of this variation is probably related to the strength or accessibility of different grammatical subsystems, and at least some, we suspect, is genetic, the kind of thing that would be shared by identical twins reared apart. For example, Bever et al. (1989) have extensive experimental data showing that right-handers with a family history of left-handedness show less reliance on syntactic analysis and more reliance on lexical association than do people without such a genetic background.

Moreover, beyond the "normal" range there are documented genetically transmitted syndromes of grammatical deficits. Lenneberg (1967) notes that specific language disability is a dominant, partially sex-linked trait with almost complete penetrance (see also Ludlow & Cooper, 1983, for a literature review). More strikingly, Gopnik (1990) has found a familial selective deficit in the use of morphological features (gender, number, tense, etc.) that acts as if it is controlled by a dominant gene.

This does not mean that we should easily find cases of inherited subjacency deficiency or anaphor blindness. Pleiotropy – single gene changes that cause apparently unrelated phenotypic effects – is ubiquitous, so there is no reason to think that every aspect of grammar that has a genetic basis must be controlled by a single gene. Having a right hand has a genetic basis but genetic deficits do not lead to babies being born with exactly one hand missing. Moreover, even if there was a pure lack of some grammatical device among some people, it may not be easily discovered without intensive analysis of the person's perceptions of carefully constructed linguistic examples. Different grammatical subsystems can generate superficially similar constructions, and a hypothetical victim of a deficit may compensate in ways that would be difficult to detect. Indeed, cases of divergent underlying analyses of a single construction are frequent causes of historical change.

5.2. Intermediate steps

Some people have doubted that an evolutionary sequence of increasingly complex and specialized universal grammars is possible. The intermediate links, it has been suggested, would not have been viable communication systems. These arguments fall into three classes.

5.2.1. Nonshared innovations. Geschwind (1980), among others, has wondered how a hypothetical "beneficial" grammatical mutation could really have benefited its possessor, given that such an individual would not have been understood by less evolved compatriots. One possible answer is that any such mutation is likely to be shared by individuals who are genetically related. Because much communication is among kin, a linguistic mutant will be understood by some relatives and the resulting enhancements in information sharing will benefit each one of them relative to others who are not related.

We think there is a more general answer, however. Comprehension abilities do not have to be in perfect synchrony with production abilities. Comprehension can use cognitive heuristics based on probable events to decode word sequences even in the absence of grammatical knowledge. Ungrammatical strings like *skid crash hospital* are quite understandable, and we find we can do a reasonably good job understanding Italian newspaper stories based on a few cognates and general expectancies. At the same time, grammatical sophistication in such sources does not go unappreciated. We are unable to duplicate Shakespeare's complex early Modern English but we can appreciate the subtleties of his expressions. When some individuals are making important distinctions that can be decoded with cognitive effort, it could set up a pressure for the evolution of neural mechanisms that would make this decoding process become increasingly automatic, unconscious, and undistracted by irrelevant aspects of world knowledge. These are some of the hallmarks of an innate grammatical "module" (Fodor 1983). The process whereby environmentally induced responses set up selection pressures for such responses to become innate, triggering conventional Darwinian evolution that superficially mimics a Lamarckian sequence, is sometimes known as the "Baldwin effect" (Baldwin 1896; Hinton & Nowlan 1987; Maynard Smith 1987).

Not all linguistic innovations need begin with a genetic change in the linguistic abilities of speakers. Former Secretary of State Alexander Haig achieved notoriety with such expressions as *Let me caveat that* or *That statement has to be properly nuanced.* As listeners, we cringe at the ungrammaticality, but we have no trouble understanding him and would be hard-pressed to come up with a concise grammatical alternative. The double standard exemplified by Haigspeak is fairly common in speech (Pinker 1989b). Most likely this was always true, and innovations driven by cognitive processes exploiting analogy, metaphor, iconicity, conscious folk etymology, and so on, if useful enough, could set up pressures for both speakers and hearers to grammaticize those innovations. Note also that if a single mental database is used in production and comprehension (Bresnan & Kaplan 1982) evolutionary changes in response to pressure on one performance would automatically transfer to the other.

5.2.2. Categorical rules. Many linguistic rules are categorical, all-or-none operations on symbols (see, e.g., Pinker & Prince 1988; 1989). How could such structures evolve in a gradual sequence? Bates et al. (1989), presumably echoing Gould's "5% of an eye" (1989a), write:

> What protoform can we possibly envision that could have given birth to constraints on the extraction of noun phrases from an embedded clause? What could it conceivably mean for an organism to possess half a symbol, or three quarters of a rule? (p. 3) . . . monadic symbols, absolute rules and modular systems must be acquired as a whole, on a yes-or-no basis – a process that cries out for a Creationist explanation." (p. 30)

Two issues are being collapsed here, however. Although one might justifiably argue that an entire system of grammar must evolve in a gradual continuous sequence, that does not mean that every aspect of every rule must evolve in a gradual continuous sequence. As mentioned, mutant fruit flies can have a full leg growing where an antenna should be and the evolution of new taxa with different numbers of appendages from their ancestors is often attributed to such homeotic mutations. No single mutation or recombination could have led to an entire universal grammar, but it could have led a parent with an n-rule grammar to have an offspring with an $n + 1$ rule grammar, or a parent with an m-symbol rule to have an offspring with an $m + 1$ symbol rule. It could also lead to a parent with no grammatical rules at all and just rote associations to have an offspring with a single rule. Grammatical rules are symbol-manipulations whose skeletal form is shared by many other mental systems. Indeed, discrete symbol manipulations, free from graded application based on similarity to memorized cases, are highly useful in many domains of cognition, especially those involving socially shared information (Freyd 1983; Pinker & Prince 1989; Smolensky 1988). If a genetic change caused generic copies of a nonlinguistic symbol-replacement operation to pop up within the neural system underlying communication, such protorules could be put to use as parts of encoding and decoding schemes, whereupon they could be subject to selective forces tailoring them to specific demands of language. Rozin (1976) and Shepard (1986) have argued that the evolution of intelligence was made possible by just such sequences.

5.2.3. Perturbations of formal grammars. Grammars are thought to be complex computational systems with many interacting rules and conditions. Chomsky (1981) has emphasized how grammars have a rich deductive structure in which a minor change to a single principle can have dramatic effects on the language as a whole as its effects cascade through grammatical derivations. This raises the question of how the entire system could be viable under the far more major perturbations that could be expected during evolutionary history. Does grammar degrade gracefully as we extrapolate backward in time? Would a universal grammar with an altered or missing version of some component be good for anything, or would it result in nothing but blocked derivations, filtered constructions, and partial structures? Lieberman (1989, p. 200) claims that "the only model of human evolution that would be consistent with the current standard linguistic theory is a sudden saltation that furnished human beings with the neural bases for language." Similarly, Bates et al. (1989, pp. 2–3) claim that "if the basic structural principles of language cannot be learned (bottom up) or derived (top down), there are only two possible explanations for their existence: Either universal grammar was endowed to us directly by the Creator, or else our species has undergone a mutation of unprecedented magnitude, a cognitive equivalent of the Big Bang."

Such arguments are based on a confusion, however. Although a grammar for an existing language cannot tolerate minor perturbations and still be a grammar for a language that a modern linguist would recognize, that does not mean that it cannot be a grammar at all. To put it crudely, there is no requirement that the languages of *Homo erectus* fall into the class of possible *Homo sapiens* languages. Furthermore, language abilities consist of not just formal grammar but also such nonlinguistic cognitive processes as analogy, rote memory, and Haigspeak. Chomsky (1981) refers to such processes as constituting the "periphery" of grammar, but a better metaphor may put them in the "interstices," where they would function as a kind of jerry-rigging that could allow formally incomplete grammars to be used in generating and comprehending sentences.

The assertion that a natural language grammar either functions as a whole or not at all is surprisingly common. Yet it has no more merit than similar claims about eyes, wings, and webs that frequently pop up in the anti-Darwinian literature (see Dawkins, 1986, for examples), and which occasionally trigger hasty leaps to claims about exaptation. Pidgins, contact languages, Basic English, and the language of children, immigrants, tourists, aphasics, telegrams, and headlines provide ample proof that there is a vast continuum of viable communicative systems displaying a continuous gradation of efficiency and expressive power (see Bickerton 1986). This is exactly what the theory of natural selection requires.

Our suggestions about interactions between learning and innate structure in evolution are supported by an interesting simulation of the Baldwin effect by Hinton and Nowlan (1987). They consider the worst imaginable scenario for evolution by small steps: a neural network with 20 connections (which can be either excitatory or inhibitory) that conveys no fitness advantage unless all 20 are correctly set. So not only is it no good to have 5% of the network, it's no good to have 95%. In a population of organisms whose connections are determined by random mutations, a fitter mutant arises at a rate of only about once every million (2^{20}) genetically distinct organisms, and its advantages are immediately lost if the organism reproduces sexually. But now consider an organism where the connections are either genetically fixed to one or the other value or are settable by learning, determined by random mutation with an average of 10 connections fixed. The organism tries out random settings for the modifiable connections until it hits upon the combination that is advantageous; this is recognizable to the organism and causes it to retain those settings. Having attained that state the organism enjoys a higher rate of reproduction; the sooner it attains it, the greater the benefit. In such a population there *is* an advantage to having less than 100% of the correct network. Among the organisms with, say, 10 innate connections, the one in every thousand (2^{10}) that has the right ones will have some probability of attaining the entire network; in a thousand learning trials, this probability is fairly high. For the offspring of that organism, there are increasing advantages to having more and more of the correct connections innately determined, because with more correct connections to begin with, it takes less time to learn the rest, and the chances of going through life without having learned them get smaller.

Hinton and Nowlan have confirmed these intuitions in a computer simulation, demonstrating nicely that learning can guide evolution, as the argument in this section requires, by turning a spike in fitness space into a gradient. Moreover, they made an interesting discovery. Though there is always a selection pressure to make learnable connections innate, this pressure diminishes sharply as most of the connections come to be innately set, because it becomes increasingly unlikely that learning will fail for the rest. This is consistent with the speculation that the multiplicity of human languages is in part a consequence of learning mechanisms existing prior to (or at least independent of) the mechanisms specifically dedicated to language. Such learning devices may have been the sections of the ladder that evolution had no need to kick away.

5.3. Reproductive advantages of better grammars

David Premack (1985, pp. 281–82) writes:

> I challenge the reader to reconstruct the scenario that would confer selective fitness on recursiveness. Language evolved, it is conjectured, at a time when humans or protohumans were hunting mastodons. . . . Would it be a great advantage for one of our ancestors squatting alongside the embers, to be able to remark: "Beware of the short beast whose front hoof Bob

cracked when, having forgotten his own spear back at camp, he got in a glancing blow with the dull spear he borrowed from Jack"?

Human language is an embarrassment for evolutionary theory because it is vastly more powerful than one can account for in terms of selective fitness. A semantic language with simple mapping rules, of a kind one might suppose that the chimpanzee would have, appears to confer all the advantages one normally associates with discussions of mastodon hunting or the like. For discussions of that kind, syntactic classes, structure-dependent rules, recursion and the rest, are overly powerful devices, absurdly so.

Premack's rhetorical challenge captures a conviction that many people find compelling, perhaps even self-evident, and it is worth considering why. It is a good example of what Dawkins (1986) calls the Argument from Personal Incredulity. The argument draws on people's poor intuitive grasp of probabilistic processes, especially those that operate over the immensities of time available for evolution. The passage also gains intuitive force because of the widespread stereotype of prehistoric humans as grunting cave men whose main reproductive challenge was running away from tigers or hunting mastodons. The corollary would seem to be that only humans in modern industrial societies – and maybe only academics, it is sometimes implied – need to use sophisticated mental machinery. But compelling as these commonsense intuitions are, they must be resisted.

5.3.1. Effects of small selective advantages. First, one must be reminded of the fact that tiny selective advantages are sufficient for evolutionary change. According to Haldane's (1927) classic calculations, for example, a variant that produces on average 1% more offspring than its alternative allele would increase in frequency from 0.1% to 99.9% of the population in a little more than 4,000 generations. Even in long-lived humans this fits comfortably into the evolutionary timetable. (Needless to say, fixations of different genes can go on in parallel.) Furthermore, the phenotypic effects of a beneficial genetic change need not be observable in any single generation. Stebbins (1982) constructs a mathematical scenario in which a mouselike animal is subject to selection pressure for increased size. The pressure is so small that it cannot be measured by human observers, and the actual increase in size from one generation to the next is also so small that it cannot be measured against the noise of individual variation. Nonetheless, this mouse would evolve to the size of an elephant in 12,000 generations, a slice of time that is geologically "instantaneous." Finally, very small advantages can also play a role in macroevolutionary successions among competing populations of similar organisms. Zubrow (1987) calculates that a 1% difference in mortality rates among geographically overlapping Neanderthal and modern populations could have led to the extinction of the former within 30 generations, or a single millennium.

5.3.2. Grammatical complexity and technology. It has often been pointed out that our species is characterized by two features – technology and social relations among nonkin – that have attained levels of complexity unprecedented in the animal kingdom. Toolmaking is the most widely advertised ability, but the knowledge underlying it is only a part of human technological competence. Modern hunter-gatherers, whose lifestyle is our best source of evidence for that of our ancestors, have a folk biology encompassing knowledge of the life cycles, ecology, and behavior of wild plants and animals "that is detailed and thorough enough to astonish and inform professional botanists and zoologists" (Konner 1982, p. 5). This ability allows the modern !Kung San, for example, to enjoy a nutritionally complete diet with small amounts of effort in what appears to us to be a barren desert. Isaac (1983) interprets fossil remains of home bases as evidence for a lifestyle depending heavily on acquired knowledge of the environment as far back as two million years ago in *Homo habilis*. An oft-noted special feature of humans is that such knowledge can accumulate across generations. Premack (1985) reviews evidence that pedagogy is a universal and species-specific human trait, and the usefulness of language in pedagogy is not something that can be reasonably doubted. As Brandon and Hornstein (1986) emphasize, there is presumably a large selective advantage conferred by being able to learn in a way that is essentially stimulus-free (Williams, 1966, made a similar point.) Children can learn from a parent that a food is poisonous or that a particular animal is dangerous; they do not have to observe or experience this by themselves.

With regard to adult-to-adult pedagogy, Konner (1982, p. 171) notes that the !Kung discuss

> everything from the location of food sources to the behavior of predators to the movements of migratory game. Not only stories, but great stores of knowledge are exchanged around the fire among the !Kung and the dramatizations – perhaps best of all – bear knowledge critical to survival. A way of life that is difficult enough would, without such knowledge, become simply impossible.

Devices designed for communicating precise information about time, space, predicate-argument relations, restrictive modification, and modality are not wasted in such efforts. Recursion, in particular, is extraordinarily useful. Premack repeats a common misconception when he uses tortuous phrases as an exemplification of recursive syntax; without recursion you can't say *the man's hat* or *I think he left*. All you need for recursion is an ability to embed a phrase containing a noun phrase within another noun phrase or a clause within another clause, which falls out of pairs of rules as simple as NP→ det N PP and PP→ P NP. Given such a capacity, one can now specify reference to an object to an arbitrarily fine level of precision. These abilities can make a big difference. For example, it makes a big difference whether a far-off region is reached by taking the trail that is in front of the large tree or the trail that the large tree is in front of. It makes a difference whether that region has animals that you can eat or animals that can eat you. It makes a difference whether it has fruit that is ripe or fruit that was ripe or fruit that will be ripe. It makes a difference whether you can get there if you walk for three days or whether you can get there and walk for three days.

5.3.3. Grammatical complexity and social interactions. What is less generally appreciated is how important linguistically supported social interactions are to a hunter-gatherer way of life. Humans everywhere depend on cooperative efforts for survival. Isaac (1983) reviews evidence that a lifestyle depending on social interactions among nonkin was present in *Homo habilis* more than two million years ago. Language in particular would seem to be deeply woven into such interactions, in a manner that is not qualitatively different from that of our own "advanced" culture. Konner (1982) writes:

> War is unknown. Conflicts within the group are resolved by talking, sometimes half or all the night, for nights, weeks on end. After two years with the San, I came to think of the Pleistocene epoch of human history (the three million years during which we evolved) as one interminable marathon encounter group. When we slept in a grass hut in one of their villages, there were many nights when its flimsy walls leaked charged exchanges from the circle around the fire, frank expressions of feeling and contention beginning when the dusk fires were lit and running on until the dawn. (p. 7)
>
> If what lawyers and judges do is work, then when the !Kung sit up all night at a meeting discussing a hotly contested divorce, that is also work. If what psychotherapists and ministers do is work, then when a !Kung man or woman spends hours in an enervating trance trying to cure people, that is also work (p. 371).

Reliance on such exchanges puts a premium on the ability to convey such socially relevant abstract information as time, possession, beliefs, desires, tendencies, obligations, truth, probability, hypotheticals, and counterfactuals. Once again, recursion is far from being an "overly powerful device." The capacity to embed propositions within other propositions, as in [$_S$*He thinks that S*] *or* [$_S$*She said that* [$_S$*he thinks that* S]], is essential to the expression of beliefs about the intentional states of others. [Cf. Premack: "Does the Chimpanzee Have a Theory of Mind" *BBS* 1(4)1978.]

Furthermore, in a group of communicators competing for attention and sympathies there is a premium on the ability to engage, interest, and persuade listeners. This in turn encourages the development of discourse and rhetorical skills and the pragmatically relevant grammatical devices that support them. Symons's (1979) observation that tribal chiefs are often both gifted orators and highly polygynous is a splendid prod to any imagination that cannot conceive of how linguistic skills could make a Darwinian difference.

5.3.4. Social use of language and evolutionary acceleration. The social value of complex language probably played a profound role in human evolution that is best appreciated by examining the dynamics of cooperative interactions among individuals. [See: Caporael et al.: "Selfishness Re-examined" *BBS* 12(4)1989.] As mentioned, humans, probably early on, fell into a lifestyle that depended on extended cooperation for food, safety, nurturance, and reproductive opportunities. This lifestyle presents extraordinary opportunities for evolutionary gains and losses. On the one hand, it benefits all participants by surmounting prisoners' dilemmas. On the other hand, it is vulnerable to invasion by cheaters who reap the benefits without paying the costs (Axelrod & Hamilton 1981; Cosmides 1989; Hamilton 1964; Maynard Smith 1974; Trivers 1971). [See also Maynard Smith: "Game Theory and the Evolution of Behavior" *BBS* 7(1)1984.] The minimum cognitive apparatus needed to sustain this lifestyle is memory for individuals and the ability to enforce social contracts of the form "If you take a benefit then you must pay a cost" (Cosmides 1989). This alone puts a demand on the linguistic expression of rather subtle semantic distinctions. It makes a difference whether you understand me as saying that if you give me some of your fruit I will share meat that I will get, or that you should give me some fruit because I shared meat that I got, or that if you don't give me some fruit I will take back the meat that I got.

But this is only a beginning. Cooperation opens the door to advances in the ability of cheaters to fool people into believing that they have paid a cost or that they have not taken a benefit. This in turn puts pressure on the ability to detect subtle signs of such cheating, which puts pressure on the ability to cheat in less detectable ways, and so on. It has been noted that this sets the stage for a cognitive "arms race" (e.g., Cosmides & Tooby 1989; Dawkins 1976; Tooby & DeVore 1987; Trivers 1971). Elsewhere in evolution such competitive feedback loops, such as in the struggle between cheetahs and gazelles, have led to the rapid evolution of spectacular structures and abilities (Dawkins 1982). The unusually rapid enlargement of the human brain, especially the frontal lobes, has been attributed to such an arms race (Alexander 1987; Rose 1980). After all, it doesn't take all that much brain power to master the ins and outs of a rock or to get the better of a berry. But interacting with an organism of approximately equal mental abilities whose motives are at times outright malevolent makes formidable and ever-escalating demands on cognition. This competition is not reserved for obvious adversaries. Partial conflicts of reproductive interest between male and female, sibling and sibling, and parent and offspring are inherent to the human condition (Symons 1979; Tooby & DeVore 1987; Trivers 1974).

It should not take much imagination to appreciate the role of language in a cognitive arms race. In all cultures, human interactions are mediated by attempts at persuasion and argument. How a choice is framed plays a huge role in determining which alternative people choose (Tversky & Kahneman 1981). The ability to frame an offer so that it appears to present maximal benefit and minimum cost to the buyer, and the ability to see through such attempts and to formulate persuasive counterproposals, would have been a skill of inestimable value in primitive negotiations, as it is today. So is the ability to learn of other people's desires and obligations through gossip, an apparently universal human vice (Cosmides & Tooby 1989; Symons 1979).

In sum, primitive humans lived in a world in which language was woven into the intrigues of politics, economics, technology, family, sex, and friendship and that played key roles in individual reproductive success. They could no more live with a Me-Tarzan-you-Jane level of grammar than we could.

5.4. *Phyletic continuity*

Bates et al. (1989), Greenfield (1987), and Lieberman (1976; 1984) argue that if language evolved in humans by natural selection, it must have antecedents in closely related species such as chimpanzees, which share 99% of their genetic material with us and may have diverged from a common ancestor as recently as 5 to 7 million years ago (King & Wilson 1975; Miyamoto et al. 1987). Similarly, because no biological ability can evolve out of nothing, they claim, we should find evidence of nonlinguistic abilities in humans that are continuous with grammar. Lieberman claims that motor programs are preadaptations for syntactic rules whereas Bates (1976) and Greenfield (Greenfield & Smith 1976) suggest that communicative gestures flow into linguistic naming. As Bates et al. (1989, p. 8) put it, "we have to abandon any strong version of the discontinuity claim that has characterized generative grammar for thirty years. We have to find some way to ground symbols and syntax in the mental material that we share with other species."

The specific empirical claims have been disputed. Seidenberg and Petitto (Seidenberg 1986; Seidenberg & Petitto 1979; 1987) have reviewed the evidence of the signing abilities of apes and concluded that they show no significant resemblance to human language or to the process of acquiring it. In a study of the acquisition of sign language in deaf children, Petitto (1987) argues that nonlinguistic gestures and true linguistic names, even when both share the manual-visual channel, are completely dissociable. These conclusions could be fodder for the claim that natural language represents a discontinuity from other primate abilities and so could not have evolved by natural selection.

We find the Seidenberg and Petitto demonstrations convincing, but our argument is not based on whether they are true. Rather we completely disagree with the premise (not theirs) that the debate over ape signing should be treated as a referendum on whether human language evolved by natural selection. Of course human language, like other complex adaptations, could not have evolved overnight. But then there is no law of biology that says that scientists are blessed with the good fortune of being able to find evolutionary antecedents to any modern structure in some other living species. The first recognizably distinct mental system that constituted an antecedent to modern human language may have appeared in a species that diverged from the chimp-human common ancestor, such as *Australopithecus afarensis* or any of the subsequent hominid groups that led to our species. Moreover, chimpanzees themselves are not generalized common ancestors but presumably have done some evolving of their own since the split. We must be prepared for the possible bad news that there just aren't any living creatures with homologues of human language, and let the chimp singing debate come down as it will.

As far as we know, this would still leave plenty of time for language to have evolved: 3.5 to 5 million years, if early Australopithecines were the first talkers, or, as an absolute minimum, several hundred thousand years (Stringer & Andrews 1988), in the unlikely event that early *Homo sapiens* was the first. (For what it's worth, Broca's area is said to be visible in cranial endocasts of two-million-year-old fossil hominids [Falk 1983; Tobias 1981].) There is also no justification in trying to squeeze conclusions out of the genetic data. On the order of 40 million base pairs differ between chimpanzees and humans, and we see no reason to doubt that universal grammar would fit into these 10 megabytes with lots of room left over, especially if provisions for the elementary operations of a symbol-manipulation architecture are specified in the remaining 99% of the genome (see Seidenberg, 1986, for discussion).

In fact, there is even more scope for design differences than the gross quantity of nonshared genetic material suggests. The 1% difference between chimps and humans represents the fraction of base pairs that are different. But genes are long stretches of base pairs, and if even one pair is different, the entire functioning product of that gene could be different. Just as replacing one bit in every byte leads to text that is 100% different, not 12.5% different, it is possible for the differing base pairs to be apportioned so that 100% of the genes of humans and chimps are different in function. Though this extreme possibility is, of course, unlikely, it warns us not to draw any conclusions about phenotypic similarity from degree of genomic overlap.[4]

As for continuity between language and nonlinguistic neural mechanisms, we find it ironic that arguments that are touted as being "biological" do not take even the most elementary steps to distinguish between analogy and homology. Lieberman's claim that syntactic rules must be retooled motor programs, a putative case of preadaptation, is a good example. It may be right, but there is no reason to believe it. Lieberman's evidence is only that motor programs are hierarchically organized and serially ordered, and so is syntax. But hierarchical organization characterizes many neural systems, perhaps any system, living or nonliving, that we would want to call complex (Simon 1969). And an organism that lives in real time is going to need a variety of perceptual, motor, and central mechanisms that keep track of serial order. Hierarchy and seriality are so useful that for all we know they may have evolved many times in neural systems (Bickerton, 1984; 1986, also makes this point). To distinguish true homology from mere analogy it is necessary to find some unique derived nonadaptive character shared by the relevant systems, for example, some quirk of grammar that can be seen in another system. Not only has no such shared character been shown, but the dissimilarities between syntax and motor control are rather striking. Motor control is a game of inches so its control programs must have open continuous parameters for time and space at every level of organization. Syntax has no such analogue parameters. A far better case could be made that grammar exploited mechanisms originally used for the conceptualization of topology and antagonistic forces (Jackendoff 1983; Pinker 1989b; Talmy 1983; 1988), but that is another story.

6. Conclusion

As we warned, the thrust of this target article has been entirely conventional. All we have argued is that human language, like other specialized biological systems, evolved by natural selection. Our conclusion is based on two facts that we would think would be entirely uncontroversial: Language shows signs of complex design for the communication of propositional structures, and the only explanation for the origin of organs with complex design is the process of natural selection. Although distinguished scientists from a wide variety of fields and ideologies have tried to cast doubt on an orthodox Darwinian account of the evolution of a biological specialization for grammar, upon close examination none of the arguments is compelling.

We hope, however, that we have done more than try to set the record straight. Skepticism about the possibility of saying anything of scientific value about language evolution has a long history, beginning in the prohibition against discussing the topic by the Société de Linguistique de Paris in 1866 and culminating in the encyclopedic volume edited by Harnad et al. (1976) that pitted a few daring speculators against an army of doubters. A suspicious attitude is not entirely unwarranted when one reads about The Age of Modifiers, Pithecanthropus Alalus ("Ape-man without speech"), and the Heave-ho theory. But such skepticism should not lead to equally unsupported assertions about the necessity of spandrels and saltations.

A major problem among even the more responsible attempts to speculate about the origins of language has been that they ignore the wealth of specific knowledge about the structure of grammar discovered during the past 30 years. As a result, language competence has been equated with cognitive development, leading to confusions between the evolution of language and the evolution of thought, or has been expediently equated with activities that leave tangible remnants, such as tool manufacture, art, and conquest.

We think there is a wealth of respectable new scientific information relevant to the evolution of language that has never been properly synthesized. The computational theory of mind, generative grammar, articulatory and acoustic phonetics, developmental psycholinguistics, and the study of dynamics of diachronic change could profitably be combined with recent molecular, archeological, and comparative neuroanatomical discoveries and with strategic modeling of evolution using insights from evolutionary theory and anthropology (see, e.g., Barkow et al. in press; Bickerton 1981; Brandon & Hornstein 1986; Hinton & Nowlan 1987; Hurford 1989a; 1989b; Tooby & DeVore 1987). It is certain that there are many questions about the evolution of language that we will never answer, but we are optimistic that there are insights to be gained, if only the problems are properly posed.

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NOTES

1. For example, he says that "Language must surely confer enormous selective advantages" (Chomsky 1980a, p. 239; see also Chomsky 1975, p. 252), and argues that,

> . . . suppose that someone proposes a principle which says: The form of a language is such-and-such because having that form permits a function to be fulfilled – a proposal of this sort would be appropriate at the level of evolution (of the species, or of language), not at the level of acquisition of language by an individual (Chomsky 1977, pp. 86–87).

2. Dennett (1983), it is interesting to note, argues that Gould and Lewontin's critique is remarkably similar in logic to critiques of another large-scale theory, the representational theory of mind in cognitive science, by behaviorists. Dennett sees common flaws in the critiques: Both fail to account for cases of adaptive complexity that are not direct consequences of any law of physics, and both apply the criterion of falsifiability in too literal-minded a way.

3. Note also that historical change in languages occurs very rapidly by biological standards. Wang (1976) points out, for example, that one cycle of the process whereby a language alternates between reliance on word order and reliance on affixation typically takes a thousand years. A hominid population evolving language could be exposed to the full range of linguistic diversity during a single tick of the evolutionary clock, even if no single generation was faced with all humanly possible structures.

4. We thank John Tooby for pointing this out to us.

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Welcome to functionalism

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Torn between their attachment to the Chomskyan view of a set of autonomous linguistic modules and their appreciation of the central role of natural selection, Pinker & Bloom (P&B) have opted to bring their earlier linguistic persuasions (Pinker 1984) into accord with the biological facts. In doing so, they have moved, far more than they realize, into the camp of the linguistic functionalists where they are indeed welcome.

Bates and MacWhinney (1989) laid out four levels of the functionalist position, ordered by the amount and type of evidence that is necessary to make each claim go through. In Level 1 theories, communicative functions play an historical role in determining the range of forms a natural language can take – keeping in mind that the term "function" refers not only to communicative content, but also to the range of information processing constraints that impinge on communication in real time. Level 1 claims can only be substantiated indirectly, using diverse lines of evidence to reconstruct a plausible historical scenario. Level 2 theories go one step further: Communicative functions continue to play a causal role today in constraining the range of languages that a human being can process and acquire. Such claims presuppose Level 1 functionalism, but also require evidence from correlational studies of the range of contexts in which a given form or set of forms is used as well as experimental studies manipulating form/function relations directly. Level 3 theories take yet another step: If correlations between form and function exist in language use by mature native speakers, then children can exploit the same links directly to acquire the mappings developed in earlier generations. Level 3 theories are common in the field of child language, whether or not they are so labeled. In fact, Pinker's (1984) notion of semantic bootstrapping qualifies as a weak Level 3 theory and Bloom (1990) moves toward a stronger Level 3 position. Finally, Level 4 theories, such as Langacker's Cognitive Grammar (1987), presuppose Levels 1 through 3, adding the claim that functional constraints on form should be specified directly in models of grammatical competence.

In this paper, P&B constrain themselves to Level 1 functionalism, accepting an association between language form and language function as a crucial basis for language evolution while explicitly disavowing functionalist claims at the next three levels. Despite the fact that there are Level 3 features in their other published works, they now argue that the causal links between form and function have been "sloughed off" by modern speaker/listeners. To be sure, humans have developed more efficient machinery for language use, and more efficient cultural and genetic means for language transmission. But the radical break from past to present that P&B postulate is unjustifiable. We are reminded of a complaint by New York Gov. Mario Cuomo about the political commitment of anti-abortion forces: "These people apparently believe that life begins at conception and ends at birth." In defense of their position, P&B invoke the notion of conventionality: Modern day language is symbolic, based on arbitrary links between form and meaning. Words do not "resemble" their meanings, and there is little or no iconicity (similarity of form) between grammatical devices and the semantic roles that they indirectly convey. But arbitrariness of form is not equivalent to arbitrariness of function. The word "dog" does not look like a fuzzy four-legged mammal, but the word must still operate under the constraints of retrievability, expressibility, and perceivability in real time human language use. Some kind of symbol had to be available to do the communicative work of talking about dogs. Presumably, the information processing and existential factors that constrain grammar are even more complex. Those of us who are working to uncover these links view the selectional processes operating during evolution and the selectional processes operating during language acquisition as part of one seamless natural fabric, an enormous constraint satisfaction problem with local and long-range competitions that are still being resolved.

Although P&B are clearly on the right track in rejecting the most radical forms of unmotivated nativism, their defense of a weaker form of nativism contains at least two theoretical inconsistencies. First, P&B have ruled out the possibility that infrahuman adaptations have served as part of the biological groundwork for language, rejecting the position embraced by Bates et al. (in press). With this move, P&B are forced to the view that language evolved *de novo,* in a relatively short period of time. This artificial truncation of the evolutionary history of language is difficult to square with the biological principles of adaptation, natural selection, or exaptation (i.e., recycling of old forms to serve new functions). Furthermore, it leads P&B to some improbable speculations about the speed with which a complex adaptation like language could have spread throughout the gene pool, and to some unfortunate confusions between genetic transmission (a slow process even if we are dealing with a single genetic trait) and cultural transmission (a rapid process, but one that begs the question of origins – see P&B's Note 3 for an example of such a confusion). In defense of their arguments against infrahuman preparation for language, P&B cite the Seidenberg and Petitto (1987) critique of the chimpanzee language literature, concluding that no infrahuman primate is capable of "true language." Unfortunately, P&B appear to have completely missed the point of the Bates et al. paper, i.e., that "language is a new machine constructed out of old parts." In other words, in the evolution of language there has been a new and unique *reconfiguring* of social skills and cognitive mechanisms that originally evolved in the service of other functions. An infrahuman primate may have all or most of these prerequisite skills in place, but to an insufficient degree or in a configuration that is simply inadequate for "true language" to emerge. The Bates et al. view is more compatible with evolutionary theory, and provides a more realistic time frame for the evolution of language. So why should P&B reject it out of hand, based on something as irrelevant as the Seidenberg and Petitto critique? We suspect that P&B find the "old parts" notion unappealing because it threatens the cherished notion of linguistic autonomy: If language was originally constructed out of nonlinguistic mental stuff, then it may still share its hardware and software with other mental systems.

A second inconsistency arises from P&B's commitment to the idea that language is fully canalized, an evolutionary product that is now strongly determined by innate constraints. This belief is incompatible with a number of facts: (1) the huge range of variation that we see from one language to another (i.e., behavioral plasticity); (2) human beings can acquire language in a different modality or with a different part of the brain if the areas that usually mediate language are damaged early in life (i.e., neural plasticity); and (3) natural languages continue to change over time, even within an isolated community. Variations of this kind would be expected in a system that was only weakly canalized. There are many reasons to believe that human evolution is still underway. Indeed, no one who has ever experienced (or witnessed) childbirth in our species could defend with a straight face the notion that we have reached an optimal biological state – and yet, surely P&B would agree with us that the selectional pressures on reproduction are even greater than the selectional pressures that impinge on language.

The best that can be said of the Chomskyan antiselectionist position is that it cannot yet be proven false. On the other hand, the adaptationist view is thoroughly compatible with and supported by evolutionary theory, population genetics, and functionalist linguistics. This is another place where "boring" is "best." Inconsistencies in the P&B position could be eliminated if the authors were willing to jettison their remaining commitments to unrealistically strong notions of linguistic autonomy and linguistic innateness and accept a full functionalist account of language acquisition and language use. This was the move that we made 15 years ago. It was precisely a belief in neo-Darwinian approaches to evolution and a dissatisfaction with Chomskyan biological theorizing that led us (Bates et al. 1979; Bates & MacWhinney 1982; 1989) into explorations of linguistic functionalism. The links between linguistic structure and language use, developed in our earlier papers, were not "yo-heave-ho" and "bow-wow" models; they were based on linguistic analysis and supported by a wide range of data from cross-linguistic and developmental studies of language acquisition and sentence processing. It is not clear whether P&B will be able to make a similar transition. They still have a spiritual commitment to formal linguistic theories in which autonomy and canalization play a major role, a faith that is directly responsible for the inconsistencies that are so apparent in this otherwise delightful paper.

Linguistic function and linguistic evolution[1]

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1. Introduction. Pinker and Bloom (P&B) have done us a service in refuting the widespread belief among generativists that language could not have evolved by natural selection. Their argument has an important implication for the direction of future linguistic research in the evolution of language – generative inquiry into the evolution of language must incorporate a theory of synchronic communicative function. P&B argue that although communicative function plays little or no role in language acquisition in individuals (linguistic ontogeny), it must play some role in the development of language in the species (linguistic phylogeny).[2] My comment enlarges on two ways that the study of current communicative function is important to a theory of the evolution of form.

2. Function and genetic variation in the linguistic faculty. It is obvious that there is some variation in linguistic ability, but it is not obvious whether any of this variable ability is due to genetic variation. P&B (sect. 5.1) cite several pioneering studies in this area that suggest that some of the variation is indeed related to genetics. To interpret such variation, however, we must understand the functional constraints on linguistic form. Variation in biological systems can be due to either random drift or the forces of selection. Distinguishing the two kinds of variation is important for reconstructing evolutionary history. Traits that are closely tied to functions are more likely to have been shaped by the forces of selection, whereas traits that are not linked in any clear way to function may serve as evidence of the unadapted state.

Consider the following scenario: We discover that some subjects produce grammaticality judgments consistent with a definition X′ of a governing category,[3] which is slightly different from definition X, which is held by the majority of the population. Moreover, this definitional difference produces no obvious difference in output, so we can assume that the input to children of speakers with definitions X and X′ is identical.

If we then find that definition X′ is significantly associated with families, and varies independently of other characteristics such as intelligence in general, then we would have a potential case of genetic variation in the language faculty. However, our interpretation of the data would vary according to our conception of the functional role of anaphoric systems.

If anaphora is linked to some communicative function like reference tracking, then studying the ways in which X and X′ serve this function can provide us with evidence for the direction of the evolutionary change. If X′ performs this function more efficiently then we can argue that the direction of change is from X to X′.

3. Form/function mismatches. Synchronic communicative function can also provide information about evolution when it fails to match up with linguistic form. Gould (1980d) points out that we can see the clearest evidence of evolution when a form serves either a function for which it seems ill-designed or no apparent function at all.

Gould's example of the first type is the panda's thumb, which is a modified wrist bone in evolutionary terms. It will be difficult for us to identify analogous elements in language if we have fuzzy notions of what constitutes a good 'match' between linguistic form and function.

An example of the second type is a 'rudimentary organ' like the appendix, which serves no apparent synchronic function. These can be valuable for the insight they provide into earlier states of the system. Once again, however, we cannot identify linguistic 'appendices' unless we have a theory of communicative function.

4. Which theory of function? There are many theorists of communicative function (Foley & Van Valin 1984; Halliday 1985; Tomlin 1987), their research answers some of the questions raised above. Yet theories of function from nongenerativist perspectives will almost, by definition, fail to answer our questions about the evolution of form. To study the evolution of linguistic form, we must agree on what that form is; and bluntly put, few if any theorists of function accept a generative theory of form. The generative notion of form appeals to universal, biologically based rule systems, whereas most functionalists focus on language in use and in the surface patterns that the rule systems generate. At one extreme of functionalist thought there is a denial that rule systems even exist, other than in an epiphenomenal sense (Givón 1979).

To resort to further biological analogy, when we study the evolution of the human body, we don't look only at the parts that are obviously involved in its external functions. We could watch people for years and never suspect that they have gall bladders. It is only when we examine the body in very special, unnatural situations that we see the gall bladder. Similarly, in the generative model, the organs of language are often hidden away from the easily observable communicative functions. To study the evolution of these organs requires a different, largely experimental approach to the use of language in discourse.

We have already successfully applied linguistic experimentation in the form of grammaticality judgments to discovering the existence of these organs of language. The challenge that Pinker & Bloom's argument presents is to create a generative approach to linguistic function that will allow us to understand the evolution of universal grammar.

NOTES

1. I thank Dean Falk and Timothy Gage for discussion of the issues involved here. Neither of them should be held accountable for my opinions.

2. I have taken the uniformitarian perspective that the communicative pressures present during the evolution of the language faculty were essentially the same as the pressures of modern communication. This need not be the case, but in the absence of evidence to the contrary, we should assume known processes rather than unknown processes.

3. A governing category is the syntactic domain in which an anaphor (like *himself* or *herself*) must find its antecedent. Thus, *John thinks [I saw himself]* is ungrammatical because the bracketed clause is the governing category for *himself*, and the antecedent, *John*, is not contained within it.

What good is five percent of a language competence?

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Pinker & Bloom (P&B) have defended a selectionist account of language. The thoroughness with which they have done so is most welcome. In many linguistic accounts the opposition to selectionism has not been salient, perhaps because discussions of transformational grammar, its derivatives, and later developments in linguistics have often been framed in biological terms (e.g., Lenneberg 1967). With respect to the psycholinguistics that grew out of Chomsky's linguistics, I have argued that "What followed in theories of language acquisition was closer to creationism than any other part of contemporary psychological research" (Catania 1985a, p. 326; cf. Catania 1978, p. 27). It is reassuring to discover that others have reached a similar conclusion, and I applaud P&B's account for its sophistication and persuasiveness. Among other things, they have shown why anyone concerned with the evolution of language must be familiar with the reasons why we cannot expect to trace the evolution of language by following the development of language in the child and why it is important to be clear about the relation between human and computer languages (for the purpose of constructing an evolutionary scenario, comparing human language to computer language is about as useful as comparing eyes to cameras: Cameras take pictures, but they don't see). I therefore offer my comments here in the hope that they will supplement and strengthen P&B's case.

As P&B have documented, language has often been presented as a unitary competence, and even when the argument has been made that it can be broken down into components, those components have typically been presented as discrete modules rather than as continuously modifiable structures (e.g., Chomsky 1980b). One problem in making a case for the evolution of language is that there are few plausible evolutionary scenarios, if any, based on capacities that seem to have the graded properties required by a selectionist account. I suggest that this is because we have not properly identified the functions served by language, and therefore have not been looking in the right places for relevant evidence.

In any survey of opinions on the functions of language, the odds are that communication will be high on the list. We humans place great value on being understood, on expressing our emotions, on conveying our feelings. Nevertheless, as I shall argue, it is probably correct to say that communicative function did not shape language in the species.

One obvious case against communication as a candidate for the primary function of language is that it has been around as a candidate for a very long time and still has not provided a generally acceptable account of language origins. And for some kinds of communication, language is not necessary. Consider the emotions. Many nonhuman organisms, and especially primates, are highly competent at communicating pleasure, anger, fear, and other emotions (Darwin 1965/1872). Why should language have evolved if its main function was communicating emotions? Within a given species, evolutionary contingencies rarely create new systems that duplicate functions already well served by existing systems. Why would it be important to tell others how we feel if we could do just as well by showing them, through facial expression, posture, or other behavior? (cf. P&B, sect. 3.2: "Grammar is a notoriously poor medium for conveying subtle patterns of emotion.")

My candidate for the primary function of language is more fundamental than that of communication: Language is an efficient way to change another's behavior. By talking, we can change what someone else does. Sometimes what gets done involves nonverbal consequences, as when we ask someone to move something or to bring something to us. Sometimes it involves verbal consequences, as when we change what someone else has to say about something.

The main argument for the primacy of this function is that other functions gain their significance only through it. Is there some more important reason to tell others how we feel than that they may then treat us differently? Is there some more important reason to give information to others than that they may then act upon it? Could any social function more powerful or more general than changing what another individual does have provided the selective contingencies under which human language evolved? This is not an argument against speaking of language as communication but rather it is about the primacy of communication as a function of language. If communication had selective consequences in evolution, it was able to do so only to the extent that it sometimes made a difference by changing the behavior of others.

Language, quintessentially social behavior, can emerge only in organisms whose behaviors is already sensitive to social contingencies. Discriminating the behavior of other organisms, whether of one's own or of other species, has clear selective advantages. For example, predators that can distinguish whether or not they have been noticed by their prey have a distinct advantage over those that cannot; conversely, advantages accrue to prey that can distinguish whether or not they have been noticed by predators (such interspecific social discriminations are especially likely to be selected phylogenically in prey, which usually have little opportunity in their own lifetimes to learn relevant discriminations involving the behavior of their predators; it is too late to learn after one has been caught). Given the importance of maternal-infant interaction and mate or kin selection, we should not be surprised if phylogenic contingencies have also selected intraspecific social discriminative skills that do not have to be learned or that are learned with particular ease. Such intraspecific social stimuli set the stage for language because the organism that is so discriminating has become a potential listener.

We have focused here on the functions of language rather than on its structure. If we ever encountered natural languages in other species, we would probably find that they had structural properties in common with human languages. Depending on where they were discovered, such structural commonalities might be characterized as homologies (e.g., in close primate relatives) or as analogies (e.g., in visiting aliens). But we can often address questions of function independently of those about structure (e.g., anatomical constraints or phylogenically selected motor programs do not determine where and when a bird flies, though they may restrict the topography of its flight).

Compared to properties that have left fossil evidence (e.g., anatomy of the vocal tract), we are at a distinct disadvantage with regard to functional properties. Early human vocal language has left few traces. Nevertheless, as P&B point out, it would be begging the question to assume that language emerged full-blown. If it did not emerge in a saltatory creation, each of its stages must have provided a selective advantage, or at least it must not have conferred a disadvantage. P&B discuss intermediate stages, but we cannot assume that these stages will be represented in contemporary behavior, because many predecessors of the contemporary behavior will not have survived (cf. Dawkins 1982, pp. 38, 104).

Consider intraspecific competition when extreme members of a population have some selective advantage. For example, once capture by predators has selected fast escape in a population, we will eventually find few slow runners in it, even if the same speed was once on advantage, when it was fast relative to the population mean (the evolution of the horse provides a striking example [Simpson 1951]). If we cannot count on the survival of intermediate cases when selection has changed a single dimension of behavior, such as speed or sensory capacity, what then when we try to deal with the multiple and complexly interacting dimensions of language?

The argument so far has been that language could not have evolved as a vehicle of communication (similar arguments could be made for language as an instrument of reason or computation). The scenario that follows assumes instead that it evolved as a form of social control, in a progression from vocal releasers to varied verbal functions shaped by social consequences (cf. Jaynes 1977, pp. 126–38; see Catania 1985b; in press, for more detailed accounts and possibilities for alternative scenarios). Let us start with a band of preverbal hominids in which a minimal but well-established repertory of fixed action patterns is elicited by vocal releasers. We know such behavior to exist among many mammalian and avian species (as when vocal calls affect the behavior of conspecifics, e.g., Kroodsma & Miller 1982; see Provine, 1986, for a contemporary human example).

Assume that the calls of a primate leader once determined the behavior of members of its band as reliably as a releaser elicits a fixed action pattern. At first, the vocabulary of releasers was limited to just a few calls, not yet qualifying as language but with relatively simple effects (corresponding perhaps to those of words such as "come" or "go" or "stop"). Over time and generations, a more extensive repertory of varied calls was differentiated. If the details of these calls were weakly determined phylogenically, this rudimentary verbal control could later be supplemented by variations produced by ontogenic contingencies (evolutionary precedent is available in other species, e.g., in the ontogenic elaboration of phylogenically predisposed birdsong). For example, a dominant speaker might learn to attack listeners who do not respond in characteristic ways, thereby punishing disobedience.

So far we have at least one speaker, probably a dominant male, a relatively restricted repertory of vocal releasers, and a population of listeners responding to the items in the leader's vocal repertory in consistent and characteristic ways. This social control gives the group a competitive edge relative to other similar groups: It keeps the group together during movement, coordinates aggression or flight in encounters with other groups, and so on.

For the leader and the leader's successors, attentive listeners were a prerequisite for exerting verbal control. When the leader's verbal behavior changed the listener's behavior, this consequence presumably strengthened the leader's verbal behavior. Thus, the importance to the leader of having an audience was built in early. For the others, the listeners or followers, access to the leader and thus to the leader's instructions was similarly important; the verbal community could not survive as a group if it was not. Some consequences arranged by the leader may have helped to maintain obedience, but qualities of leadership, such as a commanding look or voice or those other features we sometimes call charismatic, may be evidence for a continuing phylogenic component.

This verbal control was probably asymmetrical, and the individual differences thus implied would make some members of the band more susceptible to it than others, so that some calls became effective only for particular individuals. If so, the singling out of verbal control over individuals by distinctive calls could have been the beginning of the evolution of names.

Once verbal behavior had expanded to an extensive repertory including arbitrary as well as phylogenically determined calls, idiosyncratic repertories developed by particular leaders were ordinarily lost to later generations until some ways of establishing this verbal behavior in the leaders' successors had evolved. The next step in this evolution, perhaps long in coming, was the repetition by the follower of the leader's verbal behavior (this repetition presumably began in an overt vocal form, but cf. Jaynes 1976).

Repetition by the listener became especially critical as vocabularies became more extensive. It initiated relevant vocal behavior in those who were to become the leader's successors (cf. P&B, sect. 5.3.3, para. 3: ". . . tribal chiefs are often both gifted orators and highly polygynous . . .") and incidentally also became a way in which behavior could replicate itself. Another consequence was that repetition of the leader's utterances established conditions under which the leader could give instructions to be followed in the leader's absence, at later times and in other places. In effect, control was gradually transferred from the leader's verbal behavior to the listener's own repetition of it (perhaps as a precursor of what we now call human verbal memory). The extended social organization this allowed expanded the influence of the leader and allowed coordinated human groups to expand beyond the range of the human voice.

We could elaborate further on the scenario, but let us stop here and consider what we have so far. This stage of language competence might have involved combinations of utterances, but a case could be made that the functions we have explored to this point do not demand such combinations. Perhaps all this could happen with a vocal repertory restricted to one-word utterances. If so, would such utterances count as 5% of a language competence? As language, they would be regarded as rudimentary at best, but such language competence could evolve substantially through the elaboration of vocabulary even before it reached a stage at which words were combined.

At some point, the increasing complexity of the vocabulary and its contexts must reach the point at which some calls occur in combination, and their several forms could then evolve further into verbs, nouns, and various modifiers. Perhaps the initial combination of two utterances might be regarded as a saltation, setting the stage for the development of grammars and propositional language. Even if it is so regarded, it is a saltation embedded in many continuities. In any case, if the vocal behavior in this scenario counts only as 5% of a language competence, it is clearly better than none. And if this 5% is granted, the remaining 95% is not too hard to imagine.

Seeing language evolution in the eye: Adaptive complexity or visual illusion?

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Pinker & Bloom (P&B) suggest that the evolution of language is the result of natural selection operating on intermediate grammatical systems, conferring an advantage on a particular system when random modifications lead to an improvement in communication. They thus challenge the assumption that language serves no function until the entire system is present and that language differs from such other complex systems, such as the vertebrate eye, where the acquired function of the whole is needed to explain the specialization of its parts.

1. On the presumed function of language. P&B's argument fails if communication alone is the function of language. The communicative functions that might plausibly be attributed to a partially developed precursor to language presumably include such functions as reference and predication. For this, all that is needed is some system of semantic labeled categories. No "duality of patterning" is necessary – any extendable set of labels for semantic categories will do. For example, imagine a five vowel (a, e, i, o, u) six consonant (p, k, l, r, sh, zh) inventory. Assign the first 11 semantic categories to single sounds, then create new labels by assigning some sound before each of the existing labels. The system is extendable. Hence, no obvious adaptive communicative purpose is served by introducing another layer of patterning – the intricate lexical and phrasal phonology found in all natural languages. P&B might reply that phonological structure serves an indirect communicative function, allowing the speaker to talk more rapidly, for example. This is suspect, however: The average speaking rate of five to six syllables per second is nowhere near the human limit on articulatory speed (Deese 1980). Hence, it is implausible to explain the existence of phonological structure with reference to some hypothetical role it might play in facilitating rapid communication.

P&B might abandon the idea that the primary function of language is to convey propositional content and focus instead on shibboleths and the role language plays in establishing group identity. Exploring the role of phonological structure in aiding human memory might also strengthen P&B's thesis. For whatever reason, human memory benefits from the presence of seemingly irrelevant temporal or spatial structure. At the very least, the social (group identification) and memory functions of language must be recognized if P&B's thesis is to be reconciled with the existence of the elaborately structured, variable, rule-governed phonological systems of natural languages. Otherwise, even the grossest outline of the language system that has evolved in humans is far too complex and intricate for its hypothesized function – like evolving a cannon to kill a flea.

2. Grammatical constancy given language-input variability. According to P&B, the evolution of language is no different from the evolution of the eye. But the failure of an evolved system to incorporate effective competitor solutions seems more difficult to explain in the case of language than in the case of vision. Imagine, for example, that "subjacency" is a universal grammatical principle that reduces parsing complexity (Berwick & Weinberg 1984). It is trivially easy to imagine competing principles that also reduce parsing complexity.[1] In addition, violations of "subjacency" probably occur regularly in the language input as speech errors – perhaps even common errors.[2] Language is acquired under conditions where successful acquisition is defined by a match between the acquired grammatical system and the input data wherever biology doesn't contravene. Maintaining "subjacency" as a universal grammatical constant under these circumstances presupposes that, although subjacency itself developed through natural selection due to the subtle advantage it confers in reducing parsing complexity, an alternative locality principle replacing subjacency could not develop by the same mechanism of selection even when subjacency exacerbates the acquisition problem by increasing the distance, or complicating the relation, between input data and target grammar.

The availability of competing solutions may be greater for language than for vision because of the circumstances under which a mutable language is acquired (matching of input with the output of the target grammar), together with the existence of evidence in the input that may violate a grammatical universal and offer apparent support for an alternative to an existing grammatical principle. Hence, providing a plausible explanation for the absence of competitors for proposed grammatical universals in the many languages of the world is not so trivial as explaining the absence of alternative mechanisms for vertebrate vision.

What may be needed in the case of language is to understand why selection should favor grammatical principles that have already evolved as part of the organism, compared to conceivable alternatives, perhaps even some that are more consistent with the input data.[3] To simply claim that the input is analyzed in accord with existing grammatical universals will not suffice (see Note 2) as an explanation for why easily conceived functionally effective alternatives don't penetrate what appears to be a fixed grammatical system (Universal Grammar), at least if the assumption that speech errors can violate universal principles is correct. The production and comprehension of certain sentences violating existing grammatical constraints can be accomplished on the spot; these sentences logically define the target grammar for the child even if they are produced as errors. Hence, language acquisition principles must somehow be entrenched, preventing sentence structures present in the external target language from entering the internal grammatical system of the child. Otherwise, the constancy of proposed universal grammatical principles is not explained.

3. Getting down to details. The general thrust of the Chomsky and Piattelli-Palmarini position(s) is based on a concern for the details of natural language grammars. Looking closely at grammar from the perspective of its presumed functions is simply unilluminating; the fit between function and system is so loose that it is laughable to attempt an explanation of grammatical properties in terms of solutions – even arbitrary nonunique ones – to functional problems (acquisition, production, parsing) or communicative ones (e.g., expressive power).

The only reason this is not a devastating critique of P&B's position is the assumption that the various functions of language place *conflicting* demands on the grammatical system and that the actual properties of the grammar therefore never look finely tailored to the needs or desiderata of any particular function. P&B's case would be much stronger if they demonstrated that these demands conflict with each other, rather than merely assuming they do. The fit between grammar and function should be loose only where clearly distinct needs can be demonstrated for learners versus perceivers versus producers (viewed either as real time speakers or as ideamongers). P&B claim that adaptationist proposals are testable. Establishing a tight fit between grammatical principles and function, where functional demands do not conflict with one another, would help to support a testable version of their proposal. There are problems, however; ambiguity, for example. Lack of ambiguity should presumably facilitate learners, producers, and perceivers and it should increase or maintain the expressive power of a language. Why is ambiguity so prevalent in natural languages, then? The pervasiveness of lexical, syntactic, and semantic ambiguity may not prove to be a serious problem for P&B's view. But wouldn't it be lovely if someday a specific adaptationist account of language evolution could explain why it's not?

NOTES

1. Subjacency limits the possible application of movement rules. Alternatives to subjacency, which also limit the "distance" between fillers (moved constituents) and gaps (movement sites), can be imagined without much difficulty. Islands of filler-gap certainty might be established by permitting grammatical extractions only from complements, independent of the number of intervening cyclic nodes, or by permitting extraction only of constituents bearing semantically unique nonreversible relations to their predicates, for example.

2. Parsing a subjacency violation is often easy, and perceivers regularly assign a structure involving a subjacency violation in preference to violating other grammatical principles, such as those governing phrase structure. For example, perceivers interpret (1) below to mean, "The man who constructed what died of poverty?" and not, "The man who died of poverty constructed what?"

(1) What did the man who constructed die of poverty?

This is important because it shows that proposed universals will be violated by the parser if the alternative is to leave an input only partially structured. In addition, if my hunch is correct that subjacency violations correspond to commonly occurring speech errors, then coupled with known properties of the parsing mechanism, this will automatically result in systematic subjacency-violations in the input data for the language learner, parsed with the structure appropriate for the meaning intended by the speaker. The question then is why, given acquisition based on input, can't a subjacency-violating grammar be learned by the child?

3. Why entrenched principles should be favored by natural selection is not obvious in all cases. If language is autonomous from other perceptual and cognitive systems, coevolution of the language system and other systems cannot readily explain it. Perhaps coevolution of the various subsystems of grammar can.

Natural selection or shareability?

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To what extent is external language structurally isomorphic with internal brain mechanisms? Pinker and Bloom (P&B) implicitly suggest that this isomorphism is wide and deep. If they are right, natural selection may have considerable explanatory power for natural language. I suggest instead that P&B fail to distinguish adequately *language* – the public, shared system multiple humans create over time – from the human *language faculty* – the mental mechanisms that support the ability to acquire and use language.

Natural selection can only operate on innate mechanisms; it cannot operate on overt behavior (Cosmides & Tooby 1987) or on its external products. Those external products of human behavior may themselves "evolve" over time; however, that evolution does not operate via sexual reproduction of genetic material.

A crucial empirical question, therefore, concerns just how isomorphic external language structure is with internal language faculties. P&B follow Chomsky (1965) and others in assuming that universals – language characteristics seen across different languages – are direct evidence of an isomorphism between external language and internal language faculties. At least four nonmystical hypotheses (see Table 1) can be invoked to explain an observed universal, however. These hypotheses are not

Table 1 (Freyd). *Four ways to relate observed universals in external language with internal brain mechanisms*

1. Environmental reflections	Universals may reflect "truths" (such as, perhaps, ontological distinctions) on earth, much as the "language" of mathematics is often thought to reflect a reality outside the brain. Language could be fairly arbitrarily related to brain mechanisms.
2. Monogenesis	Universals may reflect a single common historical language. Language may be fairly arbitrarily related to brain mechanisms and even fairly arbitrarily related to the world, as clothing fashions are.
3. Biological determinism	External language is structurally isomorphic to brain mechanisms supporting language use. 3A: The brain mechanisms are language-specific; 3B: The brain mechanisms are not language-specific.
4. Shareability	External language has universal properties that predictably emerge when minds share information. The relationship between external language and brain mechanisms is thus not arbitrary; but neither is that relationship characterized by a simple isomorphism.

necessarily exclusive. I will not discuss monogenesis (see Greenberg 1966a) or environmental reflections any further except to say that these are possibilities that should be kept in mind when evaluating the implications of observed universals.

To the extent that biological determinism accounts for language universals, I agree with P&B that natural selection for language specific abilities (see 3A on Table 1) is a likely and parsimonious explanation for many of those universals. Although physical evaluation may be *necessary*, however, it may not be *sufficient* to understand much of human behavior, including linguistic behavior.

Shared knowledge exists in a community of minds over time and space; it does not depend on any one mind specifically, yet it is influenced constantly by individual minds. Perhaps most important, shared knowledge evolves at a much faster rate than our genetic code. This relatively rapid evolution of shared knowledge suggests that shared knowledge cannot be fully predicted from our genetic code. Nor can it be fully understood through an analogy to physical evolution. Although there may be a role for "mutations" in shared knowledge, it does not evolve through sexual reproduction. We need different conceptual tools for analyzing what determines whether knowledge is shareable. We must take into account the constraints of the individual brain, but we also need to consider what happens when brains interact. The emergent properties of shared knowledge suggest the need for an appropriate level of analysis.

Thus, it is the hypothesis of shareability (Freyd 1983) in which I am interested, in particular, a comparison between shareability and biological determinism. Shareability can be expressed in two propositions:

Proposition 1: Shared knowledge structures (e.g., natural languages, shared musical systems) have the structure they have partly by virtue of the fact that the knowledge structures must be shared.

Proposition 2: Internal cognitive representations are influenced by these shared knowledge structures.

Emergent shareability properties need not be directly internalized (although to some extent they may be), so long as they predictably emerge in the creation of shared knowledge structures. The individual brains may have been shaped through physical evolution specifically so that those properties emerge, much as the social structure of an ant colony predictably emerges from groups of individual ants.

The possibility most different from biological determinism is that for at least some properties shareability constraints have predictably shaped external language(s) over human history, above and beyond adaptations at the level of the internal language faculties. To see how shareability might behave this way, consider a very general language universal such as the fact that "many linguistic rules are categorical, all-or-none operations on symbols" (sect. 5.2.2). P&B note that this aspect of language is ideal for communicative systems, implying that it is probably a property with which the language faculty has been endowed through natural selection.

Shareability suggests that the communicative pressures on discreetness are, on the contrary, strong enough to render it unnecessary for internal brain mechanisms to be as fully categorical. Because of sharing constraints, minds in interaction will create shared knowledge structures that have the effect of categorizing concepts into discrete chunks along dimensions (Freyd 1983). As long as there is good reason for brain mechanisms to reflect the continuity of reality, however (especially the temporal continuity of reality [Freyd 1987]), it seems likely that a strong physical evolutionary pull would be toward computations emphasizing gradation, not categorization.

An interesting empirical test case for shareability is American sign language (ASL), a linguistic medium potentially supporting gradations, because of the spatial medium. As Newport (1981) and others have noted, however, ASL exhibits the same sort of discrete structure that spoken languages do, such that middle values between discrete morphemes are not allowed. What is particularly fascinating is that the discreteness of sign languages seems to increase with the age of the systems: "Early, newly evolved communication systems display this analytic character to a lesser degree than older, more successively learned communication systems" (Newport 1981, p. 118). Shareability predicts this: Suppose that each individual's representation of concepts is fuzzy in structure, supporting lots of gradations along continuous categories. If a group of individuals attempts to share information within a particular domain, there will initially be enormous potential for information distortion because of the graded nature of the internal representations. However, across time and given different individuals, certain modal values will emerge as anchors within the shared structure. For example, new terms will be introduced through explicit or implicit comparison with old terms. The sharing process will act like a discrete filter that is relatively stable across time and space, despite inherent fuzziness and individual variance. None of this is to say that some of the communicative pressure for discreteness has not been physically internalized (indeed, phonemic computations may be such an example); instead, there is no evidence or logical necessity that all of the discreteness we see in external languages reflects internal categorization.

Shareability can be applied to syntactic regularities as well as semantic ones. The fact that grammars can be described by generative transformational rules may reflect language evolution and may have relatively little to do with physical evolution. We need only assume that language innovations will be more or less likely to survive given their shareability, and, in particular, their memorability and learnability given preexisting structures. Consider an analogy to the transformational structure of English orthography. Chomsky and Halle (1968) showed that a set of transformational rules can be used to relate English orthography to pronunciation. Miraculously, the "deep structure" resembles Middle English, and the transformations resemble phonemic shifts that have occurred since English spelling was frozen (see Crothers & Shibatani 1975). I doubt that many would want to say there is a psychological reality to this transformational structure for English orthography. Syntactic relationships may have much the same sort of historical significance. A new grammatical construction must be rooted in established constructions; one way for this to occur would be a learnable generative rule that specifies paraphrases from the old construction to the new. Similarly, a syntactic change that involved a replacement of one form with the other would be vastly more shareable if it applied generatively, as opposed to requiring the memorization of exceptions. (The very lack of shareability may be deliberately exploited for social reasons, as in argot that defines in-group members. Widespread and long-term language change, however, should move toward shareable innovations making for a regular, generative, change.)

I have argued that shareability is a plausible alternative to biological determinism for some aspects of external language. But how does one weigh the relative plausibility of these two explanations for particular language universals? One might consider other shared knowledge systems for which there is little evidence of any specific genetic adaptation. Writing systems, for instance, surely evolved at the level of shared information; given their modernity, their evolution has probably not been mirrored in changes at the genetic level. Although there is some diversity in writing systems, there are also some remarkable similarities. Mature writing systems have visual cues for individual words, as opposed to whole phrases or sentences. Production rules for handwritten symbols optimize both ease of production (reducing the number of strokes and the difficulty of making connections between strokes) and legibility (maximizing differences between individual symbols). Like natural language changes, changes in writing systems improve shareability, but unlike natural language systems, there is little chance that the properties are isomorphic with physically evolved mental structures.

In summary: The brain faculties supporting language acquisition and language use (whether language-specific or more general cognitive mechanisms) must have evolved through natural selection. P&B have shown convincingly that the complexity of language itself does not rule out natural selection any more than the complexity of the eye rules out natural selection. Language, however, is not an eye. Language is likely to be a product of "evolution" occurring at the level of shared information. Evolution of shared knowledge has emergent properties requiring appropriate conceptual analysis, much as the theory of natural selection highlights properties that emerge from the physical and chemical building blocks of living organisms.

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A Rube Goldberg machine par excellence

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How language arrived on the scene is certainly one of the most interesting and at the same time puzzling questions with which linguists have been confronted. Early linguists assiduously avoided the question; recent linguists have not fared much better by postulating that language is an emergent property arrived at by some unknown and perhaps unknowable means. The fact that Pinker & Bloom (P&B) have so carefully established a framework within evolutionary theory for addressing the question seriously is therefore very welcome. I would like to suggest a refinement in posing the question that may make the problem more tractable, however, providing explanations in evolutionary terms, and that has a measure of empirical support.

My modest proposal is that there is no such entity as *language* and that there is therefore no sense in asking how *it* evolved. Rather, language should be thought of as a great Rube Goldberg machine, a tinkerer's delight, made up of bits and pieces that evolved at different times for different purposes. Such a view is entirely in keeping with what we know about the way evolution works and is, I think, also consistent with what P&B are proposing. They carefully lay out 10 different subsystems, each with somewhat different properties, which get put together to produce a grammar. Whether these are the right joints at which to cut remains an empirical question, but no one doubts that such subsystems exist. It seems not unreasonable, therefore, to suppose that these separate parts may have separate evolutionary histories. Such a view changes the questions and the answers about the evolutionary history of language; some parts may have an adaptive story, others may have arisen from more global changes.

There is converging evidence that in some cases of developmental dysphasia subjects are incapable of marking syntacticosemantic features in the underlying lexicon and, as a result, morphological and syntactic rules that depend on features are impaired (Clahsen 1989; Gopnik, in press). It can be shown that all such features are impaired as are all grammatical consequences, even some that are quite surprising. This does not mean that these subjects do not know the meanings carried by these features or cannot learn the appropriate lexical items to encode them. For example, (Gopnik, submitted) they can learn, certainly by the time they are adults, that "books" means more than one reading object and they can use it appropriately to refer to a set of such objects; but unlike normal three year olds, they never learn that there is an underlying rule for plural formation in English. They cannot, even at 74, pluralize nonsense words and these dysphasics do not make errors such as "foots." They cannot judge or correct errors in features nor can they reliably produce feature-marked words in stipulated contexts or recognize agreement rules that depend on features. Moreover, patterns of familial aggregation, especially in a three generational family of 30 people of whom 15 are dysphasic, seem to suggest that this impairment is associated with a single dominant gene (Gopnik 1990; Samples & Lane 1985; Tomblin 1989).

Bickerton (1984) describes precisely the mirror image of this process in the change from pidgins to creoles. In feature-blind dysphasics, distinctions that are carried by features in the normal language input turn up as lexical items in the dysphasic grammar; in normal children, distinctions that are carried by lexical items in the pidgin input turn up as features in the child's grammar.

Now what does all of this tell us about the evolution of language? First, it suggests that the part of the grammar that marks syntacticosemantic features – and the part that is seen to be a subsystem of grammar in every theory of language, that operates in normal children even in the absence of feature-marked input, and that is selectively impairable and perhaps genetically determined – might be a good candidate for an independent evolutionary history. Let me take a stab at such a story. The first question to ask concerns what good such features are to us in language. We know that many, but not all, languages have such systems. And we know further that the distinctions such features encode come from a set of semantic distinctions that seem important on independent grounds: number, animacy, tense, aspect, shape, and so forth. Not all distinctions are coded in all languages, but when they are encoded they are usually obligatory. In English, you *must* mark number and gender on pronouns, much to the consternation of anyone trying to write general rules.

What can such a system buy us? P&B suggest that it can allow agreement rules to be constructed that can indicate relationships among elements in the sentence; between the subject and its verb or between the subject and its adjectives, for example. By having these relationships marked in this way, word order, which often carries these relationships, can be used for other purposes, such as topicalization. They argue that a system that allows word order to be manipulated carries an adaptive advantage. This may be so. But surely if one were starting from the ground up one would not design a system for establishing these dependencies in this way. Though features can do the job, they do it imperfectly at best. They make clear the dependencies in question only in those cases where the two referents in the world vary on a semantic dimension that is in fact marked in the feature system of the language. Moreover, as noted above, all the redundant features must also be marked.

One could design a much more efficient system. For example, the first noun and all of its dependencies could be marked with a low tone, the next noun and its dependencies with a higher tone, and so forth. Tonal subscripting would provide an unambiguous nonredundant system of marking. But no language uses such a system. This is perfectly consistent with what is known about evolution. You never get a chance to start from the ground up, rather you must try to fashion an old solution to new uses.

I would suggest that what was already there was a system of representation that distinguished among the semantic categories cited above. These basic cognitive categories mark distinctions that we know many other organisms make. But making the distinction and knowing the distinction are two different things. To build a feature system you must not only make the distinctions, you must be able to move up to the metalevel and build rules that operate on these distinctions abstracted from their occurrence in particular representations. Given that these categories were already necessarily present in any representation of a referent in the world, and given that explicitly marking dependencies was important to do, then using the already present categories to indicate these dependencies, even imperfectly, was a natural exploitation of an already available resource once the ability to represent these categories abstractly was accomplished. Whether this move to abstraction was part of a general cognitive change or specific to these categories is still an open question. It is interesting to note that feature-blind dysphasics seem to be able to handle other abstract systems with no difficulty.

A "just-so" story? Perhaps. But it seems to me to be in the right direction; a modest local story that accords with a constrained set of facts.

Selecting grammars

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The denial that natural language (NL) is adaptive may lead to at least three different claims. The first is the extremely strong claim that the NLs could not have been selected for as their properties are logically incompatible with a selectionist account. A second, weaker but nonetheless valid claim is that there are no known plausible evolutionary scenarios that could account for any of the properties of NLs. A third, yet weaker claim is that there are no plausible evolutionary scenarios that account for the grammatical properties of universal grammar that linguists have been interested in, such as structure dependence, empty category principal (ECP), subjacency, and so forth. We can label the first position the "NL-are-spandrels" view, the second, "the NLs-might-be-spandrels" view; and the third, the "grammars-are-spandrels" view. Pinker & Bloom (P&B) vigorously argue that there is no compelling reason to hold the first position. There is no logical incompatibility in venerating both Chomsky and Gould.

To rebut the NLs-might-be-spandrels view would require doing quite a bit more than P&B attempt here. It would require doing at least what Brandon and Hornstein (1986) did, and quite a bit more. The latter paper started from the assumption that there were no capacities that were favored across all environments. The trick was to find one in which there was selective pressure for something having the properties of a NL. The four NL properties focused on were these: NLs are symbolic recursive communication devices that are not stimulus bound for their use. These are quite distinctive properties that separate human NLs from virtually every other animal communication system. The relevant environment was argued to be one that favored phenotypic transmission mechanisms and these are further specifiable as rapidly fluctuating and moderately capricious. In these sorts of environments there is selective pressure for a communication system that has the four properties noted above.

Of course, to complete this account it is also necessary to show that NLs in fact arose to "solve" the specific problem that rapidly changing, moderately capricious environments posed. A good first step in this direction would be to show that this sort of environment is one that our ancestors actually inhabited.

It is worth observing that if this could be done it would show only that *some* properties of NLs were selected for. Many others would not have been. Thus, the grammars-are-spandrels view is untouched by these specific considerations, as the imagined environment does not obviously select for any properties beyond the four noted above. For example, any communication system that met these but had nonstructure dependent rules would be subject to exactly the same amount of evolutionary pressure as the NLs that actually evolved. Thus, why all NLs exploit structure-dependent rules cannot be explained on these evolutionary grounds. This does not mean that this property does not have an adaptationist account. Until one is produced, however, one cannot tell. Furthermore, it is very hard at present to imagine any such accounts that carry any degree of conviction. The position, then, that virtually all of the specific *grammatical* properties of NLs might well be there for reasons unconnected to adaptation is quite plausible for all we currently know. Of course, these properties cannot be counteradaptive, but they may not be particularly adaptive either. If so, their presence would have to be accounted for in nonevolutionary terms and these properties of NLs would not be traceable to the workings of evolution.

It is significant that P&B do not actually produce an evolutionary model for the selection of specific grammatical properties. What evolutionary pressure selects for the case filter or structure dependence or the binding theory or X′ theory? It seems at first blush that a perfectly serviceable communication system that did not mark "abstract" case on NPs could be just as good a medium of communication as one that does. In fact, despite P&B's sensitivity to providing "just-so" stories in section 3.1, that indeed is all they provide. They do not begin to offer even the outlines of an account of what *specific* environmental pressures *specific* grammatical properties are responses to, let alone evidence that these pressures were actually impinging on our ancestors. Nor do they suggest what sorts of tradeoffs might have led to natural selection choosing some specific principle of grammar. Until this is done, however, very little has been accomplished by way of evolutionarily explaining these properties. This does not mean that we shouldn't believe in our heart of hearts that evolution *must* be responsible for all of this complexity. Perhaps it is. This strong conviction must be theoretically cashed in, however, lest evolutionary explanations become devoid of all explanatory power.

In closing, let me focus on a specific case that highlights the difficulties. P&B suggest that subjacency might be accounted for in terms of parsing considerations. They cite Berwick and Weinberg (1984) who show that a bound on left context allows us to treat NLs as parsed by LR(k) parsers and this allows efficient parsability, that is, parsing in linear time. This account is of real interest because in fact we do seem to process sentences as we hear them, that is, in linear time. Thus, one can look on subjacency as functionally related to parsing efficiency and thereby partially accounted for. To give an *adaptive* account, however, more is required: Under what environmental conditions is real time parsing adaptive? What was the selective pressure that weeded out the "slow" parsers? What costs were paid to effect this remarkable processing speed?

My impression is that P&B appreciate the point made here. Their conclusion is that "there is nothing 'necessary' about the precise English version of the constraint (i.e., subjacency N.H.) or the small sample of alternatives allowed within natural language." They further speculate, however, that subjacency "may" represent the best compromise given the demands of expressiveness and parsability. I agree, it "may." But to show that there is strong reason to think that it does one would have to spell out the environmental pressures that would make efficient parsing advantageous and the trading relations among the alternatives that would make subjacency the least costly way of meeting this demand. Despite the vigor of their discussion and many interesting observations along the way, P&B never provide a single detailed discussion of this type for a grammatical principle. Unfortunately, nothing less will do if what P&B wish to show is that the complex properties of grammars are due to the workings of natural selection.

Beyond the roadblock in linguistic evolution studies

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1. Liberation! Pinker & Bloom's (P&B's) target article is deeply satisfying and liberating. They correctly diagnose the consensus in linguistics and cognitive science, nurtured by the writings of Chomsky and Gould, that "language may not be the product of natural selection." P&B confront this stifling consensus head on, systematically addressing and rebutting its various assumptions with cogent analysis and informed counterargument. Theirs is not a revolutionary new insight, but rather the clearing away of spurious intellectual obstacles that had begun to block the path of a research program to integrate linguistics and evolutionary biology. In this exercise, P&B place in proper perspective various recent scholarly traditions, such as generative grammar (and in particular its Chomskyan interpretation as a theory of the innate language acquisition device), Lieberman's (1984) theory of the nonautonomous evolution of the language faculty, and the "ontogenetic functionalism" of such language acquisition researchers as Bates (1988). Being committed to exploring the phylogenetic mechanisms by which the structured innate human language faculty could have evolved, I wholeheartedly welcome their intervention. In the rest of this commentary I take up one major point and one minor one.

2. Linguistic structure and linguistic function. First, some semantic clarification. Linguists use "function" in a variety of ways, some of which intrude on what is properly not "function," but rather structure. Well-known examples are the so-called grammatical functions, such as Subject, Direct Object, and the like. The family of linguistic theories, which includes Dik (1978) and Halliday (1985), call themselves "functional theories" or "functional grammars," but many, if not most, of the functions these theories discuss such as "Actor," "Topic," "Theme" are elements in structural configurations distinguished by the level at which they apply. Contrast this with the ordinary language use of function, in which the function of some tool is not some part of its own structure but some task described independently of the shape of the tool itself. A spade, for example, has two parts that are not functions, but rather *serve* functions, namely, being held (the function of the handle) and cutting into the earth (the function of the blade). Similarly, to take up P&B's analogy with the vertebrate eye, the lens is a *part* of the eye, whose *function* is to focus light on the retina. The lens is no more a function of the eye than focusing light is one of the eye's parts.

This relates to a worry that nags me about P&B's paper. It arises from passages such as: "Grammar is a complex mechanism tailored to the transmission of propositional structures through a serial interface" (Abstract). "Devices designed for communicating precise information about time, space, predicate-argument relations, restrictive modification, and modality . . ." (sect. 5.3.2). The first of these quotations sounds dangerously like "Our noses were made to carry spectacles, so we have spectacles," which P&B deride.

The question is whether things like propositional structures, predicate-argument relations, restrictive modification, and modality are elements of the structure of languages or whether expressing these things constitutes some subset of the functions that various parts of languages serve. Both cannot be the case. Did predicate-argument relations, for example, somehow exist before language in a domain independent of language, like time and space, and, arguably, discrete physical objects, colors, noises, and smells? If predicate-argument relations do predate language in this way, they are not themselves part of the structure of language, although expressing them could well be a function of some particular part of language structure. If, on the other hand, predicate-argument structure is part of language structure, then we face the task of specifying what nonlinguistic aspect of the world it is its function to express.

The difference is rather like that between nouns and physical objects. Physical objects are typically denoted by nouns. Nouns are elements of language structure whose function is prototypically to denote physical objects. It is not a primary function of language to communicate information about nouns, but rather to communicate information about objects, and nouns are the structural elements in languages that serve this function. (To complicate matters, this *is* a function of language *used as a metalanguage* to communicate information about nouns.) So I think P&B are being careless about this important distinction when they write of "Devices designed for communicating precise information about . . . predicate-argument relations, restrictive modification, and modality . . . ." For each of these it could be argued that they are not what language is designed to communicate information about; rather, they are parts of language designed to market information about various aspects of nonlinguistic reality. Articulating just how this nonlinguistic reality is structured, and how aspects of language form are adapted to communicating this reality, is a major research challenge.

P&B may not be radical enough in their vision of evolutionary explanations for the form of language. As noted, they regard grammar as "a complex mechanism tailored to the transmission of propositional structures though a serial interface." (By propositional structures P&B presumably mean structures in which a distinction exists at least between a predicate and one or more arguments.) This does not push the search for explanations far enough back, however. The next set of questions needs to be put: Why should a mechanism for the transmission of propositional structures through a serial interface be selected for? Why propositional structures (as opposed to, say, analog representations of visual images)? It is conceivable that propositional structures themselves arise through the selection of an effective means for transmitting *messages* (information), where the latter are taken to be more primitive than propositional structures. That is, messages have the "aboutness" associated with propositions, but not necessarily their predicate-argument structure. And why a serial interface (as opposed to, say, a generatable set of infinitely graded static body positions)? And, finally, the most radical question, why should transmission of any kind between individuals bring selective advantage? These are some of the research questions we should be asking, and which would arise from a somewhat more radical view of what needs to be explained than that of P&B, who are somewhat conservative and cautious in this regard.

3. Grammars are not genetically transmitted. In section 5.2.2, P&B express a thought in a compressed way that might cause misunderstanding. I am confident that they do not themselves suffer this misconception, but it is worth pointing out the risky passage. It is: "No single mutation or recombination could have led to an entire universal grammar, but it could have led a parent with an n-rule grammar to have an offspring with an $n + 1$ rule grammar, or a parent with an m-symbol rule to have an offspring with an $m + 1$ symbol rule" (sect. 5.2.2). In the normal terminology of generative grammar, a grammar is specific to a particular language and is acquired on exposure to relevant primary linguistic data. A grammar, in this sense, is not coded into the genes. Prefixing the modifier "universal" makes all the difference. Universal grammar *is* coded into the genes, but generativists do not typically speak of universal grammar as consisting of rules, but rather of principles and parameters. P&B would have been less prone to misunderstanding if they had written: "A single mutation . . . could have led a parent with an n-parameter (or n-principle) universal grammar to have an offspring with an $n + 1$ parameter (or $n + 1$ principle) universal grammar."

4. Conclusion. To reiterate, P&B have demolished some intellectual roadblocks to progress in understanding the relation between evolution and language. One may wonder how such blocks come to exist. Part of the answer must be the narrow specialism that plagues much modern scholarship. Your typical linguist doesn't know much about evolution and is content to adopt the stance toward natural selection that emerges from popular writings such as Gould's, or from Chomsky's various comments (Chomsky 1968, pp. 82–83; 1980, pp. 99–100; 1982, p. 29; 1988, pp. 167 & 170). The work of such truly interdisciplinary scholars as Pinker, and the existence of such truly interdisciplinary outlets as *BBS*, allows us to see over the barricades, and, one hopes, eventually to sweep them away.

What would a theory of language evolution have to look like?

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1. First a remark on what seems to be at stake in the argument that natural selection could not produce an innate, highly structured universal grammar that serves as the substrate for the child's acquisition of linguistic competence. For most commentators, this argument has served as part of a larger agenda of discrediting the claim that language is a specialized mental capacity, and of showing that linguistic competence can be accounted for purely in terms of a general purpose cognitive architecture, be it associationist, symbol-processing, or parallel distributed processing. This agenda, however, largely ignores the very real problems of characterizing the linguistic code that have preoccupied thousands of linguists during the last 30 years. Whether or not current solutions are correct, the complexity of the phenomena cannot be denied. How would it look if someone were to dismiss so easily the problems studied by, say, molecular biologists?

The more puzzling case, as Pinker & Bloom (P&B) point out, is Chomsky himself, who one would think would have a stake in language having evolved by attested evolutionary mechanisms. Much as I am indebted to Chomsky for virtually creating the discipline within which I work, my suspicion is that his view in this matter is a particular case of a more general rhetorical strategy he has been known to invoke against attacks on the overall feasibility of generative grammar: To argue that evidence from outside grammatical competence that apparently does not square with grammatical evidence is in principle irrelevant to his problem, the characterization of universal grammar (UG).

Although in the short-term Chomsky may be right in this argument – evolutionary considerations are not going to settle technical details of syntactic theory – the effect of this mutual discounting is a profound alienation that inhibits cross-disciplinary interaction on the Big Issue of understanding mind and brain.

2. To help disinhibit some pathways of communication, let's try to clarify some real issues facing an integration of language into evolutionary theory.

The problem I see is this: All the characteristics of organisms that have been examined for evidence of natural selection have been either physical structure – wings, feet, body size – or patterns of behavior – alarm calls, social organization, reproductive ritual. The argument for natural selection is always that the particular characteristic can have been shaped from known or plausible variants in other species. As a consequence, the search for antecedents of language has quite naturally focused on linguistic behavior.

Linguistic theory is not about behavior, however, but rather about the mental representations that help determine perception and behavior. UG, under this construal, characterizes the combinatorial repertoire of the forms of information that mediate between conceptual representations (roughly, the "language of thought") and the peripheral motor and acoustic information that permits transmission and detection of physical signals. UG alone, though, is not sufficient to make the channel usable; the combinatorial repertoire must be further constrained to form grammars for particular languages (including their lexicons).

What is pretty much a mystery at this point is how linguistic rules and representations are neurally instantiated – that is, how physical structure in the brain could make possible the combinatorial regularities discovered by linguistic research. In fact, other than certain aspects of low-level vision, I know of no success at relating systematicities of mental representation to the details of neural architecture. Conversely, little is known about the combinatorial properties of (i.e., representational treatment of) high-level vision, planning, or motor activity; and what is known is not correlated with neural properties beyond gross brain localization.

The upshot is that linguistic theory is not stated in a form that can be properly compared with putative evolutionary antecedents. For a comparison to be meaningful, the question of the evolution of UG must be stated in one of the following forms: (1) How could the neural architecture supporting UG have evolved from something else? or (2) How could the representational capacity (the combinatorial organization) of UG have evolved from the representational capacity of something else?

The difficulty in answering question (1) is that we know almost nothing about the connection of linguistic representations to neural architecture; the difficulty in answering question (2) is that we don't know much about representations outside the language faculty.

3. Actually, though, there is a case where we can begin to see the shape of an answer to question (2). Lerdahl and Jackendoff (1983) and Jackendoff (1989b) show that the representations supporting the rhythmic structure of music bear a strong resemblance to those for the rhythmic organization of speech, roughly the stress and timing subsystems of phonology. It does not appear, however, that one can force a complete identity on these systems. Instead, they seem to be related rather the way fingers and toes are: systems with the same basic morphology but differentiated for distinct functions. Although the evolution of music is, if anything, more of a mystery than the evolution of language (what selectional pressures seem even remotely plausible?), it is possible to see these two rhythmic representations as alternative differentiations and specializations of a common ancestor. One would guess that this ancestor had something to do with production of temporal regularities in behavior; but because there is no commensurate *representational* theory of such abilities, it is hard to formulate coherent hypotheses.

Also in the phonological domain, recent theories of autosegmental phonology and feature geometry begin to make phonological structure less like a sequence of discrete segments and more like a stream of independent processes governing the position of vocal tract articulators, coordinated by a "timing skeleton." (Goldsmith, 1990, summarizes much of this research; see also Clements 1985; Halle, forthcoming; McCarthy 1988; and Sagey 1986.) Again, one can suspect evolutionary antecedents in motor control and motor planning, but in the absence of commensurate representational theories, the precise innovations made in phonological representations cannot be evaluated.

4. A somewhat different issue arises concerning the evolution of language learning. There are two possibilities consistent with the specialization of linguistic representations: (1) Along with the linguistic representations provided by UG, a learning device evolved to make grammars of particular languages learnable. (2) The constraints provided by UG plus independent general-purpose learning strategies are sufficient to account for language acquisition. One would be happier prima facie with (2); but, in the absence of representational theories of rule learning in other capacities or of neural theories of learning related to representations, the issue can't yet be properly addressed. I suspect that current research in learning by neural nets may come to bear on this issue when it comes to be understood how to encode genuine rather than toy linguistic representations in network terms.

5. Suppose one could justify much of phonological structure as a specialization of motor programs. Suppose further that one could justify conceptual structure ("meaning") as a specialization of the "language of thought" available to animals, which coordinates representations derived from various faculties and encodes basic parameters of nonlinguistic categorization (Jackendoff 1983). What else would you need to get a language system off the ground?

At the very least, you would need a channel whereby vocal productions can be construed as expressing concepts, that is, an ability to take vocal productions as conventional symbols for something else – a rudimentary sound-meaning mapping. This is perhaps what we are seeing in the studies of ape language: This much of language appears to be discoverable in other biological systems.

This leaves syntax, the essential part of Chomsky's conception of language, to be accounted for. Notice, however, that having a rudimentary symbolic ability would create exactly the sorts of evolutionary pressures that P&B discuss: Reliable complex communication greatly benefits from a finely tuned system for correlating novel sound combinations with novel conceptual combinations. But this is just what syntax does.

I am therefore suggesting that syntax – the component of language for which evolutionary antecedents are hardest to imagine – has evolved as a refinement and elaboration of a preexisting informational link between phonological and conceptual structure. The evolution of this way-station has proceeded to the point where it has taken on its own autonomous properties in the service of greater expressivity and reliability. I leave it to connoisseurs of evolution to argue whether or not this is a development with parallels elsewhere, but to me it has the right flavor: a structural innovation that strengthens a connection between preexisting antecedents.

Five exaptations in speech: Reducing the arbitrariness of the constraints on language

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Pinker & Bloom (P&B) argue that because language, like an eye, shows clear evidence of design for a specific purpose it could only have evolved by the traditional Darwinian mechanism of natural selection. Unlike eyes, whose design appears to be determined by engineering constraints (which apply equally to the design of cameras), many of the constraints on language design appear to be formal, and essentially arbitrary, rather than functional (sects. 3.3–4). In this commentary, I outline five features of speech production and perception, all of which appear to be exaptations, in an effort to evaluate the arbitrariness of this aspect of linguistic behavior. A beneficial side effect of this evaluation is appropriately specific evidence for more phyletic continuity (cf. sect. 5.4) than is typically granted to language.

Consonantal gestures are relatively brief and rapid movements of lips or parts of the tongue, which change the vocal tract configuration locally, whereas vocalic gestures are larger and slower movements of the jaw and tongue mass, which change the vocal tract configuration globally. Furthermore, consonantal gestures are incorporated into a continuous cycle of vowel gestures (Öhman 1966; Fowler 1983), such that their timing can be predicted from that of vowels but not vice versa (Tuller & Kelso 1984; Tuller et al. 1982; and cf. Munhall 1985). Vowel gestures also determine the sequence of consonants in clusters such that within a syllable the more vowel-like consonants occur closer to the vowel (see Clements, 1990, for review). Incorporating noncyclic gestures at specific points in an ongoing cycle of movements closely resembles the incorporation of food transport and swallowing movements into the cyclic jaw movements of chewing (Franks et al. 1984; Hiiemae & Crompton 1984), suggesting that the pattern in speech is taken over from eating, with modifications specific to manipulating the shape of vocal tract resonators in place of ingesting food.

The perceptual interactions between the dimensions of acoustic difference among contrasting phonemes and the very construction of acoustic categories out of these dimensions also rely on non- or prelinguistic abilities. The first point is demonstrated by the fact that the same interactions are observed in listeners' responses to nonspeech analogues of speech (Best et al. 1981; Diehl & Kluender 1989; Kluender et al. 1988; Parker 1988; Parker et al. 1986), and the second by the fact that nonhuman listeners form the same categories that human listeners do (Diehl & Kluender 1989; Kluender et al. 1987; Kuhl 1981; Kuhl & Miller 1978; Kuhl & Padden 1983). These two aspects of speech perception would appear, therefore, to be founded on general auditory mechanisms rather than speech-specific ones (cf. Lieberman & Mattingly 1985; 1989).

Finally, language uses contrasts in the distribution of energy across the spectrum to convey contrasts in meaning that are quite parallel to those signaled by such contrasts in many nonhuman vocalizations (for review, see Ohala 1983), where a high-frequency bias is found in submissive signals and a low-frequency bias in aggressive ones, apparently because the high-frequency bias makes the signaler appear smaller, younger, and thus less threatening, whereas the low-frequency bias indicates a large, older, and thus more formidable animal. In speech, pitch is typically higher in questions, conveying the speaker's uncertainty, than in statements; in diminutive/augmentative morphology, the selection of vowels and consonants across languages favors those in which a high-frequency bias corresponds to smallness and vice versa. Both patterns show the formal encoding of the same form-meaning correspondence as in nonlanguage-using organisms' vocalizations.

In none of these cases is the behavior taken over unchanged from its nonlinguistic precursors, but to the extent that these parallels are genuine homologies, the apparent arbitrariness of speech is reduced.

An apparent exaptation for speech is the long pharynx allowed by lowering the larynx in modern *Homo sapiens*. Pharynx lengthening greatly expands the variety of resonator shapes over what was possible in even our most recent relative, Neanderthal, in which a higher larynx is inferred from differences in basicranial angle (Laitman et al. 1979). (That newborn *Homo sapiens* neotenously have the larynx high also suggests that its descent is recent.) However, the assumption of a predominantly upright stance, which probably led to the descent of the larynx, appears to have occurred much earlier in the lineage, perhaps as early as *Australopithecus afarensis* (Simons 1989), so why did the larynx lower so much later? This question can be answered by another: Do qualitative differences between the speech of Neanderthals and modern *Homo sapiens* necessarily imply that the grammars of Neanderthal languages are otherwise fundamentally different from those of modern languages? Even if language is a product of interlocking systems (cf. sect. 5.2.3), the answer is surely no, because these systems are also highly autonomous and could, in principle, have evolved at different times, with the modern vocal tract and its acoustic possibilities lagging behind others.

Speech is characteristically a rapid and coordinated modulation of the acoustic signal, with commutation of its elements. The modulation would have led to segmentation and thus have been the prerequisite for commutation of segments. Modulation is not entirely absent from the vocalizations of other primates: The coos of Japanese macaques (Green 1975) exhibit the kind of modulation that allows segmentation, but commutation is not observed. The modulation of macaque coos suggests that this property of speech may be latent in primates. If so, then modulation and the commutation it allows would be an exaptation in speech rather than a true innovation, and the question is why this capacity is not exploited by primates other than *Homo sapiens*. What modulation and commutation allow is the composition of an indefinitely large number of messages from a small inventory of elements meaningless in themselves. In short, these properties of the signal are the essential medium for the arbitrariness of the sign, and their evolution follows from the necessity that linguistic signs be arbitrary.

Lessons from the study of speech perception

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Pinker & Bloom (P&B) make two basic claims. First, a system with the complex design of human language requires specialized mechanisms. Second, processes of natural selection are adequate to account for the development of natural language by *Homo sapiens*. The latter claim does not imply the former. If one appeals to specialized processes then one must suggest how those processes came to exist, and natural selection is the appropriate candidate. The fact that natural selection can deliver putative specializations, however, does not necessarily imply that such specializations exist.

P&B argue persuasively that natural selection gave rise to processes necessary for human language; they may be too anxious to disengage language from processes general to learning and cognition, however. Consider the related case for speech perception. Here, too, an appeal to special mechanisms led to a neglect of general solutions. Early studies of phonetic categorization revealed behavior not easily accommodated by either '60s-style learning theory or by studies of psychoacoustics. The failure of then-contemporary theories gave rise to the view that phonetic categorization requires the operation of a uniquely human "phonetic mode" that was claimed to be specific to speech, sharing very little with auditory or visual categorization processes for other environmental events (Liberman et al. 1967; Liberman & Mattingly 1989). This "special mechanisms hypothesis" dominated theorizing in the field of speech perception for most of the last two decades, as many researchers felt it necessary to posit the innate existence of uniquely human and highly specialized structures and processes for the perception of speech. Much of the justification for specialized mechanisms was of a somewhat negative sort and lay in an alleged inadequacy of general mechanisms to support the process of speech perception.

The persistent appeal to speech specific mechanisms has been like a table with several legs. In succession, the special mechanisms hypothesis has found support in phenomena such as categorical perception (Studdert-Kennedy et al. 1970), selective adaptation (Eimas & Corbit 1973), phonetic trading relations (Repp 1982), and duplex perception (Liberman & Mattingly 1989). But each leg has, in its time, been cut away as later experiments gave evidence that more general processes were at work. Categorical perception (Miller et al. 1976; Pastore et al. 1977), selective adaptation (Diehl 1976; Remez 1979), trading relations (Parker et al. 1986; Kluender et al. 1988), and duplex perception (Fowler & Rosenblum 1990) have all since been demonstrated for nonspeech sounds. Moreover, phonetic categorization (Kluender et al. 1987), categorical perception (Kuhl 1981; Kuhl & Miller 1978), and phonetic trading relations (Kluender 1988) have all been demonstrated for nonhuman animals.

Despite the fact that evidence for each of these supporting legs has been cut away, the special mechanisms hypothesis continued to stand or, at least, to levitate. Why did the hypothesis persist?

First, the fact that the evidence eventually failed to prove strongly supportive of special mechanisms did not conclusively rule out their existence. (Note, however, that the remaining possibility rests upon the imparsimonious assumption that convergences between speech and nonspeech, and between human and nonhumans, are only serendipitous.) Second, the special mechanisms hypothesis was for some time the only game in town. Although each piece of evidence gave way under increased scrutiny, a new phenomenon always took its place. And, although persisting in the belief that speech was indeed special, most researchers paid little attention to potential alternatives. Theories about modularity of mind (Fodor 1983; see also multiple book review, "The Modularity of the Mind" *BBS* 8(1) 1985) further encouraged a principled ignorance of more general potential explanations. In spite of the dominance of this approach, a few genuinely general approaches to speech perception have been developed (e.g., Diehl & Kluender 1989; Fowler 1986; Lindblom et al. 1984).

The message for P&B, who seem to accept the modularity hypothesis for speech and for language in general, is that one should not let an appeal to some specialized processes give rise to a theory that appeals to only specialized processes. Even those processes specialized for language must have origins in nonlinguistic function. Evolution may not deliver everything a theorist desires, and it never provides solutions de novo. Whether language is like insect wings, one function developing from an unrelated function, or where language represents the slow accretion of ability, language did not come from nowhere. Although P&B forcefully argue that intermediate steps were both possible and useful, it is never made quite clear whether language or any of its precursors came from. When serious thought is given to such precursors, general processes of learning and cognition should come to mind. One might accordingly expect P&B to agree that at some point general psychological processes play an essential role. They are reluctant to allow any role to general processes, however, particularly learning. Although they may be justified in rejecting Gould's argument that, given the sheer computational power of the human brain, one could learn grammar from examples without resorting to special processes, P&B's zeal for the nongeneral is not always well founded.

Consider the simple case in which children must learn language conventions particular to their community P&B suggest that "They (children) must be programmed so that the mere requirement of conformity to the adult code, as subtle and arbitrary as it is, wins over other desiderata." They assume this because some of these conventions have little functional significance for language use. There is no limit, however, to the number of examples in which children learn societal conventions that have no functional value. "Innate arbitrary foundations" are not required. P&B's appeal to natural selection is well founded. And some special processes may be required, particularly for duality of patterning (Hockett 1960). But general processes must be part of the scenario for the evolution of special processes and general processes of learning and cognition probably continue to play an important role in language acquisition and use.

How much did the brain have to change for speech?

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Before turning to an analysis of Pinker & Bloom's (P&B's) claim that the natural language facility must have been the direct result of natural selection, we need to clear away a linguistic confusion of theirs. Both in the very first line of their abstract and in the body of their paper they refer to "people" (apparently Gould and I are among them) who want to argue that the "evolution of the human language faculty cannot be explained by Darwinian natural selection." If there are such people, I do not know them. What Gould and I have argued in many places, including in our spandrels paper (Gould & Lewontin 1979) is that in general one cannot tell after the fact whether or not some feature has been the direct product of natural selection, because there are too many reasonable competing explanations, including direct natural selection. Ours is a methodological claim about uncertainty. P&B have confused two English constructions: "A *cannot* be the cause of B," and "A *need not* be the cause of B." Let us now examine their claim that "A *must* be the cause of B."

The heart of their argument lies in two concepts: complexity and adaptation. They argue that natural language is a complex structure composed of many tightly interacting subfunctions, and that such a complex structure cannot arise by nondirected (i.e., nonselective) forces.

The first difficulty is that this argument depends critically on how we anatomize the linguistic function. That is, we must have a developmentally and genetically correct scheme of what the independent components of language really are. We cannot divide the language function into subfunctions simply on the basis of our conceptual biases.

The second difficulty, which P&B recognize, is that we are not at all clear about how to measure or even order complexity. We do not need an exact mathematical description of complexity, provided our intuitive notions can be cashed out in particular cases. We would all agree that a human being is more complex than a soap bubble, if for no other reason than that the forming of a soap bubble can be described from a simple law of energy minimization. But we cannot all agree that a dog is more complex than a fish, although fishlike forms preceded doglike forms by 500 million years and were their ancestors. How complex is natural language? Compared to what? How complex does something have to be before we find its appearance without design incredible? What, in fact, do we mean by complexity? P&B's rough answer is the one usually given. Complexity increases as the probability of the particular arrangement of matter decreases or as the number of constraint rules necessary to specify the arrangement increases. We must specify which arrangements count as indistinguishable, and as we shall see, this is the role that the notion of adaption plays in P&B's argument.

P&B's biological mistake is that it is not the complexity of language or its organs that is at issue, but the *increase* in complexity from the ancestral state. How many independent constraints and anatomical reorderings took place in the change from hominids without language to those with the minimal structure that is said to define natural language? The evolutionary fact is that Broca's and Wernicke's areas were recruited from regions in the primate brain that subserved some of the same functions necessary to language although they were not in themselves linguistic, such as the essential distinction between self-generated sounds and those made by others. How much increase in complexity was involved in this recruitment, and was it incredible without design? Because analogies play such an important part in P&B's presentation, let's try one. The definitions of complexity they accept will certainly rate a basin of water at rest much less complex than the stream that results when it is tipped out. Indeed, no one can write the equations for that stream, and even computer simulations of hydrodynamic flow will only produce a generic description. Yet the physical operation that produced the stream from the still basin was trivially simple. Because of bifurcations in the solutions of relatively simple nonlinear systems, one can produce a lot of complexity out of nothing rather easily. Many crystals and precipitates were once thought to be the remains of pre-Noachic creation.

But complexity is not all. It is adaptive complexity "where the details of the part's structure suggest design to fulfill some function" that is at issue. The argument from design, however, is tricky. How can we decide whether the function preexisted the form, or is a purely tautological redescription of it, as when a lump of clay perfectly fills the space it fills. Two of the escapes taken by P&B – the fact that new functions need not be invented for each organism and the existence of convergent evolution of independent forms – are irrelevant because natural language, as far as we know, has happened only once in evolution. The third, that the faculty of language can be related plausibly to reproduction and survival, completes the confusion of might with must and comes close to making the entire argument circular. Of course, the language faculty might have increased the survivorship and reproduction of its possessors relative to others, so it might have been selected. But was it? We know nothing about the political economy of our prelinguistic ancestors. If it was anything like present-day hunters and gatherers there was a great deal of sharing of resources and danger. To give the linguistic faculty a selective advantage, one has to make our ancestors into competitive, individualistic Pleistocene bourgeoisie. Of course they may have been.

For P&B, this seems to be unproblematic. They know:

"In sum, primitive humans lived in a world in which language was woven into the intrigues of politics, economics, technology, family, sex, and friendship and that played key roles in individual reproductive success. They could no more live with a Me-Tarzan-You-Jane level of grammar than we could."

Really? When I saw those movies, they seemed to be doing O.K. in the politics and the sex departments. If we assert that the language faculty did, in fact, give a selective advantage, then we assert what we started out to demonstrate in the first place and all the talk about complexity is beside the point. If it was selected, it was selected.

The problem is that P&B have an incorrect view of form and function in biology that is a carryover from human artifacts and intentions. The "functions," "problems," and "environments" of organisms do not preexist them. They are created in the actual evolution of the organisms. Flying is not a "problem" for a tuna (although there are "flying" fish) and swimming is not a "problem" for a thrush (although there are penguins). As the old song has it, "fish gotta swim and birds gotta fly." And, so it seems, "people gotta talk." At what stage, if any, in the evolution of the language faculty, communication by speaking and listening became a significant evolutionary "problem" so that natural selection may have started to operate on it, we cannot know. In part, the answer depends on contingencies of the particular order of events. The explanatory reconstruction of the origin of the camera eye by natural selection requires a particular ordering of light receptor and ennervation first, followed by lens, followed by focusing distortion of the lens and iris diaphragm. The reverse order would not work, if every stage was to be an improvement in vision. Is there an unambiguous ordering for the elements of natural language? Did we have to have them all at once, in which case the selective theory is in deep trouble?

And finally, to repeat my first problem, how much change in the brain really had to take place to make linguistic competence, and how many independent neurodevelopmental changes were needed? Does anyone know? The fact that they do not and often cannot know the basic facts on which theory rests does not seem to deter academics from presenting speculations as if they were well founded. Fish gotta swim, birds gotta fly, people gotta talk, and academics gotta write.

"Not invented here"

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It is refreshing to see Pinker & Bloom (P&B) adopting some of the major premises of my 1984 book: (a) that human linguistic ability evolved by means of Darwinian processes, (b) that the biological substrate for human linguistic ability is subject to the constraints of biology, in particular, variation, and (c) that data from psycholinguistics, anthropology, neurophysiology, and so forth, are germane. However, P&B still carry much of the baggage of the MIT School of Linguistics, in particular, that guiding principle, "Not invented here."

It is apparent that language equals grammar equals Chomsky's hypothetical universal grammar (UG) to P&B. However, there is more to language than syntax. Human linguistic ability involves a number of biological components that have different evolutionary histories. Although speech and syntax appear to be unique, derived properties of modern *Homo sapiens*, chimpanzees clearly have the ability to acquire and use a limited number of words (Gardner & Gardner 1984) – as would the earliest hominids. P&B accept without question a number of premises that are not universally accepted, for example, that pedagogy is a species-specific human trait: Boesch and Boesch (in press) demonstrate that chimpanzees instruct their young in the use of stone hammers. P&B likewise accept the premise that the linguistic input to a child is so disordered that a powerful UG must be present to guide the acquisition of language. Their argument rests on the presumed absence of negative information and the supposed inadequacy of general cognitive processes, such as associative learning. Self-generated "negative" information, however, is always present through the process of imitation. As Meltzoff (1988) demonstrates, humans, not cats, deserve the appellative "copy cat." If we followed the current linguistic fashion, we might postulate a universal clothes grammar to account for the way that people continually dress in the latest fashion without overt negative corrective information.

P&B now posit a UG that incorporates genetic variation – that's a step forward, but they don't have to rewrite history. Their remarks concerning my critique of UG (Lieberman 1989a) are irrelevant as I was arguing against a UG that *did not take account of genetic variation.* Present UGs – for example, the one proposed by Dresher (1989) to account for the acquisition of the stress patterns of English – consist of a set of rules and parameters that are so tightly interlocked that the absence of any single bit of putative genetically coded data will make it impossible for the child to acquire the target grammar. The UG is furthermore identical for all human beings. At the conference at which Pinker and I debated this issue (my paper is referenced by P&B as Lieberman 1989), Pinker's first rejoinder was to the effect that all people have a heart and a nose that carry out certain functions; they therefore all have a UG that allows them to acquire language. The answer to this common argument is as plain as the nose on one's face – noses vary. A biologically plausible UG has to take account of genetic variation; though some aspects of the UG might be highly buffered, coded in a manner that minimized variation, the problem facing linguists is to determine the nature of the pattern of variation. Moreover, to the extent that the UG, as P&B now propose, becomes a menu from which the grammatical features of languages as different as English and Warlpiri are selected, such general cognitive principles as associative learning and imitation, become more important.

Again, there is no need to rewrite history concerning Chomsky's views on modularity and evolution. My remark that the "current standard linguistic theory" (sect. 5.2.3, para. 1) would demand a sudden saltation follows from Chomsky's claim that human linguistic ability, "like the capacity to deal with the number system . . . is not specifically "selected" through evolution, one must assume . . . even the existence of the number facility could not have been known, or the capacity exercised until human evolution had essentially reached its current state" (Chomsky 1980, p. 3). A strict modular theory of mind is the premise that causes this theoretical problem. Because Darwinian natural selection involves small incremental steps that enhance the present function of the specialized module, the evolution of a "new" module is logically impossible. Chomsky has not considered the Darwinian mechanism of preadaptation, whereby an "organ originally constructed for one purpose . . . may be converted into one for a wholly different purpose" (Darwin 1859, p. 190). Despite claims to the contrary by Piattelli-Palmarini (1989), Charles Darwin was not a fool and he realized that abrupt transitions occur in the course of evolution.

In this respect, P&B seem to be basing their view of Darwinian evolutionary theory on Gould's (1987a) lecture at MIT. They apparently believe that natural selection is a mechanism that operates independently of "spandrels" (Gould & Lewontin 1979). They might instead read Darwin (1859). According to the "spandrel" theory, the evolution of language derives from a preadapted neural mechanism that became the basis for syntax *without* further natural selection for syntax (Gould 1987a). The burden of proof is on Gould because natural selection for a "new" behavior always occurs after preadaptation. Darwin (1859) introduced the concept of preadaptation (Gould's spandrel) to account for the transition from aquatic to terrestrial animals. The preadaptive starting point in the evolution of the lungs is the swim bladder, but natural selection for respiration has resulted in a respiratory system that is quite different from swim bladders. A simple test to determine whether the lung-spandrel was unmodified would involve opening your mouth underwater.

I have proposed that the preadaptive basis for the brain mechanisms underlying human syntactic ability is motor control *for speech* (Lieberman 1984; 1985; 1989; 1989b; 1991). The data that support this claim include clinicopathologic studies of aphasia which show that lesions that interrupt brain circuits regulating speech production result in agrammatism (Stuss & Benson 1986), recent studies of basal ganglia diseases that demonstrate similar linked speech production and syntax deficits (Lieberman et al. 1990), and comparative data on chimpanzees who lack the ability to produce the complex voluntary maneuvers that underlie speech and the similar rule governed operations that underlie complex syntax. It is therefore ironic to read P&B's statement that "we find it ironic that arguments that are touted as being "biological" do not take even the most elementary steps to distinguish between analogy and homology . . . Lieberman's only evidence is that motor programs are hierarchically organized and serially ordered (sect. 5.4, para. 6)."

Language evolved – So what's new?

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I find little to disagree with in Pinker & Bloom (P&B). Indeed, I have discussed these same issues at length myself nearly a decade ago – with some strikingly similar conclusions (Limber 1977; 1982). A century after Darwin's *Descent of man*, it is about time to stretch beyond ad hoc rationalizing of the fitness virtues of this or that feature of language. We must accept the fact that not *every* component of an intricate system need have been directly synthesized just for the functions of that system and we should get on with modeling the evolutionary dynamics that resulted in human language. After a somewhat slow start fending off a curious band of antiselectionists, P&B begin to do this in section 5.

Everyone accepts the fact that human language is comprised of a number of components. Some of these, and their constituents, surely had an independent evolution apart from language whether one focuses on anatomical features such as the larynx or conceptual/semantic structure. Should it be a surprise in hindsight that language "emerged" as a joint product of existing structures put together in a unique fashion, along with perhaps relatively minor structural innovations? Subtle genotypic modifications can certainly have "saltatory" phenotypic consequences – especially in concert with environmental variation. These "minor" structural innovations, of course, can have enormous functional consequences.

In my own reflections on language origins I have tried to model the process using whatever information and speculative tools are available, including the application of the "Baldwin effect" and game theory, both mentioned briefly by P&B. These warrant greater attention by everyone, for they offer a resolution to the traditional conflict between "conventional" and "biological" origin fables – a conflict somewhat ignored by P&B, who seem predisposed to focus on language as biology. The point of looking at game theory and the Baldwin effect, however, is that in an evolutionary perspective – unlike a synchronic perspective – the distinction between biology and culture is obscured.

Game theory suggests how conventional behavior might evolve without a preexisting language as it has in a variety of animal rituals, whereas the Baldwin effect (genetic assimilation) provides a means for cultural artifacts – language conventions, for example –to become part of our species genetic heritage without any of the Lamarckian assumptions prevalent in the nineteenth and early twentieth centuries.[1]

Baldwin (1902) argued that adaptive acquired behaviors that were passed culturally from generation to generation might be assimilated into the nervous system exclusively by means of natural variation and selection. No counterfactual Lamarckian implications about the modification of germ cells were necessary. This was scarcely different from Darwin's own analysis of selective breeding implanting human culture in domestic species.

Applied to language origins, this process can be easily modeled in a contemporary "neural net" metaphor in which randomly varying initial states closest to the culturally acquired norms attain them first and hence are more "fit." Thus, at some point the population initial states will more or less approximate the cultural norms or –as is necessary given the variety of human languages – efficient inductive aids to attaining those norms. The network metaphor here helps reinforce the idea that I'm not talking about "hopeful monster" mutations here, only population modifications in neural connectivity, thalamic projections, dendritic spines, and the like. Space and ignorance preclude pursuing this in any detail, however, but several gaping holes remain to be plugged.

Why didn't the network converge on a uniform human language? P&B (sect. 3.3) address this issue to some extent as I have, pointing out the utility of cultural variation and, more important, the role of culture as a large capacity external memory device. There is also an even more important answer that speaks to the origins of syntax. P&B come close to this in their discussion of "parity" (sect. 3.4.2) but there is a related, yet more fundamental issue that comes out of this – a rationale for the autonomy of syntax as a format or "packaging" device that interfaces mind with phonology.

Syntax is autonomous because the same formats can be used to convey an unforseeable diversity of semantic content in coordination with lexicalization of content. This is critical if highly dependent young are going to rapidly acquire the culture of their society. Cognitive complexity bears very little relation to syntactic complexity. One can express the most complex conceptual message imaginable with the simplest syntactic structure if that content is packed into a few morphemes. The earliest complex syntactic structure in three-year-old children differs little from that of adults (Limber 1973). From another perspective, a parser adequate for those same children would need only minor upgrading in order to deal with the full range of adult sentences (Limber 1976). Syntax and phonology thus function so as to allow young children to learn the form of the adaptive "messages" of their society long before they have developed the cognitive structures necessary to understand most of those messages. The nature of this format varies in different languages as varying basic conceptual elements are pressed into syntactic service to convey universal grammatical relationships. The real problem here, like most of evolutionary problems, is not that we don't have good ideas about how evolution works: We just don't know enough about the processes of language acquisition and language change – we don't know what we want to model!

What about the origins of recursive syntax? I can only agree with Darwin that only the most general conclusions concerning mental evolution will ever be found. Consideration ought to be given, however, to the possibility – improbable as it may have been – that one or more individuals essentially invented language as we know it, factoring or restructuring a primitive message list into abstract, functionally equivalent recursive patterns, which then became part of our biology as sketched above and in more detail in Limber (1982). Who can say it wasn't just so?

NOTE

1. Few today realize that many scientists of that era, including Darwin, Freud, J. B. Watson, and Piaget – but notably not James Mark Baldwin – had some Lamarckian learnings despite strong evidence to the contrary beginning about 1885. This meant that the distinction between conscious rule and species instinct was just a matter of a few generations for a highly adaptive behavior. Lamarckism was part of a progressive political ideology in addition to a biological theory. I suspect that the disproof of biological Larmarckism with its progressive potential for change was one of the threads underlying the rise of behaviorist ideology in American psychology and comparable foolishness in the Soviet Union.

Adaptive complexity in sound patterns

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These comments summarize some recent work showing that both form and substantive aspects of phonetic structure can be seen as adaptations to universal constraints on the production and perception of speech and are likely to have been shaped by processes of variation and selection fully compatible with the mechanisms postulated in current evolutionary theory. Although our discussion focuses on sound systems, it strongly reinforces Pinker & Bloom's (P&B's) general claim that language evolved by natural selection.

First, some cross-linguistic observations. Phoneticians have traditionally applied two main principles in constructing universal phonetic alphabets such as the IPA (Ladefoged 1987): (i) Within a given language, variants of a given phoneme are represented by a single symbol; and (ii) across languages, the same symbol is used for physically similar sounds. It is remarkable that for the languages analyzed so far this procedure has not produced a chaotic collection of sound types but seems to converge on a small set of phonetic dimensions. For instance, all languages have a core set of consonants similar to the small systems of many Indo-Pacific languages. With minor variations, such sets are also included in larger systems, e g !Xũ with its 95 consonants, about half of which are clicks (Maddieson 1984). A large percentage of languages use five vowel phonemes. In more than 90% of a large typological sample (Crothers 1978), the values of those five vowels were /i e a o u/. Maddieson (1984) lists more than 200 different vowel segment types in his survey of 317 languages. In how many ways can we randomly select five vowels from this set of 200? The answer is: $200!/(200-5)!*5!$. In other words, in an incredibly large number of ways.

Viewing the contents of the world's sound inventories from this combinatorial viewpoint, we should be struck – not by the numerous and exotic mechanisms invoked – but by the strong convergence on highly similar phonetic substance. A comparison of speech and nonspeech sounds makes the same point. The human speech mechanism is capable of making a wide range of articulatory gestures and noises that never find their way into phonologies. Against the background of the total sound-producing capabilities of man (Pike 1943; Catford 1977), it is clear that languages underexploit the possibilities in principle available for phonological contrast (Lindblom 1989c). Why this convergence? What are the constraints?

Another striking fact about sound systems is the combinatorial use they make of their discrete segmental and featural components. For instance, Alawa uses five places of articulation for stops. These five places are combined to form three complete series: oral, prenasalized, and nasal stops. Sui has a three-by-four system of nasal consonants: Three source mechanisms: voiced, voiceless, and glottalized, and four places: bilabial, dental, palatal, and velar. Such tightly packed matrices are typologically very general. Languages apparently tend to obey the principle of "maximum utilization of the available distinctive features" (Ohala 1980). Why not a more diverse exploitation of feature possibilities? And where does such a principle come from?

Next, let us consider the problem of deciding whether the above-mentioned phenomena – the contents of phonetic systems and the phonemic coding of lexicons – could have evolved by the mechanisms postulated by current evolutionary theory. How do we support, or refute, the claim that sound patterns evolve by natural (and cultural) selection? What is the structure of the argument? Our answer takes the form of an analysis-by-synthesis approach illustrated by a series of computational experiments that phonetically implement a Darwinian variation-selection framework (Lindblom 1984; 1988c; 1989a; Lindblom et al. 1983; in preparation). Our findings suggest that many phonological patterns can be naturally explained within such a framework and that both substantive and formal aspects of sound structure should be seen not as arbitrary formal idiosyncracies of an autonomous language module, but as adaptive responses to selection pressures originating in universal constraints on the production and perception of speech.

Conceptually this computational scheme is a "source-filter" theory. It exemplifies a research agenda structured in three general steps: (1) Define the search space, that is, the pool of phonetic variation from which selections can be made. (The task here is to provide an independently motivated quantitative specification of notions, such as "possible vowel," "possible syllable," and so forth.) (2) Define performance constraints, e.g., "discriminability," "articulatory complexity," and so forth. (The goal here is to quantify the adaptive value of sounds and gestures, again anchoring definitions in independently known facts and principles.) (3) Define the criteria that (sufficiently) optimal sets of speech sounds should meet.

Let us mention two main results: The model successfully predicts significant aspects of the contents of the world's vowel inventories (Lindblom 1986). It suggests that vowels evolve so as to be easy to discriminate and that the selection of vowel qualities is controlled by a demand for "sufficient discriminability" implicitly imposed by the on-line processes of lexical access (Lindblom 1989b). The model also sheds light on the origins of the phonemic principle by revealing the conditions under which discrete structure and combinatorially organized patterns emerge from a continuous phonetic space (Lindblom 1984; 1989a; Lindblom et al. 1983; in preparation). The growth of a small lexicon was simulated by sequential selections from a universal set of holistically specified syllables. During the course of the experiment, a process of self-segmentation was observed imposing a phonemic organization on the selected syllables. This effect was found to be an *emergent* consequence of the vocabulary expansion and was initiated by optimizing intersyllable discriminability.

The strength with which positive or negative conclusions about natural selection (and alternative mechanisms) can be made is critically related to the extent to which the assumptions of models can be anchored in independently established facts and principles. In the phonetic simulations just reviewed, we derived the search space with the aid of a physiologically based model of articulation and the measure of discriminability from acoustic theory and psychoacoustic experimentation. The model was accordingly developed from assumptions that have their motivation in independent research not primarily concerned with language. Hence, it provides more than a mere "just-so" story.

This conclusion illustrates an advantage that phoneticians may have over researchers in syntax and semantics with respect to the relative accessibility of evolutionarily relevant information. For example, it is interesting to note that, within phonetics there is a paradigm of "experimental phonology" and "phonetic explanation" (Diehl 1991; Ohala & Jaeger 1986) aimed at accounting for the origins of sound patterns, whereas, in syntax, researchers like Chomsky seem to despair of finding precursors and preadaptations of grammatical structure and programmatically abandon the search for them.

Are the present examples of adaptations in sound systems marginal cases? Despite our preceding conclusions, is most of phonological and grammatical structure nevertheless, like Gouldian spandrels, idiosyncratic and beyond functional explanation? How can we ever hope to find the answer to such questions without first exhaustively testing the most comprehensive and successful theory of adaptive complexity proposed so far (Dawkins 1983)? Pursuing that agenda poses serious problems for the student of language because, as suggested above, the adaptationist argument cannot be made convincingly and rigorously unless the functional selection pressures and the space in which they operate are understood on independent grounds. Obviously, the temptation to bypass such mechanisms is enormous in the case of language. Nevertheless, for methodological reasons there can be no short-cuts. Dismissing the neo-Darwinian framework is prejudging the issues. Pinker & Bloom are to be congratulated for stating them so clearly.

Causal stories

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The principle problem with Pinker & Bloom's (P&B's) argument concerns the details of their biological story. To help clarify things, I will consider several sketches of ways that language could have developed.

Story 1. Each of the major linguistic universals that make up universal grammar (UG) is internally represented in the brain. Each universal serves to improve the fitness of individuals (by improving linguistic competence) and each has been selected for. There may be some pleiotropy but, for the most part, each element of UG is determined by a pair of alleles at a single locus.

Story 2. Universal grammar can be thought of as analogous to an organ (Chomsky 1980a). Gradually, bit by bit, this organ has evolved by natural selection, as "small mutations" have produced changes that contribute to the development of a more complex (and better adapted) UG.

Story 3. A genetic change that affects rates of development produced a large-scale morphological change in our ancestors. Through a process of neoteny (retention of juvenile traits in adulthood), for example, these large-scale changes resulted in a large increase in the size of the brains of our ancestors. With an enlarged brain, several things become possible, including a rudimentary language. Natural selection acted to "modify" this ability, gradually leading to the modern UG.

Story 4. Neoteny produced enlarged brains in our ancestors, which allowed rudimentary language, along with many other capacities. Although these capacities are not all to be understood as results of an underlying general learning model (i.e., modularity is true) these capacities are interrelated in such a way that changes in a part of the brain that affect one capacity will typically have effects on other capacities. Natural selection leads to modifications of the brain, but what is selected is the whole package of interrelated capacities. Eventually this results in us, with UG as one of the features (capacities) of our brains.

Story 5. The process of neoteny produces an enlarged brain. Selection for increased brain size (for reasons having nothing to do with language) eventually results in a brain large enough for language use. Most of the features of the brain responsible for language acquisition, including UG, are simply a byproduct of an enlarged brain.

Note that all these stories are assuming modularity (which Gould might not be assuming). P&B can be seen as arguing that story 5 is implausible – that something as functional and complex as UG could not be completely an "epiphenomenal spandrel." They have no evidence that delineates stories 1 – 4, however. The first might be rejected as implausible, but stories 3 and 4 leave open the possibility that natural selection plays only minor "modifying" role in the evolution of language. Story 4 goes even further and leaves open the possibility that what was selected in the process of modification was a complex interacting structure of the brain and not merely a language acquisition device.

P&B's tendency to want to label stories like 3 and 4 as cases where natural selection is "the cause of evolution" is a misunderstanding of the processes at work. Gould has rightly emphasized that in such cases (or in cases like exaptation) the typical view of natural selection as *the* cause will give a misleading account of the actual processes at work. If P&B are merely arguing that natural selection is likely to have played at least some role in the development of language, there is some merit to their argument. It is not clear that Chomsky and Gould are opposed to such a view, however. The view that natural selection is the principle agent in producing the physiological structures ultimately responsible for language acquisition, as in story 2, is a stronger claim, and one not warranted by their argument. There is simply insufficient evidence to choose among options 2, 3, 4, or other stories that can be told.

Middle positions on language, cognition, and evolution

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This is a very rich paper, with much room for commentary. Because space is limited, I would like to concentrate on two matters: (1) how Pinker & Bloom (P&B) arrive at their position by assuming a "golden mean" stance on the relation of language to general cognition, and (2) the particular ramifications of cross-linguistic variation. To be as boring as they claim they are, I agree with them very broadly on both positions they appear to take (with some variation in particular views, no doubt).

One's view of language shapes one's views of specificity of innate endowment. It seems apparent that Gould and Chomsky take distinct and opposite positions on this matter, both positions being closely related to their final stance. Chomsky has always focused on those aspects of language that are unlike anything else. This focus has always been part of his position that the central properties of syntax cannot plausibly be tied to, explained by, or derived from nonlinguistic properties of the mind, and thus require a specific language "module" for their proper explanation. When this focus is central, it is obviously not reasonable to ask how language might be derived from other systems, in part, by processes of gradual evolution, or how the properties of language might stem partly from the adaptive shaping of its ability to translate such properties in a communicative situation. Gould, on the other hand, clearly takes the position that language falls out very neatly from the conjunction of other already existent parts, and requires no specific processes. This, noncoincidentally, was the position of the very strong cognition-functionalist advocates in language acquisition research in the 1970s: Language had strong ties to general cognitive and communicative functions and could accordingly be explained by general cognitive processes (Cromer 1976; 1988).

P&B's position thus represents a delicate balance. On the one hand, they argue that language does show molding by its function in communicating propositional thought and its use in communicative situations. Thus, it could fit in a general adaptive evolutionary picture. On the other hand, P&B argue that language does have unique and sometimes arbitrary design features, which children do learn in robustly stable and largely error-free ways. This counters the position that language is learned by general thought processes.

This seems to me a reasonable general position. There is no guarantee that it is correct, however. In a universe that already has such odd features as material objects that have phenomenological consciousness (unless I'm the only one, as the usual caveat goes), it would not be surprising if, in the end, language turned out to have some of its own emergent peculiarities. But about this sort of possibility, nothing certain can be known in advance; at the least, P&B have done very well to fit language into a gradual adaptationist evolutionary view.

I am also in general agreement with P&B about some of the ramifications of what is known about cross-linguistic variation. Here one can make the case that variability in the lexicon could be adaptive (it allows new words to be entered), but P&B can really find no such adaptive function for variability in grammar. They take two sides again. First, they agree that there is some variation, and a likely explanation of this is that the "design" for grammar is not thoroughly worked through innately; some of the final product is left open, perhaps such that general learning processes operate with some latitude. On the other hand, P&B note that there is still some limitation on which properties of structure and meaning languages draw from. I would dwell a bit more than they do on the extent of variation, or lack of specific design in some features. For example, both agent-patient relations and definite-indefinite meaning (exemplified by the difference between "the" and "a," or "it" and "one" in English, for example) can control morphology, syntactic constituent order, or tone in various languages; tense-aspect marking can also be read as doing so, depending on the language. There really is some considerable variation. On the other hand, as P&B note, a limited pool of structural devices and semantic properties appear in combination with each other. These recombinations can give rise to unusual systems like the Tagalog grammatical relational system, which combines properties of subject-object languages, ergative languages, and topic-centered languages in a unique yet coherent fashion (which children acquire without any serious known difficulties). Some sort of balance between a limited choice of central properties and a somewhat variable combination or choice among them again seems reasonable (see Maratsos, 1989, for extensive discussion of Tagalog, in particular, in this light).

This kind of view probably has very high explanatory value for some other system that a straight "programmed-all-the-way-down" view treats only awkwardly. Consider noun declensional classes, which may number up to 17 in a given language. No nativist theory has claimed them as candidates for innate knowledge; furthermore, it is difficult to see why special biological adaptations would have evolved just for their construction. They do, however, share important organizational properties with more central formal categories such as the form classes (e.g., verb, adjective, or noun (Maratsos & Chalkley 1980). The most sensible explanation, I believe, is that noun declensional systems arise from a combination of historical accident and the operation on such accidents of the same systems that are used in part to construct major form class categories. A "through-designed" system, in which all analytic processes were completely targeted as to their analytic domain and possible outcome, has no good general explanation for the existence of categories like noun declensional categories. Allowing a mixture of targeted properties and processes with more open-ended ones is more promising.

Overall, P&B seem to have made an excellent contribution to the current stock of ideas on language and evolution. My own guess is that, at a general level, language has many of the mixed features that P&B (and others, recently) point to. This approach may lead to a more plausible evolutionary view of language and language acquisition.

Natural selection and the autonomy of syntax

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Because I find the general thrust of the Pinker & Bloom (P&B) target article to be compelling, this commentary will be devoted to further exploring the consequences of their hypothesis that the language faculty was shaped by natural selection. In particular, I will speculate on the phylogenesis of the central construct of modern linguistic theory: the autonomous grammatical system governed by principles that are not derivable from or reducible to concepts outside the system (see Newmeyer, in press, for a more extensive discussion).

Let us begin with what linguists of all persuasions agree is the task of any linguistic theory, namely, to relate sounds and meanings (perhaps "expressions" would be a more appropriate term than "sounds," so as not to exclude signed languages). Because humans can conceptualize many thousands of distinct meanings and produce and recognize a great number of distinct sounds, one's first thought might be that this relation could be expressed in large part by a simple pairing of individual sounds with individual meanings. In the domain of lexical meaning, no such one-to-one pairing exists, of course; a vastly greater number of words can be stored, retrieved, and used efficiently if sequences of a small number of distinctive sounds are paired with meanings than by a direct mapping between individual meanings and individual sounds.

But what about *propositional* meaning, where the question of a one-to-one pairing is rarely, if ever, raised? The infinity of possible messages that can be conveyed cannot in and of itself be the explanation; although humans can formulate an indefinite number of propositions, we can also produce and perceive an indefinite number of sound sequences. Thus a one-to-one pairing between the two is at least within the realm of logical possibility.

The most plausible answer is that sound and meaning are too different from each other for this to have ever been a *practical* possibility. Meanings, whatever their ultimate nature, are first and foremost *mental* realities, with no obvious physical instantiation. Sounds, physical realities par excellence, are produced by a coordinated set of articulations in the vocal tract, under the control of a very different area of the brain from that responsible for meaning.

Furthermore, in the conceptual structures that represent meanings, temporality and linearity play no role. Such structures do, however, contain diverse types of hierarchies and structured relationships: predicate argument dependencies, and relations of inclusion, implication, cross-classification, and identity. Moreover, conceptual structures are discrete; in the representation of a sentence like *the girl threw the ball,* for example, *girl, threw,* and *ball* do not grade continuously into one another.

Phonetic representations, on the other hand, have almost none of these properties. A phonetic representation is temporal and quantitative. Although it is partly hierarchical, there is no direct relationship between the hierarchy of a phonetic representation and that of a conceptual structure. Indeed, the articulatory gestures, formant frequencies, tone patterns, and so on, relevant to phonetics have nothing in common with the properties of a conceptual structure. This mismatch is alleviated only slightly by appealing to phonological representations instead of phonetic ones.

In other words, a major, necessary evolutionary step toward vocal communication was the development of an *intermediate* level between sound and meaning, a "switchboard," if you will, coordinating the two. Only at that point could propositional meanings be conveyed with any degree of efficiency.

What properties might we deduce about this intermediate level? First, it would have to contain a small number of basic units. No advantage would have been conferred by the development of a *third* level, with thousands of basic entities. Second, this level would need to share some properties with conceptual structures and some properties with phonetic representations, but it would have to be constructed out of units common to neither. Communication (and its benefits to the species) would not have been facilitated if this level had been skewed too much either to the sound end or to the meaning end of the spectrum.

What we have just done, of course, is to deduce the selective advantage of autonomous syntax! This level contains a small number of basic units (no more than a couple of dozen syntactic categories are postulated for any given language), which are related to each other by the simple notions of "dominate" and "precede." In this way, a syntactic representation contrasts markedly with the complexity of a semantic or phonetic one. Furthermore, a syntactic representation shares some properties with the former (hierarchy, dependency) and some with the latter (linear sequencing), yet is governed by a calculus that is neither semantic nor phonetic.

The emergent syntactic level drew, in particular, on conceptual structure. Indeed, if Jackendoff (1983) is right that every major phrasal constituent in a sentence corresponds to a conceptual constituent in the sentence's semantic structure, then the influence of conceptual structure on syntactic representations was profound. But the fact that syntax evolved to coordinate this former level with the vocal output channel led to other, and sometimes conflicting pressures on its design features. In particular, because concepts have to be expressed in real time and by means of a vocal tract exapted from structures originally evolved for respiration, olfaction, and digestion (and thus not in any sense "perfected" for communication), a second set of forces contributed to the shaping of syntax. In particular, there arose many conflicts between the demand that it "fit" well with semantics (which would favor a one-to-one match between concepts and syntactic categories) and the demand that it feed smoothly into the expressive plane (which would favor structures designed for ease of production and perception). The resulting level, as a consequence, came to mirror neither perfectly, but rather developed its own distinct set of governing principles.

In short, from the functional pressure favoring the development of a workable system of communication (i.e., from pressure to pair sounds and meanings efficiently) and with it the reproductive advantage that this ability to communicate would confer, autonomous syntax arose in the course of language evolution.

The genome might as well store the entire language in the environment

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For a nongeneticist, the most salient feature of this rich and complex target article is the evidence of a tension between two mutually exclusive conceptions of language. On the one hand, Pinker & Bloom (P&B) appear to subscribe to a view of language as a communicative code, inherently dependent on the existence of conventions shared among a group of people, an interpersonal rather than a private system of knowledge (sect. 3.1). On the other hand, they also see language as a genetically fixed, individually owned property of an organism (sect. 1, para. 2). Not many have tried to hold on to these two conceptions simultaneously – Chomsky (1975), for example, has achieved cognitive equilibrium by claiming priority for the intrapersonal rather than the interpersonal functions of language, a solution rejected by P&B (sect. 3.2, para. 4).

This tension leads to several ambiguities and inconsistencies in the evolutionary theory presented by P&B to the extent that it actually consists of two mutually contradictory versions. In version A, it is language itself that is claimed to have undergone evolution due to natural selection, namely, the sum total of formal grammatical rules regulating the production and comprehension of sentences at a given historical time for a language community and for each of its members. In this version, natural selection started to operate on the human language faculty from the initial grammarless moment, so that all development of language itself is simultaneously a development of the innate grammar (e.g., sect. 5, para. 1; sect. 5.2.2, para. 2; sect. 5.2.3, para. 3).

In version B, what is evolving is the proportion of all existing grammatical rules set genetically in individuals, when language itself is a constant. In this version, natural selection started to operate on a full-blown language and has consisted only of the gradual genetic fixing of grammatical knowledge, so that the parts that have to be learned get smaller and smaller. For example,

> Our suggestions about interactions between learning and innate structure in evolution are supported by an interesting simulation of the Baldwin effect by Hinton and Nowlan . . . For the offspring of that organism, there are increasing advantages to having more and more of the correct connections innately determined, because with more correct connections to begin with, it takes less time to learn the rest . . . Hinton and Nolan confirmed these intuitions in a computer simulation, demonstrating nicely that learning can guide evolution . . . This is consistent with the speculation that the multiplicity of human languages is in part a consequence of learning mechanisms existing prior to (or at least independent of) the mechanisms specifically dedicated to language. (sect. 5.2.3, para. 4–5)

Underlying these inconsistencies is a basic, crucial double message regarding P&B's views on the role, in the developmental history of language, a rule-learning by other than the innate mechanism. On the one hand, a conception of language as a (mostly) genetically set property of the individual demands the minimization of the role of learning grammar by other than the innate mechanism, as the major function of storing grammatical information in the genes is to make the workings of such an acquisition device possible. Some of the text indeed shows the attempt to downplay such nonspecific learning; for instance, in section 5.2.1, paragraph 2, and in section 5.2.2, paragraph 2, transitions from pregrammatical processing heuristics and even "just rote associations" to innate rules are presented as conceptually instantaneous, not progressing through an intermediate stage in which individuals who do not have the correct gene controlling the novel rule nevertheless come to possess such a rule purely on the basis of environmental experience.

On the other hand, a conception of language as an interpersonal medium established by shared conventions demands a flexible learning mechanism sensitive to conventions already established in the environment without any preconceptions about what these conventions may be. Moreover, on such a view, for a private linguistic innovation, including a genetically induced one, to turn into a legitimate part of human language, the rest of the community must have some means of learning the novel component of the code without first undergoing genetic modification. Unless others come to understand the way the innovation is to be interpreted and used, namely, learn the novel rule, it simply does not exist as a rule of the language. A code not shared by others is not a code at all. As Wittgenstein (1953) said, a private language is a contradiction in terms.

The pressure of such considerations led P&B to allocate a central role in version B of their evolutionary theory to such nongenetically backed learning at a historical time prior to the emergence of the innate learning mechanism (e.g., sect. 3.4.2, para. 4; sect. 5.2.3, para. 4–5). As we have seen, according to version B, all the formal rules of language somehow get acquired by man prior to the onset of selection pressure that leads to the gradual inscription of these rules in the genome. Through this move, however, P&B put themselves into the uncomfortable position of postulating a language system that is at the same time learnable from the environmental input if the learners are prehistoric, and no longer learnable when the learners are our contemporaries.

In view of the inconsistencies in the evolutionary model(s) proposed by P&B, it is difficult to see in them a well-developed theory of the operation of natural selection on the language faculty. The question whether or not the evolution of innate universal grammar is the result of neo-Darwinian selection pressures, and indeed whether or not there is an innate grammar in the first place, should be considered as yet not settled. The developmental psycholinguist's best bet is still a research program that does not make the pretheoretical assumption that present-day children have evolved to a degree where they carry knowledge of all possible grammars in their genes. If the language system is amenable to being acquired without an innate mechanism, one of the major motivations for postulating such a mechanism in the first place is removed (Chomsky 1965). Given the intrinsically interpersonal character of language, and the clumsiness of having to store all possible grammars in the genes of all humans, the genome, paraphrasing Tooby & Cosmides's (in press a) wonderful phrase, might as well store the entire language in the environment.

The emergence of *homo loquens* and the laws of physics

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The target article raises a host of important issues that deserve careful attention. For want of space, I will comment only on what I take to be the most central ones.

For Pinker & Bloom (P&B) "there is every reason to believe that language has been shaped by natural selection," presumably meaning "shaped in some respects," because they also assert that "it is certainly true that natural selection cannot explain all aspects of the evolution of language." (sect. 4.2) Most culturally developed people will readily agree. The fossil record, sparse and patchy as it is, seems to leave little or no room for doubting the *fact* that the evolution of the species, including the human species (and in particular the evolution of the human brain), was shaped in some respects by processes of natural selection (cf. Eldredge & Tattersall 1982). Recent analysis of the "molecular trees" suggests that humans and African apes diverged only 5 million years ago, some 15 to 20 million years later than the paleontologists had surmised on the basis of differences of anatomical traits (Wilson 1985).

It comes, then, as a surprise to read, in the same paragraph of the target article, that Noam Chomsky (1988b) and Stephen Jay Gould (1989a) "repeatedly urge us to consider a startling contrary position." Such a conclusion demands some comment. Here, I can only examine briefly some aspects of Chomsky's view, as it appears on the written record, and P&B's interpretation of it.

There are clear indications in Chomsky's writings that he believes that language was shaped in some respects by natural selection. In their footnote 1, P&B themselves give the reference to this statement:

> Over the past centuries or millenia there has been no selectional advantage in an ability to discover the principles of quantum theory, though there is an obvious selectional advantage in an ability to discover the language of one's speech community. (1975, p. 252)

They also quote the following: "Language must surely confer enormous selectional advantage" (1980a, p. 239). The same point is made in other contexts, for example, one that appears in a passage they quote in which Chomsky states that the language capacity "has an obvious selectional value" (1982, p. 19). Still another instance is found in the sentence immediately after one of their quotes (see quote from 1982b below). However, P&B do not take these statements to be representative of Chomsky's position. The reason will become apparent as we proceed.

Let's first consider some other statements that clarify Chomsky's position. A crucial one is the following, written by January 1967 (P&B quote only the sentence in italics, which they rightly offer as representative):

> How did the human mind come to acquire the innate structure that we are led to attribute to it? Not too surprisingly, Lorenz takes the position that this is simply a matter of natural selection. . . .
>
> In fact, the processes by which the human mind achieved its present stage of complexity and its particular form of innate organization are a total mystery, as much so as the analogous questions about the physical or mental organization of any other complex organism. *It is perfectly safe to attribute this development to "natural selection," so long as we realize that there is no substance to this assertion, that it amounts to nothing more than a belief that there is some naturalistic explanation for these phenomena.* . . . With no knowledge of the laws that determine the organization and structure of complex biological systems, it is just as senseless to ask . . . (1972, pp. 97–98).[1]

The word "simply" in the second sentence is of course crucial, as becomes clear when Chomsky spells out what he has in mind (emphasis added):

> It surely cannot be assumed that every trait is specifically selected. In the case of such systems as language or wings it is not easy even to imagine a course of selection that might have given rise to them. A rudimentary wing, for example, is not "useful" for motion but is more of an impediment. Why then should the organ develop in the early stages of its evolution?
>
> In some cases it seems that organs develop to serve one purpose and, when they have reached a certain form in the evolutionary process, became available for different purposes, *at which point the processes of natural selection may refine them further* for these purposes. . . . Possibly human mental capacities have in some cases evolved in a similar way. (1988, p. 167)

P&B appear to express a similar opinion when, for example, they refer to "subsequent modifications and arrangements" (of "structures produced entirely by nonadaptationist mechanisms") that "must be explained by natural selection" (sect. 2.2).

Chomsky's view is then consistent with the thesis that language was shaped in some respects by natural selection. He, however, is somewhat less vague about the scenario than P&B are, explicitly endorsing a well-known suggestion by Jacques Monod. As Chomsky writes, quoting from Monod (1970, pp. 150–51, corresponding to 1971, pp. 136–37),

> it is likely that the evolution of human cortical structures was influenced by the early acquisition of a linguistic capacity, so that articulated language "not only permitted the evolution of culture, but has contributed in a decisive fashion to the *physical* evolution of man"; there is no paradox in supposing that "the linguistic capacity that reveals itself in the course of epigenetic development of the brain is now a part of 'human nature,'" itself intimately associated with other aspects of cognitive function which may in fact have evolved in a specific way by virtue of the early use of articulated language. (1971, pp. 10–11)

Compare now Chomsky's actual position with P&B's rendition of it and with their own position. According to P&B, Chomsky and Gould "have repeatedly suggested that language may not be the product of natural selection, but a side effect of other evolutionary forces, such as an increase in overall brain size and constraints of as-yet unknown laws of structure and growth" (sect. 1.0). I know of no evidence that Chomsky ever (let alone "repeatedly") suggested that "language may not be [in any respect] the product of natural selection," and P&B offer none.

On the other hand, there is plenty of evidence in the target article that P&B assign a much bigger role to the environment than to "evolutionary forces such as an increase in overall brain size and constraints of as-yet unknown laws of structure and growth." This is a position that is not likely to be congenial to the molecular biologist, and is certainly at odds with the approach of Chomsky and other natural scientists, who would balk at the thought of placing pure chance and arbitrariness at the center of all biological processes.[2]

Take an intricate organ such as the vertebrate eye, which P&B offer as a paradigmatic product of natural selection. No one has ever explained how the component parts of the eye, which are so specifically interdependent, could have arisen *separately* and *gradually* (compare their section 2.3.1) by a sequence of independent accidents. It will not do to observe (sect. 2.3.2), quoting from Dawkins, that 1% vision is better than total blindness. The point is that a half-made eye would be of dubious selective advantage; it would, in fact, be utterly useless (see Chomsky's remark about wings above). If so, what are the chances that just the right sequence of purely random mutations would occur in the limited time available so that the ultimate outcome is a successful vertebrate eye? P&B have nothing to say on the matter.

They also fail to mention that in a footnote to a passage they quote from, Chomsky refers to papers by Eden, Schützenberger, and Gavadan (included in Moorhead & Kaplan, eds., 1967, reprinted in 1985) in which it is "argued on statistical grounds – through comparison of the known rate of mutation with the astronomical number of imaginable modifications of chromosomes and their parts – that [the laws that determine possible successful mutation and the nature of complex organisms] must exist and must vastly restrict the realizable possibilities" (Chomsky 1972, p. 97). From this it is not much of a step to "assuming that there is just a fairly small space of physically possible systems that can realize complicated structures" (Chomsky 1982, p. 23), which is quite at variance with P&B's guess that "the space of physically possible neural systems . . . can't be *that* small" (sect. 4.2, para. 3).

"It is no disparagement of Darwin's genius," as Monod writes, "to note that the selective theory of evolution did not take on its full significance, precision, and certainty until less than twenty [now forty] years ago," because Darwin "could not in his day have had any inkling of the chemical mechanism of reproductive invariance, nor of the nature of the perturbations these mechanisms undergo" (1971, p. 24). Similarly, it is no disparagement of present-day neo-Darwinists to note that there are laws of physics (including chemistry, biochemistry, and so forth) relevant to a better understanding of the nature and course of evolution that we still have no inkling about, as we had no inkling of the mechanism of reproductive invariance before 1953. In fact, the history of science suggests that it is reasonable to assume that there are unknown laws of physics, and that some might be relevant to the emergence of perhaps the most complex organ in the universe. Contrary to what P&B repeatedly imply, this is the assumption of working natural scientists and, more generally, of anyone who adheres to the canons of "constructive scepticism" (cf. Popkin 1979, pp. 48, 140; Chomsky 1986, p. 240; 1989a).

It is in any case not uncommon to assume that "evolutionary theory appears to have very little to say about speciation, or about any kind of innovation":

> It can explain how you get a different distribution of qualities that are already present, but it does not say much about how new qualities can emerge. It could be that the answer to the puzzle is similar to the answer to the question of why after the big bang you went from hydrogen to helium, not to something else. It was not because a lot of things were tried and helium worked, but just that that is what the world is like under particular conditions of cooling and so forth. (Chomsky 1982, p. 23; cf. Lewontin 1990)

A few months after this statement was made (either in November 1979 or in March 1980), a not dissimilar view was sympathetically discussed in a "historic conference" that challenged "the four-decade-long dominance of the Modern Synthesis." A brief sample from the report in *Science* may be enough to convey the flavor:

> adaptation, though important, is a secondary factor in shaping species morphology. There are . . . fundamental constraints in morphological possibilities imposed by mechanical properties of the building materials, basic forms embodied in the building blueprint that underlie many related species, and conservative rules that govern embryological development. In other words, organisms of all sizes, shapes, and forms are not possible. (Lewin 1980a, p. 886; cf. Jacob 1982, p. 21)

In particular, there appear to be "basic body plans, *Baupläne*, that are maintained through immensely long evolutionary periods despite dramatic changes in the life patterns of organisms and the functions of their parts" (Lewontin 1990, p. 233).

There are also fundamental constraints at the molecular level:

> Even though natural selection ultimately takes place at the level of the organism, the organization of genes on chromosomes evidently plays a crucial role in determining the variety of genetic variants that are available for natural selection to act on. (Watson et al. 1987, p. 1159)

Given this, one can reasonably speak of "the evolution of new species and their winnowing by means of natural selection" (McMahon & Bonner 1983, p. 9). Put another way, "the answers may well lie not so much in the theory of natural selection as in molecular biology" (Chomsky 1988, p. 167).

It is then highly significant that, in sharp contrast with P&B, Chomsky suspects that "if we understood more about the way organisms are put together, the way they function and the laws they obey, we would find that there are not many kinds of possible organisms" (1982, p. 23). In fact, when someone observed, during the discussion that followed one of his lectures at UCLA (January 26, 1988), that if the number of possible human languages is finite, "a language, say Aramaic, could pop up again later on," Chomsky agreed (if history "goes on long enough," because "these numbers [of possible languages] are astronomical") and went on to make the following point:

> If you went on sort of indefinitely, yes, you'd get the same languages

over and over again. In fact, you'd get the same organisms over and over again. If evolution goes on for ever, you are going to get the same organisms popping up over and over again. Probably. There it's not quite the same story, because you don't have the same finite boundedness, but just on probabilistic grounds it's going to happen, even if there is no finite boundedness. There is nothing surprising about that. Maybe it doesn't sound that natural, but it's no more surprising than the fact that things fall rather than rise.

The observation that "there are not many kinds of possible organisms" is followed by the remark that "There is some work already that goes in that direction" and a reference to Benoit Mandelbrot's *Fractals* [1977], which "is in a way related to things like D'Arcy Thompson's [1917] attempt to show that many properties of organisms, like symmetry, for example, do not really have anything to do with a specific selection but just with the ways in which things can exist in the physical world" (1982, p. 23).[3]

Needless to say, symmetry is not the only topic of interest to physicists, biologists, plant geneticists, or linguists. To mention just one more: It has been said that "in order for an animal to appreciate the joys of literature, it needs to be at least the size of a human being" (McMahon & Bonner 1983, p. 23; cf. Lewontin 1990, p. 230). It is because such joys were nowhere to be found as the hominids were evolving that Chomsky includes also the human literary capacity among "all sorts of special properties that are not individually selected" and suspects that what is selected is structural properties such as "bigger brains, more cortical surface, hemispheric specialization for analytic processing," and perhaps others. It might be, he conjectures, that unknown physical laws

apply in such a way [as to] afford the brains that evolved (under selection for size, particular kinds of complexity, etc.) the ability to deal with properties of the number system, continuity, abstract geometrical space, certain parts of natural science, and so on. (1982b, p. 321)[4]

For someone who approaches the problem with an open mind, natural selection is then not the only possible alternative. In fact,

There is no reason to demand and little reason to suppose that genetically-determined properties invariably result from specific selection – consider the case of the capacity to deal with properties of the number system. They might, for example, arise from the functioning of certain physical laws relating to neuron packing or regulatory mechanisms, or they might be concomitants to other properties that are selected, or they might result from mutation or genetic engineering. . . . It is an open empirical question what if any role experience or phylogenetic development may play. (1980a, p. 100; about regulatory evolution, see Ohno 1972, Prager & Wilson 1975)[5]

It is clear from their remarks that P&B do not find this line of reasoning congenial, and it seems natural to inquire about the source of their scepticism toward what can be expected of physics. To my mind the key is not hard to find. In their discussion of language evolution and language acquisition in section 3.4.3, they rightly rely on Chomsky's work in arguing against functionalism in ontogeny and in emphasizing the contrast between functionalist theories of the evolution of language and functionalist theories of the acquisition of language. However, this well-known contrast is not incompatible with a similarity at a deeper level. We have see that by January 1967 Chomsky was already clear about some questions that are still not widely understood today. Perhaps now it would be easier to see the importance of the part of the passage I left out above:

With no knowledge of the laws that determine the organization and structure of complex biological systems, it is just as senseless to ask what the "probability" is for the human mind to have reached its present state as it is to inquire into the "probability" that a particular physical theory will be devised.

The same is true of a theory of a particular language (the linguist's or the child's). And just as "it is idle to speculate about [language growth] until some indication of what kind of knowledge is attainable – in the case of language, some indication of the constraints on the set of potential grammars" (Chomsky 1972, pp. 97–98), it is just as idle to speculate about the phylogenetic growth of the brain until some indication of what kind of development is possible – in the case of the emergence of the human brain, some indication of the constraints on the set of potential highly complex organisms. Hence, the observation that P&B quote out of context:

In studying the evolution of mind, we cannot guess to what extent there are physically possible alternatives to, say, transformational generative grammar, for an organism meeting certain other physical conditions characteristic of humans. Conceivably, there are none – or very few – in which case talk about evolution of the language capacity is beside the point. The vacuity of such speculation, however, has no bearing one way or another on those aspects of the problem of mind that can be sensibly pursued. It seems to me that these aspects are, for the moment, the problems illustrated in the case of language by the study of the nature, the use, and the acquisition of linguistic competence. (1972, p. 98)

At this point the contrast between Chomsky's research strategy and P&B's strategy should be fairly transparent. Chomsky begins by updating what for him is enduring in the Cartesian tradition. His view is that "if empirically adequate generative grammars can be constructed and the universal principles that govern their structure and organization determined, then this will be an important contribution to human psychology" (1972, p. 71). But this is only the first phase, now in progress. In a second phase, the new psychology (the emerging cognitive psychosciences) may be able to provide the necessary guidance to the cognitive neurosciences of the future – perhaps the only way of making them possible (Chomsky 1989b, p. 462). Conceivably, in a third phase the neurosciences of the future might in turn be a guide to a better understanding of the phylogenetic development that led to *homo loquens,* "an example of true 'emergence' – the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization" – which, Chomsky insists, "poses a problem for the biologist":

Recognition of this fact, though formulated in entirely different terms, is what motivated much of the classical [Cartesian] study of language by those whose primary concern was the nature of mind. (1972, p. 70)

To conclude: The question of the interaction of "nature" and "nurture" is fundamental also from a metaphysical perspective, and the "poverty of the stimulus" argument is equally appropriate. Such a view of evolutionary metaphysics exhibits a degree of parallelism with the deductive approach to epistemology that we associate with Plato and Descartes, which is in sharp contrast with the inductive approach of Aristotle, as Chomsky has made abundantly clear (e.g., in 1975, pp. 5–7). This parting of the ways is what is at the root of P&B's misreadings: Their view, I submit, is essentially Aristotelian. They appear to be oblivious to the fact that "the environment per se has no structure, or at least none that is *directly* assimilable by the organism," in Massimo Piattelli-Palmarini's memorable phrase (1980, p. 10). One cannot seriously claim that the eye (or the liver or the heart) is in any sense just a product of the external environment.

It is for this reason that I find it difficult not to see in their paper, and in particular in its section 5, an unfortunate regression (cf. Otero 1988) to the environmentalism of Rousseau's Hobbesian discussion of the origin of language back in 1755. Even at that time, exactly 200 years before *The logical structure of linguistic theory,* it would have represented a retreat to write that "there must have been a series of steps leading from no language at all to language as we now find it, each step small enough to have been produced by a random mutation or recombination, and each intermediate grammar useful to its possessor" (sect. 5, para. 1). Like Rousseau before them, P&B have lost track of the fact that Descartes' argument for dualism, stemming from the creativity manifested in the use of language, had already suggested a radical alternative. The cultural retrogression is anything but small: We are back at the level of the eighteenth century controversy on the origin of language (cf. Viertel 1966). In fact, chances are that some representative figures of the Enlightenment (Herder, Humboldt, Goethe) would not have been able to read the target article without dismay. Recall that Goethe's "Urform," which is parallel to Humboldt's notion of "organic form" in language – this being "a kind of universal *Urform*" for Chomsky (1966b, p. 10) – "is a kind of generative principle that determines the class of physically possible organisms" (Chomsky 1966a, p. 24). In other words, the "innate organizing principles [of universal grammar] determine the class of possible languages just as the *Urform* of Goethe's biological theories defines the class of possible plants and animals" (Chomsky 1966b, pp. 7–8).

There is a difference between 1755 and 1990, however, that should not be overlooked. In contrast with P&B, Rousseau appears to have surmised that the problem of the chicken and the egg cannot be resolved along Aristotelian lines. He came to realize that he had to abandon "the following difficult problem: Which was most necessary, previously formed society for the institution of languages, or previously invented languages for the establishment of society?" As Chomsky emphasize in his still unassimilated paper "Language and freedom" (January 1970), for the Cartesian there was "no need to explain the origin of language in the course of historical evolution." Rather, human nature "is qualitatively distinct: there is no passage from body to mind." "We might reinterpret this idea in more current terms," he continues,

> by speculating that rather sudden and dramatic mutations might have led to qualities of intelligence that are, so far as we know, unique to humans, possession of language in the human sense being the most distinctive index of these qualities. (1973, p. 396; 1987, p. 147)

In a footnote he remarks that he "need hardly add that this is not the prevailing view." The target article offers abundant proof that along some dimensions the intellectual advance of the last 20 years, at least in some quarters, has not been that spectacular.

ACKNOWLEDGMENT

This commentary owes its existence to Robert Freidin, who also deserves credit for bringing to my attention several places where the original version was in need of clarification (in addition to generously sharing with me some information not generally available). Alan Strozer unwittingly contributed to making the initial steps somewhat easier. Maria Luiza Carrano provided help promptly when it was needed.

NOTES

1. The second paragraph of the quote appears already, almost verbatim, in a paper he presented at the University of Chicago in the winter of 1966 (1966, pp. 30–31).

2. Darwin himself appears to have expressed misgivings. For example, when in 1869 Wallace wrote to him that "Natural selection could only have endowed the savage with a brain a little superior to that of the ape, whereas he possesses one very little inferior to that of an average member of our learned society," Darwin replied: "I hope you have not murdered completely your own and my child" (cited in Restak 1979, pp. 58–59).

3. It should perhaps be noted in passing that the most recent (1990) annual meeting of the American Association for the Advancement of Science included a general session on "Symmetries across the Sciences."

4. Contrary to what P&B appear to imply, this kind of view is far from outlandish. A recent sample may serve as illustration:

> Human cognition may have developed as the purely epiphenomenal consequence of the major increase in brain size, which, in fact, may have been selected for quite other reasons. (Lewontin 1990, p. 244)

5. In an interview in the fall of 1977, he elaborates on the theme (see Restak 1979, 326): "People ask, 'How did humans get the power of speech?' – implying somehow that speech was selected by evolution. There's an alternative explanation possible. It may be, as evolutionary theory informs us, that properties of the human brain were selected by their evolutionary advantage. Along with this may have come other accompanying benefits. This is common in evolution, where adaptations for a particular trait may involve all kinds of other consequences. Certainly no biologist believes that *every* trait is selected for. It is possible that language is best explained in these kinds of terms: a carryover effect appears at a certain order of brain complexity. Or, to put it differently, at a certain level of complexity many of the human brain's most striking capacities may have to do with the laws of physics. They may relate to the density and packing of neurons in the brain, for example. Perhaps there is only one way of physically solving such a packaging problem, and that in turn may lead to certain consequences like speech or the ability to deal with numbers. It might turn out, in fact, that simply as a consequence of physical law, the brain will have language ability, just as a certain molecular organization will eventually result in a crystal. Such an explanation may be just as valid at the level of complex biological structures like the brain, as it is at the level of simple physical structures. There is no reason to assume that we have to switch from physical laws to evolution when we pass from crystals to living organisms." (See Waldrop 1990, a very intriguing report, brought to my attention by Noam Chomsky, about a symposium in early February 1990 on self-organizing systems with many thought-provoking ideas about how "complex dynamical systems can sometimes go spontaneously from randomness to order," a plausible "driving force in evolution.")

It should perhaps be added here that the "significantly spheroidal brain, as opposed to a cylindrically shaped brain," is for Jerison part of "a solution of a packaging problem" (1973, pp. 284–85, 368).

Complexity and adaptation

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Pinker & Bloom (P&B) remark that "natural selection is the only scientific explanation of adaptive complexity." For this dictum to be of use in an argument for natural selection of language, language must be adaptive and complex. This is not enough, however: The adaptive properties and the complex properties must be the same. If a system is both complex and adaptive, but its complexities are unrelated to its adaptive properties, then the P&B dictum will not be usable to argue that language was shaped by natural selection. Furthermore, supposing that language is an adaptive trait must not compel one to suppose that all of its significant features are adaptive. There are problems with P&B's discussion on all these fronts, as discussed below.

Language as a trait. P&B treat language as a single trait whose complex design for some function justifies us in supposing that its evolution can be explained by natural selection. At the same time, they point out that many aspects of language are to be explained not as adaptations, but as arbitrary features that are present because fixing on *some* communication protocol had an evolutionary advantage, even if there might have been far better alternatives. They even argue that there is a "need for arbitrariness" in communication protocols.

Linguists are fond of pointing out how nonfunctional many of the aspects of language that most interest them are. Thus, in allowing for arbitrariness, P&B attempt to accommodate the main objection that linguists have always had to thinking of language in terms of natural selection.

But is there any tension between seeing language as a single adaptive trait and seeing it as a collection of features, some of which are arbitrary? P&B see no problem here. They speak of the arbitrary features of language as being "adaptive so long as they are shared." P&B's reasoning seems to run like this: There is a large space of arbitrary features that human language *could* have had instead of the ones it does have. But alternative possibilities do not make the actual features fixed on by evolution any less adaptive. The other possibilities would have been adaptive, too.

Another way to put their point would be this: Imagine that you need a tubular tool to make holes in the ground to plant seeds. You look around, seeing a pencil, an old nail, and a spoon. You pick one of them at random, say the spoon, and you plant the seeds. Now the choice of the spoon doesn't add any *complexity* to the seed-planting. What calls for explanation by natural selection in the case of language is functional complexity, but the arbitrary choices made along the way don't add any complexity, and so don't weaken the case for natural selection of language.

What we've said so far is exposition. We now turn to criticism. Let us distinguish three degrees of adaptationism.

Lunatic adapationism. The view that *every* feature of a system was shaped by natural selection. No serious contributor to this debate holds this view. To take a trivial example, the features of a system include their exact distance from the Rock of Gibraltar, a property that no one thinks was shaped by natural selection.

Extreme adaptationism. The view that every feature of a system *that an engineer might regard as a design feature* was shaped by natural selection. Here we mean to include considerations about trade-offs and the constraints imposed by the materials at hand as compatible with shaping by natural selection. This is a view that many biologists might take with respect to many biological systems.

Strong adaptationism. The view that many of the most salient of the features of a system that an engineer might regard as design features were shaped by natural selection.

Here is a way of seeing the tension between adaptationism about language as a whole and the concession of arbitrariness of specific features: Though P&B might adopt extreme adaptationism with respect to the eye, they cannot be extreme adaptationists with respect to language. Their commitment to the principle of parity and the need for arbitrariness preclude it.

We have a much more serious problem for P&B, however, one that challenges even strong adaptationism about language. How do P&B know that the arbitrary choices of which they speak don't increase complexity? Suppose that in the evolution of a crablike creature, a change in environment creates a situation in which survival requires faster locomotion. There are many alterations that an engineer might think of, but evolution favors a quick and dirty approach. Thus, a mutant appears that does the trick by putting together already present behavioral patterns into an utterly shambolic combination of rolling, pushing, flipping, and sliding. If this arbitrary combination is preserved, it can lead to further complexities in later evolutionary change, some of which might themselves be adaptive, others not.

The application to language is obvious. The operation of the principle of parity plus the need for arbitrariness can result in the choice of chimerical features of language that inculcate chimerical additions and encrustations down the road. Thus we are led to a question crucial to the whole discussion: How much of the complexity that we see in language is there because of the needs of complex functional design, and how much is a by-product of arbitrary choices? In the next section, we argue that the answer to this question, though hard to come by, seems unfavorable to P&B's selectionist view of linguistic complexity.

Where does language's complexity lie? If Pinker and Bloom are to be understood as having any quarrel with Chomsky and Gould, we must assume that they are claiming that language's adaptive properties are complex, and that its complex properties are adaptive. Without an agreement on what complexity is supposed to be, the claim has not been sufficiently well formulated to be answerable. The only judgments of complexity we know of are those implicit in modern linguistics. We will argue that if these judgments are taken seriously, P&B's claims are probably false.

P&B present "an argument for design in language" in the form of a list, designed to display language's adaptive complexity in the domain of "sheer expressive power," a list "so obvious in linguistic practice that it is usually not seen as worth mentioning." The list is a recounting of some properties of syntactic structures and how they come to be associated with meanings. Thus, P&B note that morphemes and words are categorized and grouped in a hierarchical fashion into phrases that encode linear precedence, dominance relations, and such other patterns as complementation and control. Furthermore, they note our ability to use these categories and relations to express thoughts. Are these properties of language complex? Are they adaptive?

Let us consider complexity first. P&B allow that noncomplex adaptive structures (e.g., an appendage used as a visor) may receive nonselectionist explanations, but they consider such an explanation implausible for an eye. The question for them is whether language is a visor or an eye. But what (nonarbitrary) metric shall we use to decide this matter? We know of none. This fact alone makes it very difficult even to discuss the question.

Although there is no absolute metric of complexity, we might examine the judgment of linguistics on the *relative* complexity of facts about language. Here, too, it is hard to know how to proceed. By the standards that come to mind, P&B's list of properties stand much closer to the visors (and farther from the eyes) than do the properties commonly studied by linguists. Their properties get scant mention in "linguistic practice" because they (unlike the structure of the human eye) are not complex enough to merit much discussion.

What *are* the complex problems? Many examples could be given. Linguists want to know why alongside passive nominals like *the city's destruction by the enemy* we do not find *the city's sight by the enemy*. We want to know why clitic pronouns in French are normally attached to the auxiliary verb in their clause, except in causative constructions, where an intricate pattern of "clitic climbing" emerges. We want to know why *ever* is anomalous in *the claim that John ever left the room was discovered by Bill*, but not in *the claim that John ever left the room was denied*. We want to know why *a glass half full* is the same as *a glass half empty* but a *glass almost half full* is not the same as *a glass almost half empty* (an example from Barbara Partee). We want to know why Russian monomorphemic inflected forms sometimes stress the stem, sometimes the suffix, but bimorphemic inflected forms never shift stress in this fashion (an example from Janis Melvold). It is in these domains that language is judged to be complex and worth studying.

Are the domains that linguists find complex also adaptive? What reproductive advantage is conferred on speakers because they do not fully accept *the city's sight by the enemy*, a slightly abnormal pattern of clitic climbing, or a verb that shifts its stress? What function would be impaired if a speaker did accept *the city's sight by the enemy?* (We are ignoring the reproductive advantage to be gained by conforming with the rest of the local community. Such an advantage is independent of the complexity of the language that is shared.) All or most of the more complex properties of language fall in areas far removed from any currently imaginable adaptive function. In addition, their complexity is unrelated to issues of arbitrariness and parity. Equally arbitrary systems of far greater simplicity are imaginable. Yet the complex properties discussed by linguists seem to be deeply rooted in the innate structure of the human language faculty, detectable in any language whose other properties do not inhibit the discovery of these facts.

By contrast, many of P&B's "facts about substantive universals," "specific arguments for language design," involve properties that are not shared by all languages. Many languages lack one or more of the syntactic categories "exploited to distinguish basic ontological categories." For example, it has been argued that Squamish does not distinguish nouns from verbs. Many languages lack the distinction between adjectives and verbs. Languages are perfectly happy to lack a distinct class of overt auxiliaries (German) or to lack overt determiners (Russian). Languages like Chinese have aspect marking, but no tense marking. Some languages use the same forms for [*wh*]-phrases and indefinite NPs (Korean).

P&B respond to this issue by noting, correctly, that "these variations almost certainly correspond to differences in the extent to which the same specific set of mental devices is put to use, but not to differences in the kinds of devices that are put to use." Their sample list of adaptive complexities is intended to lie in the domain of *expressive power*, however. A Squamish speaker may have the mental apparatus to make the verb/noun distinction, but if he gets on perfectly well without expressing this distinction, it is hard to see how it has a function whose absence puts a creature at a reproductive disadvantage. Thus, although universal grammar unquestionably allows variation in the properties identified by P&B, the very fact of variation shows that these properties do not confer any credible reproductive advantage. Hence, even if they were "complex," they would not be adaptive.

Furthermore, when the complexity of language does not lie in the facts of any one language, it lies precisely in the facts of cross-language variation. But this is in an area irrelevant to individual organisms and hence irrelevant to any individual's reproductive chances. For example, in a recent study of variations among the Romance dialects, Richard Kayne has observed that if a language does not allow a question to be introduced by the equivalent of *whether or not* but does allow embedded questions of the form *Mary wondered whether to leave*, then, in an infinitival clause (but not a finite clause), any cliticized direct object pronoun must follow the verb. Otherwise, the pronoun will precede the verb. Suppose for the sake of the argument that some fact about *whether*, or about the order of verbs and pronouns could confer a reproductive advantage on an organism. Even under these implausible circumstances, the property discovered by Kayne cannot fall under this (imaginary) selectionist rubric. This is because no individual can conceivably benefit from the fact that his grammar falls into a class with Kayne's property, because this property is never displayed in any individual.

One might, at this point, object that the properties we have singled out are epiphenomena of deeper properties of language. These deeper properties in turn *might* show eyelike adaptive complexity. For example, the ability to "refer to an unlimited range of specific entities while possessing only a finite number of lexical items" might confer a reproductive advantage, and the biological means stumbled upon to confer this advantage might entail (as a biological fact) obedience to Kayne's generalization or a particular judgment of a passive nominal.

But if the burden of the debate moves to undiscovered deeper properties of language, the debate ends, for how can we have an opinion about the nature of undiscovered properties? The deeper properties might have evolved for some nonlinguistic function, in which case language would be a spandrel. If the deeper properties are not complex, then they are visors, and no selectionist explanation is demanded. If they are complex, we will have to ask the very question we have just been exploring about whether their complexity is adaptive.

In any case, linguistic theory has not advanced to the point where any of the well-studied complexities can be shown to be adaptive. The achievements of linguistics lie in explaining nonadaptive complexities in terms of more general nonadaptive properties. Thus, for example, Kayne argues that his discovery is related to a version of binding theory that restricts for the distribution of pronouns inside noun phrases. This is an important and fascinating result, if true, but it hardly derives the superficial complexities from the advantages of "communication of propositional structures over a serial channel" or other first principles.

P&B might reply at this point that linguists have simply been looking at the wrong things, and that an investigation of *their* list of properties will reveal complexities worthy of the human eye, vindicating their general program. But, in that case, it is incumbent upon them to display these complexities and to develop a syntax of "the communication of propositional structures over a serial channel" or a semantics of the ability to "refer to an unlimited range of specific entities while possessing only a finite number of lexical items," or a phonology of the "smooth[ing] out [of] arbitrary concatenations of morphemes into a consistent sound pattern that juggles demands of ease of articulation and perceptual distinctness." This sort of linguistics does not exist. In fact, progress in linguistics has been achieved precisely by turning away from the topics that P&B find important and by turning toward precisely those properties of language that look nonadaptive but complex. Perhaps there is another kind of linguistics that will make P&B's case for them, but for now we have no indication of what it might look like.

An ideological battle over modals and quantifiers

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In their remarkably well-written essay, based on a wealth of sources drawn from many disciplines, Pinker & Bloom (P&B) offer a novel and sophisticated version of adaptationism. In spite of serious disagreements between us, I find it quite refreshing not to have to argue for linguistic facts, hypotheses, and explanatory principles that P&B take for granted, but that have long been anathema to the orthodox Darwinian adaptationists. It is revealing, though, that they fail to notice (or, at any rate, fail to register in their article) how hard it would have been for linguists and cognitive scientists to establish these central points, had the profession been dominated by orthodox Darwinian assumptions. Each of the theses we hold dear (the high specificity of our tacit knowledge of language, the innateness of universal grammar (UG), the noninstructive, nonanalogical process of language acquisition, the internal modularity of UG, the mechanisms of selective fixation of parameters and so forth) had to overcome a fierce resistance coming *also* from the orthodox Darwinian ideology. Many of Chomsky's writings, including the excerpts in P&B's paper, bear witness to this fact.

P&B take their distance from "panglossian" adaptationism, and concede that not *all* linguistic structures are the result of natural selection. They imply, by innuendo, that it was not the pure form of the adaptationist doctrine, but rather a perversion of it, that incited implacable resistance. Be that as it may, P&B should not find it so surprising that some of us want to hold *any* doctrine accountable for the repeated, resilient, and obnoxious perversions it tends to generate. P&B will probably condemn this as a conditioned reflex, but I object to the patronizing tone of what probably constitutes the only understatement in their whole paper:

> In one sense, our goal is incredibly boring. All we argue is that language is no different from all other complex abilities. . . . and that the *only way* to explain the origin of such abilities is through the theory of natural selection." (sect. 1, para. 4. my emphasis)

This rhetoric is suspicious, because it can be anything but "boring" to discover that "the only way" to explain the origins of a structure (in this case the central properties of UG) is by applying a doctrine that went out of its way (and often out of its depth) to deny the very meaningfulness of the search for that structure. Those of us who are familiar with the rhetoric, and the (otherwise excellent) writings of Richard Dawkins (notably in Dawkins 1983; 1986), from whom P&B have derived much inspiration, will recognize the ploy. "Boredom" is caused by the impression these authors have of arguing for the recognition of a *necessary* truth. Because they maintain that for any sane and unprejudiced mind the process of natural selection is the *only* explanation of "extremely low-probability arrangements of matter" (P&B, after Dawkins, sect. 2.2, para. 3), those who resist the inevitable appear to them as boringly irrational.

There is a major mistake, which P&B explicitly import from Dawkins, and which suffices to make their position untenable: They argue as if there were a universal, objective, interest-independent and background-knowledge-independent probability metric. The fact, however, is that *any* "arrangement of matter" has whatever probability we severally assign to it, as a function of what we know about the world, *and* about the local circumstances of its assemblage. Obviously, this background knowledge keeps changing all the time. We always knew that powdery snow was an "extremely low-probability arrangement of matter" in the Sahara at any time, but a very high-probability arrangement of matter in Utah in January, above 7,000 feet. What we did *not* know until very recently is, for instance, that the most detailed and most intricate patterns of a bird's feather appear an inevitable arrangement of matter under the joint influence of just two parameters, controlled by just two CAMs (cell-adhesion-molecules) (Crossin & Edelman 1989). The growth of knowledge incessantly leads to probability reassessments, and to more and more nuanced explanations. There is no God's-eye estimate of absolute low probability, just as there is no God's-eye estimate of an objectively high complexity (I defer on this point to the commentary by Richard Lewontin in this issue). Probabilities and complexities are not sculpted into matter itself, for any sane mind to see. An explanation through natural selection, just like any other scientific explanation, can *only* be a piece of induction. A bet, not a necessity. *Pace* Dawkins, it is the theory of natural selection that is accountable to our reason, not our reason to the theory of natural selection.

Another rhetorical centerpiece that P&B borrow explicitly from Dawkins (1986) is a contempt for "the argument from personal incredulity" (sect. 5.3, para. 3). What Dawkins, Pinker, and Bloom call personal incredulity is detectable in expressions such as "I do not see how" structure S can be explained as an adaptation to function F. The reader familiar with Chomsky's writings will easily recognize the formula. Chomsky is an extremely polite and respectful scholar and he uses the understatement "I do not see how." Personal incredulity is "personal" only in this urbane sense. It is, nonetheless, genuine incredulity. It concerns adaptationist claims, and it finds, I think, excellent legitimation whenever a resounding yes can be given as an answer to each of the following test questions:

(a) Could some other biologically possible structure, radically different from S, perform the same function F equally well, or even better?

(b) Could some other function F′, radically different from F, be responsible for S, or for parts of S, or for aspects of S?

(c) Are there important requirements that F would impose, but that S *as it is* simply cannot meet?

(d) Are there structural determinants (for instance, anatomical, biochemical, developmental), or physico-topological determinants (for instance, *Baupläne*) that account for S independently of F?

Even a strong presumption of positive answers to these perfectly legitimate questions *must* make one incredulous of the *sufficiency* of an adaptationist explanation of S based on F. P&B disqualify this test as irrelevant (sect. 3.4, para. 1) because they claim that it offers "not the slightest support to any specific alternative." Moreover, they stress that even if the adaptationists have no precise and detailed story to tell about how F has shaped S, neither have we, the exaptationists, a precise and detailed story to tell about some other F′, or specific nonadaptive factors (sect. 3.4.1, para. 2). So we are, at best, left *in pari turpitudine*. But this is only a make-believe symmetry: We maintain that there are good *reasons* why so little is known about the evolution of language and human cognition (see the brilliant account given in Lewontin 1990), whereas P&B maintain that we can know quite a lot. Because they claim that natural selection is both necessary *and sufficient* to narrow the gap, they bear very different burdens of proof. We are not bound to maintain that such and such specific cause *has*, as a matter of fact, been determinant. Our position is very different, because we stress how plausible it is that several other causal factors *may* have been relevant. If we establish this point, as I think we did in the case of UG, then we show that adaptationism cannot be "the only way" to explain the origins of language. As shown by Chomsky and other generative linguists over many years, and as I summarize in my paper (Piattelli-Palmarini 1989), it is highly plausible that there are positive answers to (at least) questions of type (a), (b), and (c) for all the principles of UG. P&B now venture new adaptationist proposals, none of which passes this simple test. In spite of their protests, I will continue to apply it to a couple of their tentative adaptationist explanations:

Time. P&B claim that one of the adaptive functions of language is to enable us to mark a clear difference, in communication, between what was, what is, and what will be the case. The syntax/semantics of tenses (allegedly) is what it is *because* it allows the build-up time relations in communication. Yet, take the following simple situation: John and Marcia are going to be married, and I want to assert now that one day, three months after their marriage (call it D day) John is going to discover that Marcia is pregnant on D day. Can I state this simple thought in a more compact, less cumbersome sentence (Dowty 1990)? It seems not (Enç 1990):

(1) John will discover that Marcia is pregnant

won't do, because (1) is also true if John will discover on D day that Marcia is pregnant now, at this very moment.

(2) John will discover that Marcia will be pregnant

won't do either, because (2) is true also if, on D day, John will discover that Marcia will be pregnant at some time *after* D day. It is easy to see that other attempts are equally unsuccessful, even allowing ourselves to plainly ungrammatical constructions:

(3) John will have discovered that Marcia (is) (was) (will be) (*will have been) (has been) pregnant.

Try it as you might, there just is no simple sentence forcing the hearer to lock onto this simultaneity of the two events at a time that lies in the future with respect to the time of utterance. The resources of tenses do not allow us to state this simple thought by syntactic means alone. We *have to* use some elaborate, strained, prolix periphrasis, supplemented with lexical pointers.

On the other hand, the sheer use of lexical temporal markers ("last time," "now," "after," "tomorrow," and so forth) allows us to communicate time relations without the use of tense or other syntactic markers. It is a well-known fact that dysphasics, aphasics, and speakers of pidgins help themselves freely to this device, whenever syntactic markers are unavailable to them (Gopnik in press). Contrary to what P&B claim, temporality does not need UG, and UG has severe limitations in expressing temporality.

Once again, the challenge is: How *inadequate* (how dysfunctional) must a structure be before an adaptationist admits that it cannot have been *shaped* by the proposed function? How does adaptive *under*determination differ from the claim that the structure is only *compatible* with the function?

Conditionals and contracts. One of the central communicative functions of language, according to P&B is to allow us to establish and manage social relations. Resting their argument on a celebrated case by Cosmides (1989) and Cosmides & Tooby (1989), they maintain that universal grammar has been significantly shaped by the need to promise, instruct, threaten, persuade, induce, order, and so forth, and by the need to monitor who is, and who is not, abiding by the rules.

I will leave aside the counter evidence that this claim has met in the psychology of reasoning (Girotto et al. 1989; Light et al., submitted; Manktelow & Over 1990) and concentrate on the shaping powers that social contracts allegedly ought to have had on UG. The first puzzle is that although there are syntactic constructions especially suited to these situations (modals, hypotheticals, conditionals, and so forth), this "function" does not uniquely pick out any syntactic construction or module. There are endless ways to promise, threaten, induce. (*Brush your teeth, and you can watch the movie / Those who repent will go to Heaven / Print all the news that's fit to print.*) Conditionalization and contract-making map onto a *desperately* mixed syntactic bunch. If "cheater detectors" and "social-contract monitors" have shaped UG, then they must have shaped *most* of it, not just those constructions containing "if . . . then," "unless," "if and only if." I will apply the test that P&B judge so irrelevant. One would like to know *how* these cognitive functions may have generated a syntax that bars perfectly innocent, and possibly (why not?) synonymous, expressions (**To watch the movie, to brush your teeth/ *Those repent who will for going to Heaven/ *Print all the news that they are fit they to print*), while it freely allows the formation of infamously perplexing conditionals, such as: *Five rabbits were promised to five boys, if no two girls were promised any pie/ When the foliage turns brown in the thick of the forest, mark some entries into many villages.*

The second puzzle is that there are even straightforward "if . . . then" contract formulae that UG *forces* to be ambiguous:

(4) The village chief said: "If, after their marriage, John discovers that Marcia is pregnant, I will let them both eat caribou meat".

A villainous chief could promote tribal natility without sacrificing any of his precious caribou meat. It suffices that he sticks to the letter of (4) and freely extends the make-believe promise to all and only those couples in which the bride is not *already* pregnant at the moment of the promise. The striking *linguistic* fact is that the chief can state things truly, yet cheat, and not be held responsible for cheating by what he says.

P&B (after Cosmides & Tooby 1989; Dawkins 1986; Trivers 1974) explicitly invoke this "cognitive arms race," an escalation in subtlety, in opportunities for deceit and double-talk, as a powerful adaptive force in the shaping of UG (sect. 5.3.4, para. 2). Let me again plead guilty of personal incredulity, because I fail to see how such a poor outcome could have come from these allegedly powerful driving forces. Contractual agreements offer a *signal* situation in which we are very far from naturally exploiting the advantages offered by UG. We rather have to *fight against* the limitations of UG. The phrasing of *bona fide* contracts is exhausting and frustrating. Ordinary speakers rightly feel that they can be fooled in a thousand ways by the mere *wording* of contracts. UG is a very bad, not just a suboptimal, device for cheater-detection. The function explains next to nothing of the structure that it has allegedly shaped through natural selection. It seems vastly more plausible that, possessing the languages we happen to possess, we managed somehow to coax them into the uses we see fit, getting plenty of glitches like (4) as a result.

This is, roughly, the way I would proceed in refuting, or at least weakening substantially, each of the adaptationist hypotheses offered by Pinker & Bloom. But they manage to render their approach utterly *indefeasible* (this is not meant to be a compliment): by taking refuge in nonoptimality, by conceding that not *every* trait of UG is the product of natural selection, and, on top of that, by retrenching themselves behind the accusation of "personal incredulity." Our exchange can then only turn into an empty battle of quantifiers and modals. We would fight over what is "more" and what is "less" plausible, over what might have happened and what may have happened. Resting their case on data that their own approach would have made impossible to collect, they proceed to construct an a posteriori, ad hoc, irrefutable explanation. They force me into a position from which I can only criticize wholesale the scientific ideology of their approach. Which is, in fact, what I have mostly done in this commentary.

On the coevolution of language and social competence

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Pinker & Bloom (P&B) claim that a me-Tarzan-you-Jane level of grammar would no more do for "primitive humans" than it would do for us. The social complexities of even a technologically primitive group, such as the !Kung San, could not possibly be realized without language. The claim is a puzzling one – the relevance of the !Kung San to the claim even more puzzling.

Do P&B propose that there never was a stage at which "primitive humans" (I take their handy, undefined term to mean any "near" ancestor of human) had a Tarzan-level grammar? Or do they intend something weaker? For example, that such a stage would have been short-lived because it would have been unable to realize the social complexities of even primitive humans? Yet, even the weaker claim links language and complex social behavior in a way that is both misleading and unsatisfactory.

The evolution of language, as P&B present it, gives too little credit to intrinsic social modules. They portray the typical linguist/psycholinguist picture of the human language a shining red, other competences pink. But human social complexity is far less dependent on language than P&B suggest.

Complex human social behavior is not, as P&B imply, a byproduct of language; rather, it is produced directly by "intrinsic social modules." Consider as examples of such modules "theory of mind," personality theory, pedagogy, perception of intention, and reciprocal giving. These "traits" are either unique to, or profoundly stronger in, humans than in other species.

For example, although we find some evidence for theory of mind in the chimpanzee, their attributions are far weaker than those of humans (Premack 1988; Premack & Woodruff 1978). The ape does not distinguish its own knowledge from the false beliefs of another, guessing from knowing, the different competences of young and old creatures, and so forth. Because such failures were established with nonverbal tests, they owe nothing to the ape's lack of language.

Chimpanzees differ from humans not in being unable to realize their theory of mind because they lack language, but in having a primitive theory of mind in the first place. On the other hand, a species that lacked language but had an adequate theory of mind could instantiate all the distinctions (and others as well), which the apes failed to do, and do so with even less than a me-Tarzan grammar. Humans also, though deprived of language, could pass all the tests that the apes failed.

Personality theory is yet another module on which complex social behavior depends, though it develops later than theory of mind (four-year-old children are already experts in theory of mind). Not until about their eighth year do children regard others as kind/cruel, generous/stingy, friendly/hostile, good/bad – using that large set of bipolar distinctions humans use in evaluating one another (Damon 1977). We find no such capacities in other species. Although the most primitive species show approach/avoidance, and more advanced species recognize individuals and show preferences among them, it is doubtful that the latter behavior is a precursor, let alone equivalent, of personality theory. Moreover, although we tend to make the same error here that we make in other cases – to equate the distinctions that underlie personality theory with the names of the distinctions – one can both test for the distinctions and instantiate them without the use of language.

Pedagogy is another human hallmark, a critical one; without it humans would not have culture. All species transmit information socially (e.g., the first solid food a rat eats is typically one that it smells in its mother's milk, Galef 1981) but only in humans does the transmission take this form: One individual observes and judges another according to some internal standard and then intervenes actively to modify the performance in the event it falls short of standards.

The disposition to modify the other one according to standards has a nonsocial counterpart – the disposition to modify oneself according to standards. Just as the former gives rise to pedagogy (the attempt to improve the performance and appearance of the other) so the latter gives rise to practice (the attempt to improve one's own performance and appearance).

Both practice and pedagogy presuppose a scale of values, an aesthetic, a system for distinguishing good acts (and appearances) from bad ones. In brief, the human has an intrinsic disposition to modify his own performance and that of others according to a standard, and neither the aesthetic that the disposition presupposes, nor the disposition itself, depends on language (Premack 1984; in press).

We see neither practice nor pedagogy in nonhuman species. Yet in the languageless chimpanzee we see, on rare occasions, one animal train another in a cognitively complex way – indicating that the animal has a representation of an ideal or desired performance, can compare the desired with the observed performance, and can then act in such ways as to reduce the disparity between the two. The chimpanzee does so, however, only when the change is of immediate value to itself, that is, when the change leads the trained animal to behave in ways that benefit the trainer.

Neither the intervention in the human nor the lack of it in the ape depend on language. Apes, though languageless, do train other apes when the change benefits them. They find a way, one that does not depend on language, viz., passive guidance – placing the other's body into the desired position – a procedure humans likewise use when teaching dance, sports, and so forth.

Let us consider a hypothetical species, intermediate between ape and human, resembling apes in having no language, humans in having social modules. This species could engage in complex social behavior – attribute states of mind to the others, distinguish one's own knowledge from another's false beliefs, train others, and so forth – doing all of this nonverbally. For such a species the addition of a me-Tarzan grammar might mean a considerable gain.

According to P&B, the complexity of social behavior in which humans engage with nonkin is found only in animals toward kin. In fact, humans evidence more complex social behavior with nonkin than animals do with kin. Consider cooperation: It has not only one facet, "working together to achieve a common goal," as ordinarily considered, but also a second facet, "an agreement to share," often overlooked. Although the former can be found in many species, the latter cannot. An agreement to share is apparently confined to humans – and therefore the notion of cheating does not apply to chimpanzees, for cheating presupposes an agreement to violate, and there is no evidence for such an agreement in the nonhuman.

"Aha!" you might now say, "Finally we arrive at the case where complex social behavior does depend on language." For how can there be an agreement to share without the use of language? Moreover, the cases we have dealt with so far – theory of mind, personality theory, pedagogy, aesthetics – are all secondary; discussing them was little more than a delaying action. Cooperation is the basic case, and when we finally get to it, we see that we do need language.

An explicit agreement to share would indeed require language. But an implicit one need not; moreover, an implicit agreement may be enough to bring about those actions we associate with humans when an individual who is part of the cooperating group violates the agreement to share. Specifically, an implicit agreement to share may be sufficient to produce censure – punishment of the party who cheats or violates the agreement – which is a form of behavior we do not see in the chimpanzee (Goodall 1988; Kummer 1978). [See also Caporael et al: "Selfishness Examined: Cooperation in the Absence of Egoistic Incentives" *BBS* 12(4) 1989. *Ed.*]

An implicit agreement could take the form of an expectation of reciprocation. Such an expectation may be part of the innate endowment of the human infant (Premack 1990). According to my model, the perception of intention is hardwired, as is the perception of the intention to reciprocate. The infant not only perceives that object A intends to affect object B – when certain conditions are met – but, more important for present purposes, that B intends to reciprocate. Specifically, the infant expects that B will act upon A in a way that preserves the valence (not the form) of A's earlier act upon B, computing valence according to an innate aesthetic, and so forth.

Because tests of the model are presently preliminary (but see Dasser et al. 1989 for early tests), I can argue only in principle. In principle, any species endowed with the expectation that giving will be reciprocated (and that receiving obliges giving) would have the functional equivalent of an agreement to share. A species endowed with this expectation should show censure.

For example, a former recipient who fails to give will be punished more severely than a neutral party (an individual who was not a former recipient) who fails to give. Of course, anyone who fails to give will be disliked, but special retribution will be reserved for former recipients who fail to give – for they violate one's expectations (of an implicit agreement to share). Innate expectations of this kind obviously do not depend on language – and are further evidence of the point I have been making. Complex human social behavior is not produced indirectly by language, as P&B suggest, but directly by modules that are intrinsic to social behavior.

That P&B underestimate the role of intrinsic social modules while overestimating that of language is further shown by their discussion of the !Kung San. They tell us in detail how this group resolves conflicts, dissecting such social complexities as those of divorce with the skill of a psychotherapist. We are evidently supposed to be surprised by this. But why? Does the complexity of social relations depend on technology, whereas only that of sentences depends on genes?

P&B do not advertise the sophisticated sentences the !Kung San make, nor do they invite us to be surprised by them, and this asymmetry is instructive. Although they emphasize that human language is independent of technology, they fail to mention that the same is true of human social behavior. We should no more be surprised by "advanced" social behavior in technologically primitive *Homo sapiens* than we are by the "advanced" language of such groups. The !Kung San are not "primitive" humans; as *Homo sapiens sapiens*, they not only have the same grammar we have but also the same social modules.

Were I concerned with evolutionary theory, I should be at least as interested in the evolution of the intrinsic social modules (for lack of a better name) as in the evolution of language. The story one tells about the former will have to be consistent with what one tells about the latter, for the two competences must have coevolved. If pedagogy, theory of mind, and so forth, provided a major occasion for the advanced use of language, language must have provided a goad for more powerful pedagogy, theory of mind, and so forth.

Incidentally, my limited interest in evolution is not because of churlishness but the fact that evolutionary theory does not answer the questions that concern me. What is the nature of human competence? That is not answered by evolutionary theory, recent recommendations (Cosmides 1989) notwithstanding. In fact, one can make a better case for the opposite recommendation.

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 impedance match between an open-ended set of meaning[s] . . . and a decidedly limited set of signaling devices" (Studdert-Kennedy & Lane 1980, p. 29). Each phonological solution is a system of sufficiently contrasting phonetic elements, and the phonotactic rules for their combination in morphemes; each syntactic solution is a set of lexical categories and the rules for their combination in phrasal structures. Thus, the evolution of language became possible when our hominid ancestors chanced on a simple principle of the physical world from which the hierarchical structure of phonology and syntax could emerge despite the intrinsic limits of their signaling devices.

My second point concerns possible parallels between evolution and individual development. P&B infer from the need for arbitrariness that "language evolution and language acquisition not only *can* differ, but *must* differ" (sect. 3.4.3, italics theirs). In my opinion, this statement is too strong. The conditions of language acquisition are certainly quite different from those under which language evolved, if only because the child is guided into language by speakers of a fully evolved system. Certainly, too, the rate and order of acquisition of certain linguistic structures may vary across languages, due to differences in the transparency of the arbitrary devices used for particular syntactic functions (Slobin 1982). Finally, it is true that Haeckel's "biogenetic law" ("Ontogeny recapitulates phylogeny") has been thoroughly discredited (Gould 1977).

Nonetheless, in an adaptively complex system, such as language, where the function of any one part may depend crucially on another, at least the broad lines of development are likely to be parallel, phyletically and ontogenetically. Among the reasons for this are that evolution is a succession not of adults, but of the ontogenies that give rise to them (Garstang 1922; Raff & Kaufman 1983). For an evolutionary change, in morphology or behavior, to give an adult organism a reproductive advantage, the change must occur in development. As Darwin (1859/1964, p. 447) recognized, changes tend to occur late rather than early in development, because early changes may ramify through the system, forcing changes in other structures that depend for their development on the changed structure. This conservative mode of evolutionary change is one source of the parallel between ontogenetic and ancestral sequences (Gould 1977). Another source is that the main lines of both processes necessarily proceed through "successive grades of differentiation" (Garstang 1922, p. 84) from the general to the specific. We may accordingly gain insight into how a complex system hangs together by studying its development in a cautiously recapitulatory framework. Such an approach has already begun to pay off in studies of early phonological development where a view of the phoneme or feature as "innate" has begun to give way to accounts of their emergence by differentiation of the syllable (Ferguson & Farwell 1975; Macken 1979; Menn 1986; Studdert-Kennedy 1987). A computational model of the self-organization of a phonological system of segments and features by implicit syllable differentiation, under competing selection pressures for ease of articulation and ease of perception, has also been implemented (Lindblom et al. 1983; the results have broadened our view of the processes of both evolution and development. Perhaps analogous studies of syntactic and semantic development will reveal hitherto unsuspected functional dependencies among those stages in the sequence that are general enough to be universal.

My final point concerns the question of phyletic continuity between language and nonlinguistic behaviors and neural mechanisms. P&B are surely right to reject the notion that the adequacy of an evolutionary account of language origins depends on the discovery of antecedents to modern structures in existing (or even fossilized) species. They are also right, in my view, to reject any unqualified claim that motor programs, *tout court*, are preadaptations for syntactic rules. Nonetheless, several facts and bodies of knowledge do suggest that language rests on a specialized capacity for endogenous control of rapid, diverse (i.e., nonrepetitive) sequences of movements (Kimura 1979), and that this capacity has antecedents in the primate line. First is the well-established link between lateralized control of manual praxis and language, hinting at related systems for the control of rapid, precise movement and for unilateral coordination of a bilaterally innervated effector apparatus. Second are the discoveries that manual sign languages have a dual formational ("phonological") and syntactic structure parallel to that of spoken languages (Klima & Bellugi 1979), and that the two modes of language may be homologues, supported by closely related, lateralized neural structures (Poizner et al. 1987). Third is the growing body of evidence that lateralized control and coordination of the limbs (hands and feet) did not emerge *de novo* in *Homo sapiens*, but had its beginnings in neural regimes for control of posture and feeding in other primates (MacNeilage 1987; in press; MacNeilage et al. 1987; 1988). Future research in these and related areas may throw light on the origins and mechanisms of the specialized system for endogenous motor control that both makes language possible and sets limits on its phonological and syntactic forms.

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NOTE

1. The commentator is also affiliated with the University of Connecticut and Yale University.

Grammar yes, generative grammar no

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I am always baffled when I read things like "the ability to use a natural language belongs more to the study of human biology than human culture" (Pinker & Bloom, P&B sect. 1, para. 2). No one would argue with the fact that human culture is an evolutionary and therefore a biological product. But the biological is chemical and the chemical is physical; so why don't we just let the physicists handle it all? The reason is that people are interested in phenomena at different levels of analysis. Biologists (not to mention chemists and physicists) are traditionally not as interested in language as are psychologists, anthropologists, and sociologists because, unlike echolocation in bats and stereopsis in monkeys, human languages differ among cultures, take several years to acquire, depend in a basic way on the ambient social environment, and show large individual differences in skill level at maturity. These are properties that mark phenomena as interesting to behavioral and social scientists.

Almost all of the "facts" P&B list are debatable or, at least, open to vastly different interpretations depending on one's view of the nature of language. It is true that all human societies have language, and it is probable (though our data are far from definitive) that language did not spread from one human group to another in prehistory. But this does not mean that language is innate. If, as is currently believed by many students of human evolution, *Homo sapiens sapiens* all came from one group that arose no more than 50,000 years ago, it is plausible that that group had already invented something similar to human language as we know it, and subsequent groups have passed it along (with modifications) as a part of their cultural traditions. (It is also possible that different groups invented similar communicative systems after this primal group fractionated and that the systems resemble one another for the same reasons that houses in different cultures resemble one another.) The fact that all societies have languages of the same degree of complexity is also derivable from this scenario. In any case, no nonEurocentric scholars would think for even a minute that technological societies are more complex than traditional societies in ways relevant for language.)

P&B's "facts" of language acquisition are just as problematic. Though language acquisition does seem to be a robust developmental phenomenon compared with some other cultural acquisitions, all children do not acquire language in the same way, they do not require "innate constraints" to do it, and they certainly do not do it in any instance with "no evidence." Every linguistic structure they produce they have heard. The reason they produce *comed* is because they have heard *kicked, pushed,* and so forth, and they are generalizing organisms – that is their cognitive nature. The reason they then revert to *came* is because they are imitating organisms predisposed to do things the way adults do them (even if this does not have any obvious advantages) – that is their cultural nature. This is what makes language acquisition so interesting: the balancing act between imitative and system-making tendencies as children attempt to acquire the communicative conventions of their linguistic community.

All of this belies the profound difference in views of language that characterizes modern linguistics and psycholinguistics. Many scholars of language and language acquisition (myself included) would not agree with P&B and Chomsky that there is such a thing as a language "faculty" operating on its own principles or that the essence of language is its "complex computational" nature. Many would not agree that grammatical rules are "all or none" or that the features listed in section 3.1, paragraphs 7–16, of the target article are the essential ones. For many of us, the imputation of generative grammar to humans derives from the mistake of assuming that systems created for their mathematical elegance and efficiency are, ipso facto, the way human beings operate. This is a mistake researchers in artificial intelligence quit making long ago.

All of which leaves me even more baffled because, with all of these differences of fact and definition, I still agree with the main thesis of P&B's paper. The basic argument that the ability to learn and use language evolved by means of natural selection, as did all other human abilities, is one that has been made by a variety of functionally based linguists on many occasions previously. I am sure there would be disagreements about exactly what evolved, but this level of specificity is not involved in P&B's main argument. The basic argument that some type of grammar is useful for communication, that communication is useful for social life, and that social life is a part of the human adaptive complex has seemed obvious to many scholars for some time. As I construe it, none of this involves generative grammar at all. My understanding of Chomsky's argument is that certain very specific structures of the generative grammar kind – the specified subject condition, for instance – are such that they have no communicative value and do not even contribute to the communicative value of the system as a whole. In such cases they could be considered "spandrels" because they themselves do not display the characteristic under selection pressure, which other parts of the system do in fact display. But because I do not believe that the specified subject condition is a property of human language – it is an invention of mathematical linguists – I will leave that argument to others.

Toward an adaptationist psycholinguistics

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Gould and Lewontin (1979) have intimated that the biological and behavioral sciences are infused with Panglossian adaptationism. If only it were true, the state of our knowledge – especially in cognitive psychology – would be decades ahead of where it is now. Better still, of course, would be a community of sophisticated adaptationists, such as Pinker & Bloom (P&B): a community of scientists who are mindful that every aspect of a phenotype is either an adaptation, a concomitant of an adaptation (a spandrel), or noise; who understand that all adaptations were built out of a substrate of earlier designs; and who know how to apply the logic and standards of evidence of evolutionary functionalism. Unfortunately, those who investigate with an adaptationist eye are rarer than pandas' thumbs, so most areas of psychology are lost in a sea of proximate theories, antifunctional quasitheories, and unframed and mute observations. It is a powerful testament to the eloquence of Gould and Lewontin that the single most important and well-documented explanatory principle in biology – adaptation – has become a dirty word outside of evolutionary biology, where it has come to be regarded as intrinsically post hoc and imprecise. Yet the concept of adaptation has a rigorous logical foundation, with stringent standards of evidence resting on probabilistic analyses (Dawkins 1986; Williams 1966). And although most cognitive psychologists are unaware of it, every time they discuss the function of a mechanism, they are automatically invoking the concept of adaptation, which entails these exacting standards. It is time they understood the rules that evolutionary biology places on functional explanation, so as not to invoke functionality – or claims of its absence – sloppily.[1] The injection of stringent adaptationism will not dilute a disciplined field with vacuous post hoc theorizing: Instead, it gives the concept of function specific and rigorous content that is otherwise lacking. And, correctly used, evolutionary functionalism provides crucial theoretical guidance that can help cognitive scientists discover the design features of complex cognitive architectures.

The vigorous proliferation of misinformation about adaptation and natural selection is attributable in part to the intuitions created by the types of data researchers in various fields confront. It is no accident that, for example, paleontologists (such as Gould) and geneticists (such as Lewontin) find the concept of adaptation strained and exotic in most specific psychological applications. Adaptations are complex interdependent systems that interact in intricate ways with the complex particulars of environments to produce functional outcomes. These components are lacking from the data paleontologists recover. Paleontologists deal with the few parts of a complex interdependent system that happen to fossilize, rather than with the complex system itself. Moreover, the environment that the system was designed to interact with has vanished beyond reconstruction. So the data paleontologists encounter are stripped of almost anything that would allow them to think profitably in adaptationist terms; consequently they mistakenly conclude that natural selection is an overrated concept that played little role in the history of life (see Gould 1989b). Geneticists are similarly insulated from data that could be structured with adaptationist concepts. They confront the raw code or else the population level statistical properties of genetic variation (largely noise), rather than the complex functioning system that that code creates. Someone who, not knowing its function, examined a computer program in machine language might equally well conclude that the sequence of 1's and 0's was random. [See Searle: "Minds, Brains, and Programs" *BBS* 3(3) 1980.]

But, like physiologists, cognitive psychologists do look at the functioning of complex architectures, embedded in structured task environments, which either succeed or fail at solving intelligible problems. Because the only scientifically coherent account for the origin of any complexly organized functionality is (ultimately) evolution by natural selection (Dawkins 1982; 1986; Williams 1966), cognitive psychology is a field whose central phenomena must inevitably be not only explained by selectionist analyses, but illuminated by them as well. All (nonchance) functionality is ultimately attributable to the operation of adaptations – that is, naturally selected innate aspects of the cognitive architecture. Cognitive science and the adaptationist branches of biology are natural intellectual companions and should start exploiting their connections. For this reason, cognitive psychologists can find in a careful and reasoned adaptationism a productive addition to their other analytical tools – if they can be exposed to it in a sophisticated, rather than a bastardized, form. That is why the target article is such a keen pleasure to read: P&B have found their way through a briar patch of rhetorical obfuscation to an impeccable understanding of the core of modern Darwinism. They know which parts of evolutionary biology are relevant to disputed issues in cognition and what their implications are, and have gone on elegantly to dissect the most common prevailing confusions.

P&B's central contention seems inescapable: Given any sensible analysis of the probabilities involved, a system with so many complexly interdependent subcomponents that together interact to produce complex functional output cannot be explained as anything other than an adaptation, constructed by the process of natural selection. Still, language itself is so large and elaborated a system that any precise characterization of the total constellation of selection pressures acting on it over evolutionary time is beyond our present ability to analyze in detail (that is, we seem to be limited to the kinds of global characterizations about adaptive function that P&B make). What then? Recognizing that the language faculty is an adaptation to communication may seem obvious and relatively unilluminating, but it is, in fact, a pivotal step. P&B's demonstration opens the door to a set of promising approaches to psycholinguistic problems: If the language faculty is an adaptation, then its component mechanisms are adaptations also – organized systems that accomplish specific functional ends subserving language production, perception, and comprehension. Psycholinguistics itself can become an adaptationist discipline, by characterizing how the functional design of each mechanism and subsystem solves its particular family of problems. The selection pressures on these component systems are considerably easier to analyze, so the parts will be far more open to lucid dissection than the whole. For example, the task demands on speech perception are considerably easier to find than the total array of uses language has been put to over evolutionary time. Analyzing these selection pressures should allow psycholinguists to discover previously unknown mechanisms and design features.

Not being psycholinguists ourselves, we hope we can communicate, by using traditional examples, the kind of adaptationist reasoning that can help psycholinguists, without requiring them to take the details of our proposals too seriously. Aside from realizing that organisms consist largely of collections of adaptations (problem-solvers), the central tool of adaptationist reasoning involves a recognition of what an adaptation is. Briefly, natural selection coordinates (1) a system of innate (i.e., reliably developing) properties in the organism, with (2) a set of structural properties outside the adaptation (often, but not always, in the "outside world") that recur across generations, in such a way that (3) the interaction of the two produces a functional outcome that ultimately contributes to reproduction (i.e., that solves a problem for the organism). For example, the design features of the digestive tract allow it to interact with the chemical properties of food to produce a functional outcome – the extraction and transport of nutrients to the circulatory system – that contributes to reproduction. To function, adaptations are selected to assume the presence of, to rely on, and to exploit stable and enduring structural and statistical regularities, both in the environment (Shepard 1987) and in other aspects of the phenotype. Just as the design of digestive mechanisms for breaking down starch are more easily discovered if one has identified the chemical properties of starches, the design features of the language faculty will be more easily discovered if one has identified environmental and phenotypic regularities that it can use.

For language – or any other mode of human communication – these regularities include (1) other aspects of the phenotype that the language faculty is embedded in, (2) the recurrent (i.e., innate) architectures of other humans, (3) the patterned behaviors these architectures generate, and (4) the relationships between these behaviors and the situations in which they are generated. The design features of language adaptations should exploit these regularities to solve adaptive problems. For example:

Language acquisition device. Chomskyans have long argued that the innate procedures of a child's language acquisition device (LAD) depend on stable and enduring species-typical regularities of the grammar-producing mechanisms of adults. Many grammars can, in principle, generate whatever subset of adult language the child hears; the child must induce which of these grammars in fact generated that sample. This cannot be done unless the design features of the LAD place constraints on the child's hypothesis space that reflect actual adult grammar. The Chomskyan argument is inherently adaptationist: Nothing, apart from selection, can endow the LAD with just those innate specializations necessary to supply the information regularly missing from adult speech samples, coordinating the two so that the local adult grammar can be uniquely determined.

Semantic bootstrapping. The semantic bootstrapping hypothesis (Grimshaw 1981; Macnamara 1982; Pinker 1982) about how children initially recognize syntactic categories depends on (1) statistical regularities between the aspects of a situation that adults talk about in their speech to or in the presence of children and children's own construal of such situations, and (2) lawful contingencies between the semantic categories that compose such construals (object, action, attribute, spatial relation) and syntactic categories such as noun, verb, adjective, and preposition.

Speech perception. The innate specialized mechanisms involved in speech perception (in the likely event that they exist) should have been selected to reflect and exploit the statistical regularities and universal properties of pronunciation and word formation across human languages, which will in turn derive from such factors as the properties of the articulatory apparatus. For example, microvariation in articulation will lead to a statistical correspondence between meaning and sound that are produced by similar articulatory gestures, rather than between meaning and acoustical similarity. Selection should therefore have designed perceptual systems that categorize by acoustic cues that reflect similarity of articulatory gesture, rather than by overall acoustical similarity; observation suggests that this is the case (e.g., Liberman et al. 1967; one need not invoke the hypothesis that one models the articulatory apparatus, simply that the dimensions of categorization reflect patterns produced by the articulatory apparatus).

Semantic analysis. Children, like cryptographers, can decode messages only because they have a priori statistical knowledge about likely messages. The child's task of discovering the meanings of words involves isolating, out of an infinite set of possible meanings, the actual meanings intended by other speakers. To solve the problem of referential ambiguity, the child's procedures for semantic analysis can depend on the fact that our universal innate psychological architectures impose on the world enough standard and recurrent interpretations between speaker and listener to make the deduction of a core lexicon possible. The system for assigning semantic meaning to arbitrary signs can rely on the presence of an immensely articulated and detailed collection of human information-processing mechanisms: on specialized mechanisms that are activated in the mother early in the child's development; on mechanisms that reliably identify evolutionarily recurrent situations (such as threat, play, or eating) in such a way that all participants have similar construals of the situation and responses to it, including things likely to be said about it; and so on. For example, emotional expressions obviously function as cues that assign standardized meanings to the contingent elements of situations. Similarly, domain-specific reasoning procedures such as social contract algorithms (Cosmides 1989; Cosmides & Tooby 1989) have both intrinsic definitions for the terms used by their procedures and cues for recognizing which elements in recurrent situations correspond to those terms. These evolved reasoning specializations may function as nuclei around which semantic inference is conducted. They may also assist semantic bootstrapping, relating syntactic and semantic elements through providing interpretations of the situation that the child is witnessing.

"Needs" and pragmatics. Most cognitive psychologists do, and all should, understand that innate architecture is a necessary part of any coherent psychological theory. Every psychological claim should specify (1) what innate equipment is involved, and (2) what environmental variables, in mechanistic interaction with the innate (or innately derived) equipment, produce the phenomenon to be explained. It is magical thinking to believe that the "need" to solve a problem automatically endows one with the equipment to solve it. For this reason, the invocation of social or practical "needs," pragmatic factors, acquired heuristics, or "functionalist" hypotheses to explain language acquisition need to be reformulated in explicitly nativist terms. It may be that the phenomena motivating empiricist arguments are generated by innate microspecializations that depend on subtle statistical regularities (perhaps the first words parents try to teach infants are basic object level names; perhaps the first transitive verbs involve interpersonal action – Mary hit John – and so on). The child cannot use such relationships unless some mechanism in the child is designed either to exploit them specifically, or to exploit a more general class that includes those relationships. To hijack Ramachadran's comments on perception (1990, p. 24), such phenomena as language acquisition, speech perception, and speech comprehension operate through

> "a 'bag of tricks'; . . . through millions of years of trial and error, the [language faculty] has evolved numerous short-cuts, rules-of-thumb and heuristics which were adopted not for their aesthetic appeal or mathematical elegance but simply because they *worked* . . . . This is a familiar idea in biology but for some reason it seems to have escaped the notice of psychologists, who seem to forget that the brain is a biological organ just like the pancreas, the liver, or any other specialized organ."

NOTE

1. Adaptationist analysis can be and often is performed ineptly, but that is true of every analytic tool. Many psychologists have the mistaken impression that adaptationist arguments must meet standards of evidence, but that "spandrelist" arguments need not. But a spandrel is a *byproduct* of an *adaptation:* To demonstrate that a phenotypic property is a spandrel, one must first state what adaptation it is a byproduct of, then demonstrate that that adaptation is, in fact, an adaptation, and, finally, demonstrate that the proposed "spandrel" is, in fact, a byproduct of that adaptation. For example, if one proposes that the ability to acquire a human language is a spandrel of general purpose learning mechanisms, one must state exactly what those general purpose mechanisms are, show that they exist, demonstrate that they are adaptations, and then demonstrate that these general purpose mechanisms can, in fact, allow one to learn language (through, for example, a learnability analysis; see Pinker 1984; Wexler & Culicover 1980). It is currently fashionable in some circles to believe that everything is a spandrel and to eschew the concept of adaptation – yet every time one calls some property of the phenotype a spandrel, one is claiming that some other property of the phenotype is an adaptation. "Naive spandrelism" is every bit as conceptually weak as "naive adaptationism," lacking only the latter's sporadic virtue of prompting insights about functional organization.

Why chimps matter to language origin

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Although the aim of Pinker & Bloom's (P&B's) target article is not new (see, e.g., Lightfoot 1982), it is far from boring. It displays an uneasiness concerning the alliance between Chomsky (1988, p. 167) who claims that the origin of generative grammar need not be explained by natural selection and – among others – Lieberman (1988, p. 3) who says that it cannot be a result of evolution.

The discussion of language origin has been dominated by two strange attractors and, as a result the discussion, has displayed somewhat chaotic behavior. Either you claim that language is a result of a general purpose mechanism governed by learning without much innate structure to guide the learning, or you claim that it is a module containing principles of Universal Grammar that facilitate the language learning. P&B give good arguments for accepting that the second alternative could have evolved by natural selection. It should no longer be possible to wave off generative grammar by saying that it is a biological miracle and of no theoretical interest for that reason alone. But it is only in the most general way that the connection between generative grammar and natural selection has been established. Whether the language that actually evolved is of the kind envisaged by P&B still has to be shown. It seems to me that there are good evolutionary reasons for claiming that language learning presupposes a specialized module (Cosmides & Tooby 1987; Symons 1987). A specialized structure will perform better than a nonspecialized one and hence will out-smart the latter, giving rise to a (cognitive) system containing interacting modules. This argument, however, does not imply that the module contains an innate basis of specific rules of language and how much of the structure is innate. A language module developed by a general learning mechanism based on very little specific information is still a theoretical possibility.

I agree with P&B concerning the general arguments on the evolution of a language module and accept that it is a result of specific adaptations in the hominid line not found in other animals. But there are certain distortions in their view of the use of nonhuman primates as a source of knowledge in the research on language origin. It is true that it is irrelevant to the possibility of the evolution of language by natural selection whether chimps have a protolanguage or not. However, the cognitive capacities of the nonhuman primates still count in the reconstruction of the road to language. The separate evolution of the chimpanzee can be overplayed, as P&B do it in section 5.4. Cladistic analysis of common traits in humans and chimpanzees can be used to show what capacities the last common ancestor of humans and chimpanzees had. It tells us from what point of departure the evolution of the language module could take place. This is of much more theoretical importance than claiming that if there is no continuity between ape communication and hominid communication then language is a biological miracle (e.g., Miles 1983, p. 43). To evolve by natural selection, language does not have to evolve from functionally similar structures (i.e., animal communication systems) but only to have enough time to make the selection steps in genomic space from one structure of whatever kind to another containing language. The phylogenetic continuity is in the cognitive systems between apes and humans, not in the domain of the communication systems. I think P&B will agree to this because they accept that language is mapped onto a cognitive structure using propositions and that language is adapted to communicating messages in the propositional format.

P&B refer with approval to Seidenberg (1986), who claims that trained apes are not only incapable of producing sentences but they do not produce symbols that can refer to classes of objects either. Their performance is rote learning. In his eagerness to defend the old Chomskyan position concerning the species-specificity of language, Seidenberg exaggerates the difficulties in teaching symbols to the apes. In the Lana experiments of the Rumbaughs (Rumbaugh 1977), it took 1,600 trials to learn the name of the first object. But in the following experimental study, the need for training of the naming relation between symbol and object sharply declined to 102 trials for the chimpanzee, Sherman (Savage-Rumbaugh 1986, p. 112). Premack has reported that when his chimps had caught the idea of the plastic pieces as a symbol for an object, all he needed to establish the connection between symbol and object was to show them together to the chimp (Premack, personal communication). The double-blind procedure of the Gardners to test the vocabulary of the trained chimpanzee demonstrates that the apes were capable of using signlike gestures as symbols for classes of objects (Gardner & Gardner 1988). A more modern position like that of Terrace (1985) seems in place on this topic. Does this show that apes have a language (or a language module) after all? No, it shows only that parts of language – the understanding of the role of symbols – is not a part of the language module. The two-component model of language suggested by Sperber and Wilson (1986) seems to explain this fact. Language is based on code and inference. The module takes care of the former, whereas inference on the intention behind the coded message is handled by the central cognitive systems (Fodor 1983). The central cognitive systems are similar in apes and humans and make up the base condition from which language evolved in the hominids (Ulbaek 1989; in press).

The nonhuman primates are relevant in other respects as well. The comparison between the apes and the hominid can give us specific clues to why language has evolved in the hominid line and not in the pongid. The cognitive systems on which the ape will rely in interpreting the behavior of others confer fitness upon the individual as long as he is better off with it than without it. Language is different from other cognitive systems because its function is based on the sharing of information. Sharing information is altruistic and will be selected against in most groups of animals because of the noncooperative basis of the social life. [cf. Caporael et al.: "Selfishness Examined: Cooperation in the Absence of Egoistic Incentives" *BBS* 12(4) 1989.] Here the difference between human language and animal communication is clear. Animal communication is not a cooperative enterprise but a way of deceiving according to Krebs and Dawkins (1984). [See Whiten & Byrne: "Tactical Deception in Primates" *BBS* 11(2) 1988.] Intelligence may be a much more widely distributed phenomenon in nature than language because language demands a common interest (acknowledged by P&B in section 5.3.4), whereas the use of some cognitive capacities can be purely selfish. This means that, generally speaking, language will not confer an "enormous selective advantage" (Chomsky 1980a, p. 239) on an animal as long as it is better off by keeping its mouth shut. The social constraints on the evolution of language will help us in making theoretical models for the conditions under which language became a possible way of communicating.

The prospect of providing a theoretical, coherent, and plausible account of the origin of language by natural selection is fine if (1) we have as a starting point a complex cognitive ancestor who can map a language onto his conceptual structures, and (2) if we can show how the cognitive mechanism demands certain social structures to communicate in a cognitive way. Then we may hope to be able to give an account of a very stable protolanguage onto which the language module has adapted, thereby becoming a biological structure to be inherited by future generations.

In defense of exaptation

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Pinker & Bloom (P&B) distinguish between accounts of language evolution based on adaptation and natural selection (their own view) and accounts (attributed to Gould 1987a, also Chomsky, e.g., 1982b and Piattelli-Palmarini 1987) based on side effects of other evolutionary mechanisms, including exaptation (or spandrels). They also argue that language arose to subserve communicative needs and that therefore selectional pressures were directed toward improved communicative ability. We suggest that the discussion of language evolution is strengthened by separating the initial emergence of the capacity for language from later developments; the emergence and the subsequent developments might be under the control of different basic evolutionary mechanisms. In addition, the discussion is improved by strictly separating linguistic capacity from the use of language as a tool for communication. We address these two points in order.

First, there is good reason for basing the explanation of the emergent neural systems underlying linguistic capacity on exaptation. Evolutionary fine-tuning of these neural systems then relies on adaptive selectional mechanisms. A priori, the exaptation account is attractive in that a theorist assuming such an account needn't argue that less-than-fully developed language (whatever that might be) was, in and of itself, adaptive. The exaptation account, however, loses force if it is inexplicit or if it relies on "unexplained physical mechanisms." Exaptation becomes explanatory only if there is evidence of evolutionary pressures for the selection of physical traits that do indeed yield a substrate that can demonstrably support the emergent secondary capacity. We suggest precisely this for language.

Evolutionary pressures for bipedality, lateralization, handedness, eye-hand coordination, and so forth, related to improved tool-making and use, caused an enlargement of the specific brain regions responsible for fine motor control and somatosensory feedback. This adaptive change in neural organization gradually resulted in the emergence of what Geschwind (1965) calls the "association area of association areas," the parieto-occipito-temporal junction, henceforth POT (a major constituent of Wernicke's area).[1] Parietal organization of the pongid brain is quite different from that of the hominid brain; the pongid brain exhibits a lunate sulcus that is anteriorly positioned, indicating the absence of a POT. This precludes true linguistic ability,[2] as supported by the paucity of humanlike language behavior in the great apes.

Any primate brain in which the lunate sulcus is anteriorly located indicates a brain organization incompatible with linguistic ability by virtue of the absence of a POT. According to Falk (1980 and elsewhere), an important point is that endocasts of fossil hominid skullcases suggest a dramatic distinction in the location of the lunate sulcus between *Australopithecus* (anterior) and *Homo habilis* (posterior).[3] To the extent that linguistic ability can be tied to brain organization involving the POT, and to the extent that evolutionary evidence supports pressures for adaptive neural organization creating the POT by the time of *Homo habilis*, an exaptation account of the emergence of language becomes significant.

It is important to point out at this juncture that, on our view, exaptation must be clearly distinguished from the use of a so-called spandrel (a distinction not stressed by P&B). A spandrel has no particular utility at the moment it arises; it is an architectural byproduct of growth and form. An exaptation, on the other hand, comes about by means of natural selection, has a primary utility when it arises, but is recruited for a secondary function. A spandrel account would be the least preferred explanation of any cognitive function, particularly in light of the high metabolic cost of maintaining neural tissue.

Our exaptation explanation makes no claim that a selective advantage was conferred at the evolutionary moment that the capacity for language emerged. Nor does it claim that when it emerged, language was used for communication. This relates to the second point in our first paragraph. P&B do not make a clear distinction between language and communication. In our view, the POT, hence linguistic capacity (but see Note 2), is most closely related to a particular human mode of organizing perceptual information (a similar point is made in Bickerton 1990). At the time the POT shows up in the fossil record (as indicated by the shift in placement of the lunate sulcus), there is also evidence for a developed Broca's area (Falk 1983; Tobias 1981). Refinement of motor cortex near Broca's area and associated modifications of orofacial morphology suggest that natural selection for sophisticated articulation, hence language fine-tuned for communication, became operational subsequent to the appearance of the underlying neurological capacity. In fact, it may be as late as *Homo sapiens neandertalensis* that hominids gained articulatory facility approaching that of modern humans.

In support of this interpretation, we consider it difficult to show a clear survival advantage provided by linguistically formatted communication for the task of group hunting, presumably a main communal interaction for archaic, primitive hunter-gatherers. Other communal hunters (e.g., wolves) maintain rather sophisticated communication and cooperative hunting in the absence of a linguistic system. A perfectly adequate communication system that did in fact derive from primate vocalizations would have been in place for these early humans. When, as P&B point out, it became advantageous to transmit the type of world knowledge not encoded in primate vocalizations, then selection for language would have become operational.

The important implication here is that human language did not evolve directly from the primate communication system as P&B suggest. In fact, the linguistic capacity of early hominids would have co-occurred with their primate vocalization system. Over time, information carried primarily in primate vocalizations ceased to be vocally encoded, as that system atrophied and the linguistic system became the dominant mode of communication.

NOTES

1. The sophisticated feedback mechanism through which the development of motor control regions of the frontal lobe derives the expansion of the corresponding somatosensory areas of the parietal lobe is discussed in detail in Dumford and Wilkins (1990).

2. More accurately, the absence of a POT precludes *modality-free conceptual structure* (in the sense of Jackendoff [1983] and elsewhere); space limitations prevent us from explaining this more fully. We therefore refer here to "linguistic ability," which is an oversimplification.

3. Holloway (1981), and elsewhere, disputes Falk's interpretation, arguing for a hominid brain organization for *Australopithecus*. Independent evidence for the anterior (pongid) position of the lunate sulcus in *Australopithecus* comes from as yet unpublished work on comparative primate neuroanatomy by Armstrong et al. (1989), which favors Falk's interpretation of the fossil data.

Anatomizing the rhinoceros

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I would like to pose a problem for both critics and defenders of Pinker & Blooms (P & B's) target article to consider. The subject at hand is whether "the human language faculty" can be explained by Darwinian natural selection. Both sides will be happy to grant that "the human language faculty" has a multiplicity of characteristics. How much of a dispute would remain if this single question about the evolution of a univocal object – "the human language faculty" – were replaced by a set of questions, each of them focusing on a different characteristic of the human language faculty?

If some other complex phenotype were under consideration, the need to move to a finer-grained set of descriptors would be quite apparent. It is a waste of time to wonder whether "the human birth canal" is the product of natural selection. Rather, one wants to focus on specific features that the canal possesses. Some may be adaptive; others not. Presumably, we would want to tell quite different stories and to muster quite different kinds of evidence when we replace a single phenotype with a set of more finely individuated phenotypes.

Lewontin's (1978) example about rhinoceros horns shows how adaptationist and nonadaptationist explanations can sometimes exist in perfect harmony, once the different *explananda* are separated. If we ask why rhinoceri have horns, we may feel inclined to tell an adaptive story concerning self-defense. But if we ask why one rhino species has one horn while the other has two, we may feels less inclined to see this difference as the result of selective advantage. Purely historical factors concerning the state of the two ancestral populations may suffice to explain the difference. "Why do rhinoceri have horns?" is a very different question from, "Why do rhinoceri have precisely the number of horns they do?"

P & B recognize the need to distinguish some features of the human language faculty from others. They observe that "even if it could be shown that one part of language has no function, that would not mean that all parts of language had no function." The converse of this claim is no less true and no less important. An overall assessment of whether the entirety of the structure is "mainly" due to natural selection cannot be obtained without this sort of attention to details ("parts").

My concern that the two sides may be talking past each other particularly applies to P & B's discussion of *arbitrariness*. They concede (sec. 3.4.3, para. 2) that "evolution has had a wide variety of equivalent communicative standards to choose from; there is no reason for it to have favored the class of languages that includes Apache and Yiddish, but not Old High Martian or Early Vulcan." They also argue that there is an obvious advantage for all speakers in a community to use the same linguistic system. There is obviously no conflict in saying that there is utility in agreement and that it is arbitrary what is agreed upon. An adaptationist answer may be appropriate for one question, but not the other.

One of the central problems that may divide P & B from their critics is their picture of how much the function of language constrains its form. P & B offer an analogy that illustrates this question. Being told that something functions as a sunshade or a paperweight, one is at a loss to say much about what its structure is. But if one is told that an object functions to display television broadcasts, the range of possible physical realizations is much narrower.

P & B list various structural features of human language – the existence of major phrasal categories, phrase structure rules, rules of linear order, case and verb affixes, auxiliaries, and a few others. P & B suggest that these substantive universals subserve obvious functions in communication. But even if these points were granted, two questions would remain: Could the functions be performed as well by other structures? To what degree do these features exhaust what is distinctive of the human language faculty?

A separate problem that bears mentioning comes into play after some specific trait of the language faculty is singled out for attention. Competing evolutionary explanations of a trait are especially difficult to evaluate when the trait is unique. From the point of view of testing, the best data set is the one that exhibits variation in a set of traits, none of which is uniquely possessed by any one species. For example, it would be too easy to invent explanations for the evolution of sex if there were just one sexual species. Fortunately, the data we face make the problem hard enough so that it may actually be solvable. Explaining the evolution of the human language faculty thereby inherits a standard problem facing much of human sociobiology.

Authors' Response

Issues in the evolution of the human language faculty

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> For the same reason that it was once effective in the theological "argument from design," the structure of the vertebrate eye can be used as a dramatic illustration of biological adaptation and the necessity for believing that natural selection for effective vision must have operated throughout the history of the group. In principle, any other organ could be used for illustration although the adaptive design of some parts may not be as immediately convincing as that of the optics of the eye. . . . [At times] the purpose of a mechanism may not be apparent initially, and the search for the goal becomes a motivation for further study. Adaptation is assumed in such cases, not on the basis of a demonstrable appropriateness of the means to the end but on the indirect evidence of complexity and constancy.
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> George C. Williams, *Natural selection and adaptation* (1966), pp. 6, 10.

> A human language is a system of remarkable complexity. To come to know a human language would be an extraordinary intellectual achievement for a creature not specifically designed to accomplish this task. A normal child acquires this knowledge on relatively slight exposure and without specific training. He can then quite effortlessly make use of an intricate structure of specific rules and guiding principles to convey his thoughts and feelings to others, arousing in them novel ideas and subtle perceptions and judgments.
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> Noam Chomsky, *Reflections on language* (1975), p. 4.

Connecting evolutionary biology and generative linguistics may seem suspect, but Chomsky's claim that human language is based on an innate biological structure makes it inevitable. Unfortunately, joining the issues across these widely separated disciplines is difficult even by *BBS* standards, because the two relevant fields are particularly vulnerable to partial misunderstandings. The theory of evolution has an apparent simplicity that is deceptive, and it is absent from university curricula in the cognitive and behavioral sciences (Barkow 1989). Moreover, its foremost contemporary popularizer often stresses his unorthodox personal slant in his accessible writings (Wright 1990). Appreciation of linguistics by nonspecialists is equally precarious. We suspect that many readers of Chomsky's books get exercised by Chapter 1 but find their eyes glazing over when the discussion in Chapter 2 turns to the fine points of why *John's friends appeared to their wives to hate each other* is interpreted differently from *John's friends appealed to their wives to hate each other*. However, the scientific justification for the attention-getting claims about innate linguistic knowledge springs from rigorous attempts to explain just such phenomena.

We found that the arguments in a number of the commentaries can be blunted by two simple tests. Among those that attempt to show that the human language faculty is not an adaptation, we ask whether the criteria employed would also rule out the vertebrate eye being an adaptation. And among those who deny that there is any special language faculty whose evolution needs to be accounted for, we ask whether they can provide a precise alternative that can account for why, say, English speakers accept *the child seems sleepy* but not **the child seems sleeping*, to say nothing of the more complex cases documented in linguistic research.

1. The logic of adaptation and natural selection

We were gratified to read the lucid arguments from philosophers of biology and evolutionary theorists (**Pesetsky & Block, Ridley, Sober, Tooby & Cosmides**) about the central role of adaptation in evolution and the ubiquity (and irrelevance) of arbitrary *aspects* of even the most obvious adaptations. Furthermore, as Ridley notes, the design of genuine adaptations need not be the only one conceivable or even the only one found in nature; examples range from the number of neck vertebrae in mammals to the genetic code itself. Attention, linguists: From this point on it is no longer permissible to make the argument "Aspect x of grammar could be otherwise, therefore language is a spandrel."

Ridley makes the interesting point that the *origin* of a structure and the *maintenance* of a structure are two different questions in evolutionary biology, and that often the tractable scientific work, driven by analyses of selective pressures and adaptive design, is restricted to the latter. We note that for similar reasons, the appearance of a structure in its initial form is a different matter from the elaboration and complication that leads to its fulfilling its current function. We believe that this distinction helps explain why the evolution of language has gotten the bad reputation of unmotivated storytelling. In fact, it is reconstructions of the *origin* of language, from "bow-wow" theories onward, that are often tainted by a lack of constraining evidence and far-fetched efforts to find precursors. Our strategy is different: We say virtually nothing about the precursors and very first forms of language and the specific sequence leading to its current form (cf. **Catania, Wilkins & Dumford, Limber**); we instead focus on evidence of adaptation from signs of design in synchronic language structure and acquisition, where the data are rich and abundant.

Ridley points out a common misconception: The term "adaptation" refers only to systems that have the *same* function as when they first evolved. This reinforces our argument in section 2.3.2 that "exaptation versus adaptation" is a false dichotomy (a section that **Lieberman** misread as the opposite of what it says). Somehow people have gotten the impression that "exaptation" means that a complex well-engineered organ can be co-opted from a spandrel or an organ with a radically different function as an immediate consequence of a single fortuitous mutation or a change of timing in morphogenesis. The role of natural selection is seen as mere "fine-tuning" (**Magnus, Wilkins & Dumford, Otero**). But the same probability arguments that militate against well-engineered organs arising out of macromutations count against organs that are well-engineered for one purpose arising almost finished, by sheer coincidence, out of organs originally engineered for a completely different purpose. Bird wings, mole claws, human arms, horse legs, and whale flippers are all exaptations from fish fins, but to describe the role of natural selection in their evolution as "fine-tuning" or "minor modifications" would be to miss an important point.

For example, **Magnus** accuses us of saying little because we fail to distinguish among a variety of "stories" he describes in which various combinations of macromutations, neoteny, and global increases in brain size precede natural selection. He feels that these stories are radically different from each other and from accounts in which natural selection is given central stage. Our point is that global factors like macromutations or brain growth cannot reasonably explain the complex design of grammar, so natural selection must play a central role in any of his alternatives.

Sober ends his helpful commentary by pointing out that explaining the evolution of language is methodologically problematic because we're the only species that has it, vitiating comparative correlational analyses that could distinguish among hypotheses about possible selective pressures. But evidence of complex adaptive design in one species can trade off with evidence from differences in structure across species. No one would doubt that the evolution of the eye was shaped by selection for sight even if only humans had eyes. Conversely, comparative data are crucial in explaining sex because its adaptive function is notoriously obscure, even paradoxical. The function of language is anything but obscure, and there is good evidence that it shows complex design for that function in the species that has it. This makes Sober's comparison with sociobiology relevant, but we draw the opposite conclusion. Our own arguments spring from the adaptive complexity of the computational mechanisms underlying the psychology of language as it is currently understood. The main flaw in many applications of sociobiology to human psychology is that their proponents do not focus on cognitive and emotional *mechanisms*, which are the proper subject for studies of adaptive complex design, but on particular *behaviors* (such as female infanticide) or on folk-psychological personality *traits* (such as "indoctrinability"), which are far too superficial and variable for such studies (see Cosmides & Tooby 1987; Symons 1989; Tooby & Cosmides, in press b).

Lewontin begins his commentary by attempting to clear up a "confusion," but the confusion that needs to be cleared up is not ours. He claims that we falsely include him (together with Gould as the author of the "Spandrels" paper) among the "people [who] have argued that the evolution of the human language faculty cannot be explained by Darwinian natural selection." But we did no such thing. Neither he nor the "Spandrels" paper is mentioned anywhere outside of the section where he would insist it belongs: "2. The role of natural selection in evolutionary theory." It is absent from section 4, "Arguments for language being a spandrel," which, incidentally, abundantly documents (together with sects. 1 and 2.2; see also the quotes in **Otero**) that there are people Lewontin knows who have made such arguments.

Lewontin characterizes the argument from his papers (including "Spandrels") as a "methodological claim about uncertainty," but his moral is not methodological but skeptical in the philosophers' sense. The "adaptationist program" that is their target (the assumption that "all aspects of an organism are adaptive," Lewontin 1978, p. 242), is aptly characterized by **Pesetsky & Block** as "lunatic adaptationism" and not in need of refutation. Contrary to Lewontin's claim, he and Gould have not shown that "in general" there are "too many competing reasonable explanations, including natural selection" for a feature being the product of natural selection; to do so they would have had to show that there is a reasonable explanation other than natural selection for, say, the vertebrate eye, which they do not. Of course one can be skeptical about whether natural selection in fact caused the evolution of the eye, but this would leave one unable to say that there is any well-founded difference between the evolution of the eye and the evolution of the chin. Basically, Lewontin and Gould duplicate (without attribution) the half of Williams' argument that warns not to invoke adaptation for just any beneficial trait, but they provide no counterpart to the complementary half, that complex design does license it. It is also questionable whether Gould's and Lewontin's strictures have a practical "methodological" benefit. As Mayr (1983) points out, "If it had not been for the adaptationist program, we probably would still not yet know the functions of thymus, spleen, pituitary, and pineal" (p. 328; see also Williams 1966, p. 10). And because the operation of some chance factor (e.g., drift, hitchhiking, fortuitously useful spandrels) is the one evolutionary explanation that can never be directly falsified, it can be discovered only after an organism's adaptations have been searched for and identified (**Tooby & Cosmides**).

Lewontin and **Piattelli-Palmarini** question the soundness of Williams's (1966) criterion of complex design (though in his 1966 review Lewontin described Williams's book as "95% correct," and the 5% he disagreed with did not pertain to adaptive complexity). Their thoughtful critiques deserve detailed discussion, as complex design is indeed the crux of our argument. Lewontin points out that the complexity of an arrangement of matter can be said to increase as its probability decreases or as its minimal description increases, but this depends on which sets of arrangements are counted as equivalent. We disagree with **Piattelli-Palmarini,**

however, when he says that this is a fatal defect. As Lewontin concedes, "we . . do not need an exact mathematical description of complexity provided our intuitive notions can be cashed out in particular cases." This "cashing out" involves three components.

First, we must show that intuitive assessments of complexity of design are widely shared among observers and not surreptitiously derived from some favored explanation for it. This is clearly the case for most instances of biological complexity. Paley tapped a widespread intuition when he said that the formation of the vertebrate eye demanded an explanation, and he was not exactly trying to make a point about the selection of alternative alleles in a Mendelian population. There may be difficult borderline cases, but the ones **Lewontin** brings up are largely irrelevant. True, we cannot all agree that a dog is more complex than a fish, but then little of biological importance hinges on the answer. The complexity of a stream pouring out of a basin, or of crystals and precipitates once thought to be of biological origin, are largely irrelevant, as Lewontin points out two sentences later. It is not complexity per se that is at issue, but complexity of *design*.

Second, it should be possible to characterize what is behind intuitions of design (see, e.g., Boorse 1973; Cummins 1984). The key features seem to be (1) a constant but heterogeneous structure: The parts or aspects of an object are unpredictably different from one another; (2) a unity of function: The different parts are organized so as to cause the system to achieve or maintain some special effect – special because it is improbable for systems lacking that organization that are otherwise physically similar to it, and special because it is among the small set of states that we would antecedently recognize as beneficial to someone or something. Insisting that both these conditions hold breaks the circle that we and Lewontin discuss (a function that is defined as exactly what an arbitrary structure does): In cases that we would all recognize as showing signs of design, the function must be stated independently and more economically than the description of the structure, and the heterogeneity of the structure is thereby explained in terms of the ability of the different parts acting in concert to carry out the function. A lens is very different from a diaphragm in structure and no physical process would lead one to predict that they would end up in the same object, but they are related by their role in high-fidelity image formation, and this allows one to understand the presence of one given the presence of the other. The space-filling lump of clay, in contrast, requires you to duplicate the exact description of the structure to state the purported function.

Finally, the intuitive notion must be replaced by a more substantive characterization as the science matures, grounded in a theory that can explain a wide range of phenomena other than the one that launched the inquiry. This is not a case of circularity but the bootstrapping of theoretical concepts out of pretheoretical ones that characterizes all of science. There are significant differences between the pretheoretical concept of design and what we now know as *evolutionarily possible* design; these differences obliterate any innuendo that the former is tautologously defined in terms of the latter. Natural selection requires that the benefit toward which a system is designed be that of survival and reproduction (and not pure beauty or pleasure or the harmony of nature); that the entity that benefits be the one that created the designful system (and not be some other organism, or the group or species or ecosystem); that the entity have the power to replicate itself over long timespans; and that in the past there must have been a population of similar reproducing systems varying in their ability to carry out the function. As Darwin (1859) pointed out, the existence of a designful structure that benefited some other organism, such as horses evolving saddles, would have immediately refuted his theory.

Furthermore, as this bootstrapping progresses, the set of possible functions (sensation, predator repulsion, metabolism, etc.) is refined as more cases are studied. These functions or "problems" cannot be defined solely by the environment, as **Lewontin** notes, but that does not invalidate the entire concept of a survival problem: Given the existence of one feature of an organism at time t (e.g., spending some time in the water), the organism can be said to face a problem (e.g., swimming faster) that affects how natural selection changes it by time $t + 1$.

It is also true, as both **Lewontin** and **Piattelli-Palmarini** point out, that the kinds of effects we view as physically improbable will be revised as our knowledge of physics and development progresses. **Lewontin** and **Otero,** for example, call attention to examples of complexity arising out of small differences in initial conditions of nonlinear dynamical systems. There may be reasons to expect such processes, poorly understood at present, to play a role in the development of complexity in morphogenesis, especially in the nervous system. But although there may be certain variables of complex systems that can vary widely with small changes in initial conditions, there is one that cannot: complexity of *design*. Complexity of design is not a variable that there can be a physical law about. It depends crucially on events at much higher levels of interaction involving the internal and external environments. It is simply not reasonable to expect that some currently unknown chaotic process will result in the formation of transparent lens-shaped tissue just in case it is in front of a light-sensitive diaphragm (as opposed to behind the kneecap) or in the formation of fin-shaped structures just for systems that develop in water, and claw-shaped structures just for systems that develop in trees. In fact, Gould (1980a) makes this exact point in defending the notion of natural selection as a process that operates at the level of the whole organism.

It is actually on a different issue that our disagreement with **Lewontin** is most profound. He correctly points out that the argument from design for language depends critically on how we divide the language function into subfunctions. But he dismisses our scheme for doing so, calling it "our conceptual biases." We call it generative linguistics and psycholinguistics. Indeed, elsewhere Lewontin dismisses all of cognitive science in a few sentences: "It is not easy, given the analytic mode of science, to replace the clockwork mind with something less silly. Updating the metaphor by changing clocks into computers has gotten us nowhere. . . . Imprisoned by our Cartesianism, we do not know how to think about thinking" (Lewontin 1981, p. 16; discussed by Dennett 1983; see also Lewontin 1983, p. 368.).[1] If cognitive science (including generative linguistics) were indeed a monumental mistake, we would admit that our particular argument for natural selection of the language faculty collapses.

But concede that we have learned some things about language over the past 35 years, and **Lewontin**'s contentions lose their force. There is no longer complete ignorance about whether some unknown neurodevelopmental law could create an entire language organ; the complexity and heterogeneity of language structure united by common function (mapping meanings onto pronounceable and recoverable sounds) makes it extremely improbable. There is no circularity in bringing up possible evidence for the actual reproductive advantages of language in a hunter-gatherer way of life (see also sect. 5) because our argument is based on evidence for the *design* of language for enhanced survival, not evidence for the *use* of language for enhanced survival, although the latter is important as an independent source of supportive data. And there is no longer the complete ignorance of functionality of language parts that renders the reconstruction of language origin an empty exercise. Just as, knowing how eyes work, we would not expect the lens to have evolved before innervated photoreceptors, knowing how language works, we would not expect complete syntax to have evolved before articulation (see the specific proposals by the linguists and psycholinguists among the commentators: **Jackendoff, Studdert-Kennedy, Limber, Wilkins & Dumford, Sperber, Newmeyer**).

It is true that modern linguistics does not characterize the language faculty directly in terms of neuroanatomy, much less genetics. But if the cognitive approach is at all on the right track, representational models such as grammars and parsers are abstract characterizations of neural structure and processing and cannot be of a radically different order of complexity of design. And inasmuch as the innate aspects of the neural substrate underlying language are not identical to those underlying brain tissue in general, some degree of genetic complexity is implicated, as well. The case studies described by **Gopnik** (and others cited in the target article) bear this out beautifully. We do not find genetic syndromes of specific language impairment that wipe out the entire language faculty, as might be expected if language had evolved from a single mutation that gave rise to all currently documented linguistic complexity through massive pleiotropy. Rather, language is impaired piecewise, and at least in Gopnik's case the function made defective by a single gene – inflectional features – corresponds closely to a submodule proposed in linguistic and psycholinguistic research (e.g., Pinker 1984).

Otero hints that we may have misrepresented Chomsky's views, which are "consistent with the thesis that language was shaped in some respects by natural selection." This is somewhat misleading. Chomsky's skeptical remarks are literally stated of "language" (or "grammar"), not "certain aspects of language" (see sects. 1 and 4.2 of the target article). More important, the issue is not whether there is at least one aspect of language that was shaped by natural selection, because this weak statement encompasses a vast range of positions that are very much worth distinguishing. Although it is literally correct to say that "the eye was shaped in some respects by natural selection," it is almost a joke, as it gives us no way of distinguishing the eye from a morphological epiphenomenon like the chin, some of whose aspects, no doubt, were also shaped by natural selection. It is exactly those aspects of the eye that we are most interested in as physiologists and anatomists that are shaped by natural selection: the aspects that allow it to be used as an organ for sight. The question is whether this is also true of language, namely, whether it, like the eye, is an adaptation.

Otero claims that we are wrong about the facts of biology, and meticulously documents this indictment using footnotes and asides from Chomsky's lectures and popular writings. Let us examine them in order:

1. **Otero** quotes Chomsky as saying that "it is not easy to imagine a course of selection that might have given rise to [wings]. A rudimentary wing, for example, is not 'useful' for motion but is more of an impediment." Actually, courses of selection of organs for motion that might have given rise to wings have been imagined for more than a century. Rudimentary wings are in fact used for gliding and parachuting by a variety of arboreal animals, and could have been used for balance, leaping, and propulsion during running by ground-living animals. The currently accepted theories of the evolution of flight in vertebrates, supported by aerodynamic analyses, involve various combinations of these factors (see Wilford 1985).
2. Explaining the evolution of an organ in terms of natural selection is "placing pure chance and arbitrariness at the center of all biological processes." This is an utter misunderstanding, notorious among creationists (Kitcher 1985b). Chance and arbitrariness (in the sense of genetically determined variation that is not directed toward adaptive phenotypes) is the raw material for selection, but selection for better designs for survival and reproduction is the epitome of nonrandomness.
3. "Darwin himself expressed misgivings" about natural selection in response to Wallace's point that hunter-gatherers are as intelligent as Europeans. **Otero** has it backwards; it was Wallace who doubted the ability of natural selection to explain the evolution of human intelligence, feeling that divine intervention was necessary, and Darwin's remark was a rebuke (see Gould 1980b).
4. "No one has ever explained how the component parts of the eye . . . could have arisen *separately* and *gradually* by a sequence of independent accidents. . . . a half-made eye would be of dubious selective advantage; it would, in fact, be utterly useless." As we pointed out in section 2.3.2, in 1859 Darwin explained how it could have happened (as it did about 40 different times in the history of life; Mayr 1983). Variants of his explanation are held today and can be found in any comprehensive introduction to evolutionary biology (e.g., Dawkins 1986).
5. In 1966 some physicists and mathematicians "argued on statistical grounds – through comparison of the known rate of mutation with the astronomical number of imaginable modifications of chromosomes and their parts – that [the laws that determine possible successful mutation and the nature of complex organisms] must exist and must vastly restrict the realizable possibilities." This quotation, from a Chomsky footnote, is famous among linguists, a number of whom have asked us whether these mathematical arguments refute Darwinism. Indeed if true they would, as the denial that there are "laws" that would determine *successful* mutation, in the sense of changes systematically beneficial to complex organisms, is one of the core tenets of modern Darwinism (Mayr

1982). It is therefore worth taking a look at these claims, as recorded in the symposium proceedings (Moorhead & Kaplan 1967).

At a picnic in Geneva in 1965, Martin Kaplan and another biologist found themselves outnumbered by several mathematicians and physicists who did some back-of-the-envelope calculations that seemed to cast doubts on the Darwinian theory of evolution; Murray Eden and Marcel Schützenberger were the most vociferous. Kaplan arranged for a symposium to debate the issue and enticed many of the leading evolutionary biologists of the day to attend or provide written comments, including Sewall Wright, Peter Medawar, Ernst Mayr, Richard Lewontin, C. H. Waddington, and H. B. D. Kettlewell.

When these eminent biologists heard the arguments, however, they did not slap their foreheads and shout, "Why didn't we think of that!" Eden's (1967) first argument was that the human genome contains on the order of a billion base pairs, requiring approximately one addition of a base pair per year since the beginning of the earth. If mutation is random, the odds that the human genome would have emerged is infinitesimal. This argument is patently unsound: The existence of the human genome is specified a posteriori so it is equivalent to saying that the exact poker hand one happens to have been dealt had an infinitesimal probability. What Eden really had in mind was some a priori notion like a complex functioning organism, which he then operationalized in terms of "useful" proteins. He noted that only an infinitesimal fraction of the space of possible proteins could have been explored by the entirety of organisms existing since the beginning of the earth. Therefore, in his words, "either the vast proportion of polypeptide chains perform useful biological functions in some integrated entity (a rather implausible hypothesis) or else evolution was directed to the incredibly small proportion of useful protein forms (by environmental constraints? by thermodynamics? by some as yet unexplored physical reaction?)." There are two flaws in this argument, however. First, natural selection does not consist of a random sampling of complete modern proteins from the space of possible ones. Rather, useful incremental changes in proteins are accumulated, defining directed paths within the space toward the ones we now find. (As Sewall Wright (1967) pointed out, natural selection is like a game of Twenty Questions, not monkeys at a typewriter.) Second, because of sex and other forms of genetic recombination (almost as old as life itself), the set of mutations necessary for some useful protein does not have to be accumulated in series within a single lineage of organisms, requiring time related to the mutation rate raised to the power of the number of required mutations. Rather, different mutations are stored independently in different lineages, and recombination brings them together to form vast numbers of new combinations, in people, in their descendents.

Schützenberger (1967) made a similar point using mathematical linguistics as an analogy. Sequences of codons can be considered as linguistic strings, which define a topology or space arranged by typographical similarity. Organisms' phenotypes define an independent similarity space by their biologically interesting properties. One needs a syntax of possible mutations, he argued, to ensure that semantically meaningful changes in phenotypes will result; otherwise the vast number of mutations would result in impossible organisms, just as changing a randomly selected bit in a computer program will cause it to crash, not do something equally interesting but different. The participants, most notably Lewontin, objected that unlike computer programs, arbitrary changes in codon sequences very often result in proteins with changed (but not biologically useless) properties. Thus Eden's and Schützenberger's arguments boiled down to the banal claim that the space of explored proteins contains exactly as many useful proteins as in fact evolved. While no one disagreed with the point (fleshed out by molecular biology) that not all logically possible proteins were equiprobable as products of mutation, the key challenge to Darwinism, that the *direction* of mutation must be biologically useful to the organisms experiencing them, is groundless. Today the Eden and Schützenberger papers are not to be found in the literature on evolutionary biology, except for a wry mention by Mayr (1982) in his review of how physical scientists have raised spurious objections to Darwinism for more than a century.

6. A "historic" conference (a 1980 meeting about macroevolution, highlighting Gould and Eldredge's (1977) then-novel notions of punctuated equilibrium) challenged the "four-decade long dominance of the Modern Synthesis." **Otero** got this characterization from science reporter Roger Lewin (1980a), who was transmitting the assessment that Gould and Eldredge had initiated a scientific revolution. But many of the conference participants objected that the "revolution" only overturned straw men (e.g., the claim that organisms of all sizes, shapes, and forms are possible, or that evolution proceeds at a constant rate) and fed off false oppositions (there are developmental constraints, therefore adaptation is a secondary factor; evolution can occur quickly in geological time, therefore saltationism is plausible). Lewin's report, which minimized these objections, drew a flurry of angry letters in a subsequent issue of *Science*, including one from Futuyma, **Lewontin**, Mayer, Seger, and Stubblefield (1981), which contained phrases like "doing violence to positions," "presenting a simplistic caricature of the modern synthesis," and "encouraging widespread misunderstanding." Indeed, Gould himself has had to disavow such popular misinterpretations of his claims (e.g., Gould 1987b).

7. "There are basic body plans . . that are maintained through immensely long evolutionary periods despite dramatic changes in the life patterns of organisms and the functions of their parts." Remember that the kind of body plans we're talking about (e.g., of the vertebrates) can embrace everything from eels to hummingbirds to warthogs to whales; only if one were uninterested in such differences could one minimize natural selection (see **Tooby & Cosmides**). It is puzzling that **Otero** brings up body plans at all: How can the fact that a trait remains stable for hundreds of millions of years across thousands of species help explain the evolution of a trait that evolved in less than 7 million years in a single species?

8. Chomsky has claimed that "there are not many kinds of possible organisms . . . if you went on [indefinitely] you'd get the same organisms over and over again." As a mathematical statement of the Shakespeare-from-monkeys-at-the-typewriter form, this may be true. But as an implication of supposed constraints on possible

organic forms, it would be hard to find a claim that biologists would disagree with more, given the breathtaking diversity and uniqueness of existing and extinct species, let alone possible ones. Gould is the foremost modern defender of constraints on forms of organisms, and he has devoted an entire book (Gould 1989b) and many essays eloquently arguing against a claim like Chomsky's.

9. Again from Chomsky (1982): "Many properties of organisms, like symmetry . . do not really have anything to do with a specific selection but just with the ways in which things can exist in the physical world." Both parts of this statement are doubtful. Bilateral symmetry of external sensory and motor systems is an adaptation for linear locomotion (Corballis 1989), and livers, stomachs, flounders, lobsters' claws, and so on do exist in the physical world.

10. Chomsky speculated in an interview that, "the density and packing of neurons in the brain" may "lead to certain consequences like speech or the ability to deal with numbers." John Lorber, a specialist in hydrocephalus, has reported (Lewin 1980b), "There's a young student at this university who has an IQ of 126, has gained a first-class honors degree in mathematics, and is socially completely normal. . . . When we did a brain scan on him, we saw that instead of the normal 4.5-centimeter thickness of brain tissue between the ventricles and cortical surface, there was just a thin layer of mantle measuring a millimeter or so. His cranium is filled mainly with cerebrospinal fluid."

The rest of **Otero**'s critique seems to be: (i) the theory of natural selection is a form of environmentalism, but Chomsky has shown that environmentalism is false for language acquisition; therefore natural selection is false. (ii) Descartes, Goethe, and Humboldt would have been dismayed by our article, because it would remind them of the ideas of Aristotle, Rousseau, and Hobbes. We admit that we have no defense against these criticisms.

2. Was there a pre-existing language of thought for natural language to externalize?

Hurford asks whether we can take it as given that prior to the evolution of language, propositional meaning structures existed, benefiting their bearers if shared. Perhaps instead these structures came into being with language itself, with the only prior requirement being that thoughts about the world (possibly analog) be communicated. This is exactly the kind of question in evolution that **Lewontin** doubts can ever be answered. **Freyd** does try to answer it; she suggests that "there is good reason for brain mechanisms to reflect the continuity of reality," resulting in "a strong evolutionary pull . . . toward computations emphasizing gradations, not categorization." Discrete combinatorial structure was thus introduced by language, a consequence of its being a shareable communicative system. We agree that this is a crucial question, and will try to take a preliminary stab at answering it here.

Fodor (1975) and Fodor and Pylyshyn (1988) point out that a combinatorial language of thought allows for an infinite number of distinct but systematically related thoughts; it also supports structure-sensitive inference processes that allow one to draw unlimited numbers of true conclusions. With mentalese, if you can think the thought that John loves Mary, you can also think the thought that Mary loves John. And if you have an inference rule that allows you to reason from "John ate and John drank and John danced" to "John ate," you can also reason from "John ate and John drank" to "John ate." So part of the answer is that it is advantageous to be able to draw true conclusions from true premises. This leaves open the question of why the original true thoughts we have are couched in propositions. Why have representations for categories, which are necessarily abstract, rather than for individuals? Why use discrete symbols for entities and relations that are continuous in the world, such as prepositions for spatial relations, rather than analog representations? Why combinatorial predicate-argument structures, as opposed to holistic representations of entire states or events? We suggest that the reason is that brain mechanisms should reflect not "the continuity of reality," as **Freyd** put it, but rather what Tolman and Brunswik (1935) call "the causal texture of the environment."

First, consider conceptual categories, which underlie many word meanings. Although no two individuals are exactly alike, they are not arbitrary collections of properties, either. Things that bark also tend to wag their tails and to lift their legs at fire hydrants. Learning categories and representing individuals as exemplars of them thus enables one to infer some of the unobserved properties of an individual from its observed properties: If it barks, it is a "dog"; if it is a "dog," it may bite. The inference leads (probabilistically) to true conclusions about the world because, as Bobick (1987) put it, the world has "natural modes." Objects are not evenly distributed in multidimensional property space, but hang together in clusters because they are the products of nonrandom evolution and physical law (barking objects tend to bite; heavy objects tend to sink). Categories at different levels of inclusiveness trade off the number of unobserved properties you can infer from category membership against the ease of determining category membership: It is easier to determine that an object is an "animal" than a "spaniel," but knowing that it is a "spaniel" as opposed to an "animal" you can predict more things about it. Categories of intermediate levels of inclusiveness (e.g., a "basic level" category such as "dog" that people treat as the object's "best" name; Rosch 1978) represent particular weightings of these two factors.

Now consider why spatial relations are represented discretely. Imagine standing in a rainstorm, 10 feet from an overhanging ledge. Move one foot toward it; you still get wet. Move another foot in the same direction. You don't get any drier. The near-discontinuity between the portion of the path where incremental changes of position leave you equally wet and the portion where incremental changes of position leave you equally dry is also the point at which one would begin to describe the spatial configuration using the preposition *under*. In other words, the quantization of spatial relations that determines preposition usage has a counterpart in causal consequences in the world, which are often highly discontinuous with changes in position: The possible behaviors of a beetle are different when it is *in* versus *on* a container; a vine behaves differently when it is *around* versus *against* a tree, and so on. Analog representations could preserve all the information in symbolic ones, but symbolic ones have two advantages. Storage is more economical: By encoding causal discontinuities in the world with the binary presence or absence of a symbol, you get more bang for the bit. More important, a properly encoded symbol makes causally relevant sets of configurations *explicit*, yielding intelligent conclusions with a simple processor. To determine whether the seeds will get wet, one needn't do any geometric calculations on one's database of objects' positions; one simply checks whether the mentalese symbol "under" is present or absent. Marr and Nishihara (1978) call this computational principle the "Principle of Explicit Naming."

Finally, consider why propositions have a predicate-argument structure. As an alternative, one can imagine a large set of zero-argument predicates: Instead of thinking something like "a hyena is eating" you could think "it's hyeating" just as you say "it's raining." Similarly one might think about periods of "hyeeping" (a hyena is sleeping), "gazeating" (a gazelle is eating), "gazeeping," and so on (the labels are mnemonics; they would have no internal structure). Why carve up reality into objects and the states or actions they engage in? Presumably the answer is that there are certain causal consequences of hyena-presence that cut across whether it is eating or sleeping: Given a loud sound, it might come bounding toward you, for example. Likewise, there are cross-actional causal consequences of gazelle-presence, such as noise-induced flight. There are also causal consequences of sleepingness that cut across whether it is a hyena or gazelle doing the sleeping: A sufficiently silent approach will preserve the entity's motionlessness. And so on for eatingness and runningness, as well. The external world can profitably be decomposed into main effects of objects and main effects of actions or states, and the properties of the cross-product can, to a large extent, be predicted linearly, without one's knowing all the interaction terms. Thus it is useful to represent a sleeping cheetah with the thought, "the cheetah is sleeping," for it allows one to predict, from knowledge of the causal consequences of cheetahood and of sleepingness, what to expect if one steps on its tail. This is better than having to test and observe the consequences of a brand-new zero-place predicate "cheeping."

In sum, we suggest that a propositional language of thought is probably not a byproduct of natural language but is itself a prior adaptation, perhaps found in other primates (see **Ulbaek, Premack, Studdert-Kennedy,** and **Lieberman**). It allows a mind to use structure-sensitive inference rules (Fodor & Pylyshyn) to compute an unlimited number of valid conclusions when operating on representations containing explicit names (Marr & Nishihara) for natural modes (Bobick) in the causal texture of the environment (Tolman & Brunswik).

3. Does universal grammar in fact show signs of adaptive complexity?

Is the claim that human language relies on an innate autonomous universal grammar incompatible with the idea that it is an adaptation for communication? **Piattelli-Palmarini** suggests that adaptationism is guilty of generating "repeated, resilient, and obnoxious perversions," namely "inciting invincible resistance" to Chomsky's ideas. But even if this were true, and we do not think it is, it would at best be guilt by association.[2] We agree with **Broadwell, Jackendoff, Maratsos,** and **Newmeyer** (see also Chomsky & Lasnik 1977; and Note 1 of the target article) that the study of grammatical structure is in no way antithetical to the study of its communicative function. We were particularly particularly pleased to see that **Newmeyer** had independently made two of our key points. Along with **Jackendoff, Studdert-Kennedy,** and **Limber,** he notes that an autonomous syntax is a solution to the problem of mapping between two very different kinds of representations: hierarchical, open-ended semantics, and serial, finite phonology. He also noted that locality principles such as subjacency, the quintessential arbitrary linguistic constraints, benefit hearers but only to the extent that they impede speakers, and so are advantageous as semiarbitrary innate constraints (Newmeyer, in press). The evolution of language is the one field in which you hope that someone has scooped you, what with accusations that it's all an exercise in personal ingenuity.

Other linguists are more skeptical. **Pesetsky & Block** correctly point out that to demonstrate adaptive complexity in language it is not enough to show that language is adaptive and language is complex; one must show that the complex systems are adaptive (see also **Hornstein, Frazier,** and **Piattelli-Palmarini**). The aspects of language studied by linguists, they point out, do not seem terribly useful for anything; the fact that in Russian, monomorphemic inflected forms shift stress but bimorphemic stems don't is an example. But we feel that these critics have lost sight of the language for the trees. We agree that generative linguists should focus on unusual, nonfunctional aspects of language: Aside from demonstrating that language involves autonomous structured modules and not just common sense problem solving, this focus is necessary because generative grammar has progressed to the point where more and more exotic phenomena are needed to distinguish among theories that make similar predictions in the simple cases. Finding quirky grammatical facts is the linguists' equivalent of the more powerful particle accelerator or larger telescope. But just as physics is not the study of particle detectors in Waxahachie, "language" cannot be equated with "good topic for a Ph.D. thesis in linguistics at MIT." We don't need to determine "the reproductive advantage . . conferred on speakers because they do not fully accept *the city's sight by the enemy,*" because this is a datum about a bit of behavior, not a biological structure, and thus did not itself evolve; it's the language faculty, which gave rise to the judgment data, that evolved.

Pesetsky & Block, by focusing on what linguists find "worth studying," state that the complex features of grammar play no role in allowing people to communicate, to express an infinite number of meanings using a finite number of lexical items, and so on. This claim is surprising. Wasn't it Chomsky who characterized a grammar as defining a mapping between sounds and meanings, and who said that a speaker can "make use of an intricate structure of specific rules and guiding principles to convey his thoughts and feelings to others, arousing in them novel ideas and subtle perceptions and judgments"? Don't linguists study such things as X-bar theory, word order parameters, inflectional morphology, segmental phonology, prosody, and so on, that are implicated every time we open our mouths to speak? Linguists *do* sometimes propose devices that do nothing but block certain constructions, such as the Complex Noun Phrase Constraint or the *for-to* filter. But such ad hoc devices are generally treated as mere summaries of unexplained phenomena, triggering a search for principles of greater generality, and – most important – these searches have met with frequent success. The result is the "deeper properties of language" that Pesetsky & Block refer to, like the X-bar principles of phrase structure, whose complex structure clearly provides for such useful abilities as referring to an unlimited number of entities with finite means. They may have other complex consequences for linguists to study, but as we have stressed, this is irrelevant to whether the system is an adaptation.

Pesetsky & Block provide examples of a problem we recognized in the target article (one also stressed by **Maratsos**): Arguments for functionality are embarrassed by the existence of linguistic variation. As mentioned in section 3.3, multiple grammatical mechanisms with partly overlapping functions allow various compromises between expressive power and other factors such as parsability and pragmatics. Once available, their redundancy can permit a modern group to drift to a particular language that does not exploit one of the mechanisms. We suspect that the Squamish, who lack an overt noun/verb distinction, convey the difference between gifts and giving using some other grammatical machinery (and even for them it is far from clear that they never use the abstract featural system underlying the noun/verb distinction anywhere in the grammar). Examples of redundant skills are not unprecedented in evolution: Oystercatchers are genetically capable of either hammering a mussel shell till it breaks or inserting their beaks to sever the muscle holding it shut, but each individual does one or the other exclusively, depending on what its parents did (Wright 1988).

Frazier asserts that human language is "far too complex for its hypothesized function – like evolving a cannon to kill a flea." She says that for communicative functions like reference and predication, "all that is needed is some system of semantic categories with labels for those categories"; she then describes such a system. But what happened to syntax? What Frazier is describing is at best the infant's one-word utterances, not a system expressing "reference and predication," to say nothing of the rest of semantics. Frazier's denigration of the usefulness of phonology is no better supported: "The average speaking rate of five to six syllables per second is nowhere near the human limit on articulatory speed." But that statement is virtually a tautology; averages are always less than upper limits if the variance is nonzero. It's like saying that the structure of the human leg could not have been shaped by selection for walking efficiency, because it is possible to walk with an immobilized knee (so having a complex working knee is like killing a flea with a cannon), and because average walking speed is nowhere near the human limit on walking speed. The actual rate of transmission of phonetic segments in human speech is an engineering marvel (Liberman, et al. 1967), and phonology plays an obvious role in making it happen. (For example, assimilation rules reduce the required acceleration of articulators, and enhancement rules increase phonemic discriminability. See also **Lindblom.**)

We don't understand why **Frazier** thinks it implausible that complex grammar evolved to externalize complex thoughts, but thinks it is plausible that complex grammar evolved to establish group identity, for which a dab of face paint suffices, or to aid memory, for which, as she points out, any simple temporal or spatial structure (such as a pegword mnemonic) will do.

As to the question of why ambiguity exists, it is unclear how much of a practical problem this poses in natural settings, where context and prosody eliminate most alternative analyses. In any case, as mentioned in the target article, ambiguity is a direct consequence of economy in surface forms: Reuse an old word for a new meaning, rather than inventing a new one, and you get an ambiguity whenever the stem is used. To answer the question, therefore, one should not leap to the conclusion that grammar is a spandrel, but should seriously examine the psycholinguistic mechanisms that introduce and maintain word-sharing, and the possible alternatives. It is not clear that a community of learners could all acquire a distinct morpheme for every useful lexical meaning; hundreds of meanings can exist for an English word, and children may already be near a limit on acquisition rate (they have been estimated to acquire one word per waking hour for years; Carey 1978). One solution is to allow each word to have additional but predictable meanings, using lexical rules (e.g., conversion of participles to adjectives), or processes of semantic extension such as metaphor and metonomy, at the cost of such occasional ambiguities as *visiting professors can be boring*. As a byproduct, arbitrarily related homonyms can arise alongside the predictable ones, because when words drift historically the link between their senses can become opaque and children will acquire them as independent sound-meaning pairings. We do not claim that this answers the question of why ambiguity exists (other processes of morpheme economy may be at work), just that one cannot glibly say that ambiguity is an easily avoidable maladaptive feature of language.

Hornstein argues that "nothing less will do" than to provide an account of "what *specific* environmental pressures *specific* grammatical properties are responses to"; for example, "What evolutionary pressure selects for the case filter or structure dependence or the binding theory or X′ theory?" He claims to have done so in his paper with Brandon (Brandon & Hornstein 1986), showing that only the properties of being symbolic, recursive, communicative, and non-stimulus-bound are responses to environmental pressures, where the relevant environment would have been "rapidly fluctuating and moderately capricious."

Demanding to know what environmental pressure selected for X-bar theory is like demanding to know what environmental pressure selected for the third metacarpal or the right iris. Natural selection is not a list of environmental forces each tugging at its own bit of anatomy. A controllable iris, transparent vitreous humor, focusing lens, and densely packed fovea all contribute to acute vision, a general function that is adaptive across a wide range of environments; aside from some minor variations, it would be a mistake to assign a different environmental pressure to each. Likewise, the value of each component of universal grammar is its contribution to how the entire language faculty allows complex thoughts to be communicated, an ability that is useful across a huge range of environments (see sect. 2 above).

Brandon & Hornstein's (1986) own attempt to link specific properties of grammar to specific environments is problematic. They suggest that the environment in which we evolved was especially conducive to language evolution because it was "rapidly changing and moderately capricious." This invites the reader to think of the well-known climatic fluctuations during the Pleistocene epoch, but that is of no help. Natural selection has no foresight, so a hominid lineage could not have evolved language just because it would have been useful for their descendants in coping with an ice age 10,000 years in the future. (Moreover, many existing species had ancestors in the Pleistocene environments in which humans evolved, but did not develop language.) On the other hand, if Brandon and Hornstein are referring to fluctuations within the lifetime of an individual, it is not clear why all environments are not "fluctuating and moderately capricious." Virtually every survival-relevant variable of an environment has nonzero variance over time and space, and no organism can evolve the most appropriate response to every possible situation it can ever face – especially if the environment includes other organisms. All things being equal, it would seem that you can't be too rich, too thin, or too smart. The way to explain the evolution of language may not be to look for some climatic or ecological condition to which it was a direct selective response. Rather, one might have to examine possible preadaptations (brain organization, omnivory, forelimb anatomy, etc.) and, most important, to specify the internal structure and interactions of language, individual learning, cultural learning, intelligence, and sociality, each of which may have been the major selective environment for the others (see Tooby & Devore 1987).

Piattelli-Palmarini asks, "How *inadequate* (how dysfunctional) must a structure be before an adaptationist admits it cannot have been *shaped* by the proposed function?" It is instructive to turn the question around: How adequate (how functional) must a structure be before an anti-adaptationist admits it was shaped by the proposed function? Piattelli-Palmarini lists some things that grammar does not do well: distinguishing among the readings of *John will discover that Marcia is pregnant* or *Five rabbits were promised to five boys, if no two girls were promised any pies*. But surely this is an empty exercise – even the most adapted structure will be unable to do *some* things. One could just as easily argue that legs are dysfunctional because humans are slower than a speeding bullet, less powerful than a locomotive, and unable to leap tall buildings in a single bound.

To answer **Piattelli-Palmarini**'s question, the relevant criterion is not whether there are some things that grammar cannot do, but whether there are things it can do *that cannot be done by a system designed at random*, where by "random" we mean unrelated to the task that the system is to be used for. Take a computational system that is either assembled at random or designed for some specific randomly selected task (not a hypothetical general problem-solver or adaptive system, which Piattelli-Palmarini eschews). Would it be capable, without modification, of encoding into strings of words the tense distinctions that human languages can express, to say nothing of modality, argument structure, quantification, and so on? Of course real neural spandrels are nonrandom in the sense of being the products of highly specific neurodevelopmental processes. But they are random in the crucial sense: What does an arbitrary cell adhesion molecule know about computational systems that can encode tense distinctions (as opposed to building feathers) – unless it is nonarbitrary because it had been selected to build a system that can do so?

And in fact the claim that universal grammar is functional is far from irrefutable. It is a fact that most grammatical principles and processes have the effect of defining a recoverable mapping between meanings and sounds (few, if any, grammatical distinctions make no semantic or pragmatic difference), whereas random computational processes do not. This need not have been so; we might have discovered a transformation that would replace every word in a sentence with *the*, or a Nonrecoverability of Deletion constraint allowing deletion only if ambiguity resulted, or principles that distinguished subjects and objects structurally but did not allow them to map systematically onto predicate-argument relations. In fact, in arguing that universal grammar would be a "poor outcome" of a selective process for success in engaging in social relations, **Piattelli-Palmarini** concedes our main point when he says that "there are syntactic constructions especially suited to deal with [social contract situations] (modals, hypotheticals, conditionals, etc.)." Where did these constructions come from, given that most randomly constructed computational systems would not provide them? **Piattelli-Palmarini**'s demand that there be a unique pairing of syntactic constructions with social functions (e.g., a construction for cheater detection) is not reasonable; see our response to **Hornstein.**

4. Language acquisition and language innateness

A number of the psycholinguist commentators objected to the idea that there is any innate neural structure specific to grammar whose evolution one would have to account for. Of course, Chomsky is the supposed target here, but many of the critiques are simply based on endemic misunderstandings. Psychologists, despite the "cognitive revolution," tend to be comfortable only with explanations that invoke one or two overarching principles of learning, so the suggestion that human language ability is in part a biological specialization made possible by neural circuitry with some inherent structure is received as a wild and exotic claim. (For example, what **Bates & MacWhinney** call "unrealistically strong notions of linguistic autonomy and linguistic innateness" is the notion that there is any.) As a result, reports of nativist theories of language are exaggerated with each retelling like some urban legend, prompting **Jackendoff**'s (1989b) article, "Why are they saying these things about us?" In the commentaries we read that according to linguists – specifically, "mathematical linguists" (**Tomasello**) – language development is so "canalized" that only one eternally unchanging language can exist (Bates & MacWhinney), there are no links between linguistic form and function (Bates & MacWhinney), children store all possible grammars in their genes (**Ninio**), the biological foundations of grammar are literally identical in all human beings (**Lieberman**), there is no more to language than syntax (Lieberman), and there is no role at all for learning and cognition (**Kluender**). Although we cannot aspire to **Otero**'s bibliographical and exegetical thoroughness, we are confident that Chomsky does not hold any of these positions, and more to the point, we can assert that the theory that there is some innate linguistic structure does not entail any of them.

The real issue is which innate mechanisms children use to generalize from a finite sample of parental speech to a system capable of correctly producing and interpreting the infinite remainder of the language (see Osherson et al. 1985; Pinker 1979). Theories that assert that some of these mechanisms are specific to grammar, not part of some general problem-solving device (e.g., Berwick 1985; Pinker 1984; Wexler & Culicover 1980), are rigorous attempts at solving this problem while taking the grammatical facts of human languages seriously (e.g., the kinds of facts mentioned in sect. 3.4.3, by **Block & Pesetsky**, and so on). Some of the dissenting remarks by psycholinguists suggest that they do not seem to appreciate the nature of the problem or the attempted solutions.

Kluender and **Lieberman** refute Chomsky as follows: People learn how to dress fashionably without innate mechanisms, therefore children could learn language without innate mechanisms. But fashionable combinations of clothes are either finite lists (no stripes with plaids) or graded generalizations along analog continua (slightly thicker stripes, slightly deeper blue). They do not define the combinatorial infinity of discrete elements that a language learner must cope with in generalizing from finite data, a different matter entirely (see **Studdert-Kennedy**).

Tomasello explains how children generalize properly in language acquisition as follows: "Children are generalizing organisms – that is their cognitive nature." This is supplemented by a second explanatory principle: "They are imitating organisms predisposed to do things the way adults do them." **Lieberman,** too, finds this explanation satisfying: "Humans, not cats, deserve the appellative 'copy cat'." Of course, if children hear their parents speak English, they will speak English; the question is: How?

Tomasello tries to minimize the kinds of generalizations children make by asserting that children "do not do it [language acquisition] in any instance with 'no evidence.' Every linguistic structure they produce they have heard." Children, however, do not hear *any* linguistic structures (let alone all of them), because linguistic structures don't make sounds; what they hear is a stream of speech. How they analyze this speech in terms of colorless, odorless, tasteless, and noiseless "linguistic structures" is exactly what we're trying to explain. Moreover, even for speakers who have developed to the point where they can analyze speech in terms of linguistic structure, the combinatorial vastness of language ensures that there will be many structures never before encountered that they are perfectly capable of handling. To take just one example, the frequency of the full expansion of the English auxiliary – *Fido must have been being groomed when he saw the cat* – has been measured as indistinguishable from zero in English speech and writing (Wexler 1981). Yet it is easily recognizable as English (though not such minor variants as **Fido must being groomed*). Finally, there are numerous experiments and speech analyses showing that children have mastered specific linguistic structures with no relevant evidence in the input (Bloom 1990; Crain & Fodor, in press; Gordon 1985; Pinker 1984; 1989a).

Lieberman thinks that we argue for innate grammatical mechanisms because we believe the input to children to be "disordered," but this claim is nowhere to be found in the target article. The input, no matter how orderly, is finite and does not come with its grammatical structure diagrammed, so how the child uses it to induce the rest of English does require an explanation. **Lieberman** also supposes that children's lack of access to negative evidence (information about whether any string of words is ungrammatical in the language being acquired; see Grimshaw & Pinker 1989; Pinker 1979) can be remedied by "self-generated 'negative' information [which] is always present through the process of imitation." But if children could internally generate information about whether any string of words is or is not English, there would be nothing left to explain, for they would already know English; language acquisition would be over. Moreover, it is very mysterious how children could get *negative* evidence through "imitation." How exactly do you imitate the act of *not* saying, *What does he like fish and?*

Finally, **Ninio** suggests that "the genome might as well store the entire language in the environment." It can't; the language is infinite, so it won't fit.

Bates & MacWhinney begin with some puzzling characterizations. They define "Level 1 functionalism" as referring to historical processes but then apply it to evolutionary ones without comment. They portray us as holding that "causal links between form and function have been 'sloughed off' by modern speaker/listeners," whereas we only claimed that different *kinds* of causal processes take place in evolution and in learning. They characterize us as covert "Level 3 functionalists," apparently because we believe that semantics exists (they seem to equate "semantics" and "function") and have proposed specific ways in which children might use semantics in acquiring syntax. It's not clear who isn't a functionalist in this sense; Chomsky, for example, would count as one (1981, p. 10). Bates & MacWhinney seem to conflate *acquiring something that is functional* with *acquiring something because it is functional.* It would be clearer for all if the term "functionalism" were reserved for the latter claim, namely, that the children assess how good their current grammatical system is (how expressive, efficient, well-understood, effective at attaining goals, etc.) and adjust it in directions that detectably improve functionality. If the position were made this clear, it could be tested; we know of no current evidence for it.

Bates & MacWhinney criticize us for distinguishing evolution from learning. In contrast, they "view the selectional processes operating during evolution and the selectional processes operating during language acquisition as part of one seamless natural fabric." But the "radical break from past to present" that they ridicule is in fact the very essence of Darwinism: Currently observable adaptation is the product of natural selection operating in the past. No mechanism guarantees that the process of increasing adaptedness that a lineage experienced in the past will operate "seamlessly" in the lifetime of an individual organism in the present. Indeed, their view is nothing more than Lamarck's first principle, that organisms adaptively respond within their lifetimes to "needs" that they experience. As Tooby and Cosmides (in press b) point out, taken to its logical conclusion it renders his second principle, the inheritance of acquired characteristics, superfluous, and for that matter obviates the need for Mendelian inheritance.

Presumably **Bates & MacWhinney** would agree that in the evolution of physical structures there is in fact a "radical break from past to present": No giraffe can grow a longer neck when the trees get taller just because it needs to. Perhaps they have been misled by the fact that many systems have evolved specific capabilities for adaptive phenotypic plasticity (e.g., learning). But these mechanisms are themselves specific products of a selective process that adapted the organism to cope with the range of environmental variation its ancestors experienced, and they most definitely do *not* form a "seamless fabric" with the process of natural selection itself. In fact, the two need not have anything in common, except as a coincidence. It is not only the time scales that are different; the parameters of possible change differ (hands develop calluses during a lifetime, but not webbed fingers, even among swimmers), and the kinds of benefit can be not only different but at cross-purposes. Natural selection favors only reproductive fitness in the current environment, but people often deploy their learning mechanisms in the service of motives that are currently fitness-reducing, such as practicing contraception or eating more candy than ripe fruit (see Tooby & Cosmides, in press b).

Bates & MacWhinney refer the reader to other writings on their "Darwinian approach to language development" (e.g., Bates et al. 1989), but these sources seem only to confirm that their arguments are based on a seriously flawed understanding of Darwinism. Bates et al. define "Darwin's principle of natural selection" as "the principle that innate structures evolved from prior forms" – a blatant misdefinition that is compatible with any of Darwin's nineteenth century competitors. In fact, the paper does not apply the mechanism of natural selection to language at all; its evolutionary "theory" is a pastiche of morals from Gould's popular writings ("preadaptation," "limited recapitulation," "heterochrony," etc.), which they use to argue that human language must be homologous to nonlinguistic cognition and to chimp signing. Consider also Bates & MacWhinney's (1982) argument that the universal aspects of grammar may be "inevitable solutions to certain universal constraints of the problem of mapping nonlinear meanings onto a linear speech channel" and thus would make the evolution of specific mechanisms for language acquisition superfluous. Compare now Lamarck: "New needs which establish a necessity for some part really bring about the existence of that part as a result of efforts" (quoted in Mayr 1982, p. 355).

Freyd, too, takes the Lamarckian position that the fact that something is useful is a sufficient explanation for how it came into existence. She believes "shareability" is an alternative to the theory that grammatical representations and processes are implemented in brains: "Shared knowledge structures (e.g., natural languages) have the structure they have partly by virtue of the fact that knowledge structures must be shared." Freyd explains this view by discussing three properties of language. One is the use of transformational rules, which she apparently believes are rules of paraphrase from one construction to another, a conception (due to Zelig Harris, not Chomsky) that is about 40 years out of date. The argument that transformations somehow exist because of their paraphrasing ability is therefore moot. The second is that grammatical constructions are rule-generated, not lists of memorized irregular patterns. But how does Freyd think that speakers succeed in sharing rule-generated constructions if they don't have neural mechanisms capable of deploying generative rules – the "biological determinism" that she is trying to argue against? A species that possessed only capabilities for memory would memorize constructions from generation to generation until the end of time, no matter how much more shareable the constructions would be if generated by a hypothetical rule system. Likewise, the third property, discreteness, does not just magically appear even if it would help people in sharing data; the hardware must be designed for it (as **Tooby & Cosmides** point out.) If you make a chain of analog copies from an analog tape, the quality declines, whereas a chain of digital copies from the original analog tape can be lossless. This seems like an instance where some kind of "shareability" calls for introducing discreteness into the representations of inherently continuous data. But according to Freyd's argument, this means that we don't actually have to go out and buy digital audio tape equipment – our tape decks would all just *make* themselves digital, because it makes music so much easier to share.

5. Speech

Lindblom and **Studdert-Kennedy** point out that the evolution of phonology is a more tractable subject than the evolution of syntax. Because it interfaces with acoustics and articulation, there are measurable external optimality constraints (from discriminability and ease of articulation); the specifically *adaptive* aspects of their design can be assessed, as Lindblom's elegant research shows. It is not yet clear how we should apportion the measured optimality between evolved constraints and historical processes; Lindblom's positive comments, and experimental work he cites showing that young infants' articulation reflects acoustic optimality criteria, do suggest that some aspects of phonetics and phonology are measurably designed by natural selection. We are cautiously hopeful that the properties of phonological systems can in turn be used to measure the adaptive aspects of the grammatical modules that interface with it, together with constraints from cognition and semantics propagating in from the other direction (as **Jackendoff** points out).

Kingston attempts to point out similarities between speech and other skills, but the gross similarities with, say, chewing, are not convincing arguments for homology. Incorporating nonrepetitive fine motions within cycles of larger motions seems like a property of very general usefulness in motor control and hence is probably found in many independently evolved motor systems. Similarly, although **Lieberman** objects to our criticism of his claims for homologies between syntax and motor control, his claim that "the formal rules of Chomsky's 'fixed nucleus' are ultimately related to the way that lizards wiggle their tails" (1984, p. 35) is hardly the kind of meticulous description of nonfunctional details that comparative anatomists insist on as evidence for homology.

Compare, say, **Ridley**'s example of mice and giraffes having identical numbers of cervical vertebrae. An apt comparison from cognitive science itself is **Jackendoff**'s beautiful work showing very detailed parallels between the semantics of motion and the semantics of abstract change, and his work with Lehrdahl on parallels between phonology and music.

Kluender's commentary is a position paper in the debate over whether speech perception is a special-purpose system or just one task assigned to a general auditory pattern analyzer. (See also **Kingston**: for the other side, see Liberman & Mattingly 1985; 1989; for a review, see Miller & Jusczyk 1989.) We believe the debate could come into sharper focus if, as suggested in the target article, attention were paid to the criteria biologists use to distinguish analogy and homology. This would invalidate Kluender's argument that positing a speech perception module would be unparsimonious, because convergences between speech and nonspeech and between humans and nonhumans would not be "serendipitous" but just another case of partial convergent evolution, ubiquitous in biology. Similarly, this may clear up Kluender's perplexity about why a few experimental demonstrations of such convergences have not convinced the field that speech perception is nothing but general auditory perception. For example, the demonstration that quail can discriminate and generalize three consonants after several thousand trials of training takes on a different meaning when we remember that human infants spontaneously categorize many or most phonetic contrasts found in the world's languages around the age of one month (Miller & Jusczyk 1990). It is as if someone trained rats to discriminate between slides of the Big Dipper and Orion after thousands of trials, showed some generalization, and drew conclusions about celestial navigation abilities in migratory birds. Note also that Old World monkeys, phylogenetically much closer to humans than chinchillas, show notable dissimilarities from both in perceiving place of articulation (Miller & Jusczyk 1990).

In general, inferring the real-world objects and events that gave rise to the energy reaching the sensorium is an unsolvable computational problem unless the perceptual system is designed to assume certain specific regularities about those objects and events (Marr 1982; Von Helmholtz 1885). This does not by itself show that special sensitivity to the physics of sound generation by the human articulatory tract is built into speech perception, but it makes it plausible, as **Tooby & Cosmides** point out. And it suggests that informative tests of the speech modularity hypothesis will come, not from showing that quail or chinchillas can be taught to discriminate pairs of contrasting speech sounds, but from comparing in some detail the minimal conditions needed for humans, nonhuman animals, and well-understood computational models to recover full phonemic representations from realistic continuous speech waveforms.

6. Topics related to the process of language evolution

6.1. Continuity with chimpanzees. We are interpreted by **Bates & MacWhinney** as arguing that "no infrahuman primate is capable of 'true language'," presumably because this is the stereotype of the Chomskyan position; we never said it. In fact, the notion of criteria for "true language" does not come from generative linguistics but from the very different tradition of Hockett (1960), and is not biologically meaningful; it is like asking whether some organism has "true vision." As we argued in the target article, the relevant scientific issue is which nonhuman abilities are *homologous* with language. This is an empirical question (and we find some of the cautious statements by **Ulbaek** and **Studdert-Kennedy** to be reasonable), requiring full attention to the details of human grammar, especially the nonfunctional ones. What does not make sense is **Lieberman**'s and **Bates & MacWhinney**'s claim that the issue can be decided in the armchair by some general principle of phyletic continuity. Consider Lieberman's claim that "since Darwinian natural selection involves small incremental steps that enhance the present function of the specialized module, the evolution of a 'new' module is logically impossible." Lieberman has just proven that eyes, livers, legs, hearts, and so on are "logically impossible": Our single-celled ancestors didn't have them, and new modules can't evolve! The source of this fallacy is Lieberman's stated equation of "exaptation" with "spandrels." As we pointed out in the target article (see also **Wilkins & Dumford**) these are not the same thing: Exaptation refers to an organ that changes function; a spandrel refers to something that is not an organ becoming an organ. What is an organ in one species may have been an unremarkable spandrel in its ancestor. In particular, the neural structures underlying human language, even if not spandrels now, could have evolved from an elaboration of nondescript bits of ancestral primate brain tissue (perhaps localized between control circuits for cognition, audition, or the vocal tract; see Wilkins & Dumford) and their homologues may play no role in the artificial symbolic systems currently taught to chimps.

The crux of **Bates & MacWhinney**'s argument (and that of Bates et al. 1989) is that there has not been enough time since the human-chimp split for a language mechanism to have evolved unless it is a minor rearrangement of apes' cognitive abilities. But Bates & MacWhinney do not estimate such things as the number of genes necessary for universal grammar, the selection coefficients for each resulting change, and the population size of hominid groups, to calculate that 5 to 7 million years is the wrong order of magnitude for these genes to have become fixed. It is just an assertion, not supported by anything. (In this vein, these commentators completely misunderstand our Note 3, criticizing us for confusing the rate of genetic and cultural change, whereas the difference in rates was the whole point of the Note.)

6.2. Intermediate grammars. We are happy to acknowledge that **Limber** was the first to invoke the Baldwin effect in explaining how innate grammatical mechanisms might have evolved gradually from communication systems originally supported by general cognitive processes. Contrary to the suggestions of **Ninio** and **Sperber**, there are no paradoxes, or confusions between language and the language faculty, in such an argument. If some people are using a grammatical construction (either because of a special genetic property or general cognitive talents), there could be an advantage in others' evolving to be able to process it automatically, with dedicated hardware, as opposed to conscious inferential reasoning (It is interesting that even the most normal-sounding of **Gopnik**'s genetic dysphasics complain that they find ordinary conversation strenuous and taxing.) Moreover, a genetic change in the language faculty need not simply generate the ambient language verbatim, in which case ease of processing would be the only selection pressure, and further evolution would halt. It can generate a superset of the language (or a partially overlapping set), much the way contemporary children go beyond the information given in the development of creoles, sign languages, and their frequent creative inventions. If such creations increased expressive power and were comprehendable by others by any means, it could set the stage for the next iteration of the evolution process.

We fully agree with **Sperber** that pragmatic inferences based on shared assumptions of relevance operate now, before the evolution of language, and possibly during its intermediate stages, helping to render partial grammars useful. But we do not understand why he believes that this is incompatible with grammar being a system that encodes propositional structures. A friend walks into the room and says, "You'll never guess what I just learned." The listener infers the intention of the speaker to convey a surprising bit of information of mutual interest. But holding the constant, surely the decoding of grammatical structures into propositions is a nontrivial part of what the listener must do in what immediately follows (e.g., "George is leaving Marsha." "George and Marsha are leaving." "I forced my colleagues to resign." "I was forced by my colleagues to resign." "We're going to have a baby." "We're not going to have a baby.")

6.3. Stages of evolution. Contrary to what **Wilkins & Dumford** suggest, we never claimed that human language evolved directly from the primate communication system, nor did we distinguish accounts based on "exaptation" from "natural selection"; in section 2.3.2 we said this was a false dichotomy. Wilkins & Dumford nonetheless put forward an evolutionary scenario that they feel we would disagree with, because of the central role it gives to "exaptation." In fact, our only objection comes from Wilkins & Dumford's implication that their account minimizes the role of natural selection for communication. They argue that the initial stages of the language faculty emerged as a byproduct of the evolution of the parieto-temporal-occipital (POT) junction, a constituent of Wernicke's area; the POT itself had resulted from evolution toward better fine motor control and sensorimotor feedback required for effective tool use. Crucially, their argument explicitly equates the computational basis of language use with the existence of the POT. But in doing so it asserts what it set out to prove. The POT is just a macroscopically visible chunk of brain. Nothing inherent to its size, shape, or location guarantees that it will contain circuits adequate to the computations underlying grammar. If we accept the hypothesis that it evolved to subserve intermodal associations and feedback, then all we know is that it subserves intermodal associations and feedback. The claim that a circuit shaped by such pressures for tool use, by virtue of being situated in such-and-such a brain location, automatically gave rise through "exaptation" to anything we would want to call language is just an assertion without plausibility or support. Modern neuropsychological studies showing that the POT plays a role in language processing show at best that the POT contains circuits that are necessary for language now, not that it contained circuits that were sufficient for language when it first emerged, which is what Wilkins & Dumford's argument requires.

Catania presents us with a specific scenario for language evolution from the Skinnerian tradition: "Language is an efficient way to change someone's behavior." Although we would agree that language is adaptive only insofar as it is related in some way to changes in others' behavior, its proximal effect is the transfer of beliefs and desires, and any causal influence on behavior is so circuitous, indirect, and unreliable that it makes no sense to identify manipulation as a principle selective force in its design. Hearing a relative clause does not cause one to build a nest; verb phrase complements do not elicit submission displays. This makes Catania's evolutionary scenario, which is based on elaborations of an original function of eliciting fixed action patterns, without motivation. Although functions may change during evolution, one can't just invent old functions without guidance from contemporary evidence (as **Broadwell** points out). The scenario contradicts modern Darwinian principles in any case (see Dawkins 1982). It tries to explain individual evolution by benefit to the group, posits manipulation without explaining why nullifying countermeasures were not evolved by the manipulees, and does not explain how and why the genetic protolanguage abilities, putatively confined to dominant males, were sequestered from their offspring – in particular, their daughters – not all of whom could have been the dominant male for generation after generation over evolutionary timespans.

6.4. Hunter-gatherers. Our remarks about the individual benefits of greater language skill are called into question by **Lewontin** and **Premack.** Lewontin (1990, pp. 244–45), like us, cautiously looks to contemporary hunter-gatherers for hints about the social organization of our ancestors. He thinks it is a challenge to our argument that in modern hunter-gatherers "there is a great deal of sharing of resources and danger." But this was precisely our point. The question is not whether sharing behavior occurs, but what psychological mechanisms maintain it in the face of constant danger of exploitation by non-reciprocators. Studies of modern hunter-gatherers are unanimous in pointing out that the ethic of sharing is backed up by obsessively detailed memory of one's own and others' offers of aid and clear expectations that such favors will be reciprocated, with considerable jockeying for microadvantages (e.g., Cosmides & Tooby 1989; Konner 1982; Shostak 1983; Yellen 1990). This system of interpersonal and interfamily reciprocity – decidedly unbourgeois, by the way (Glantz & Pearce 1989; Yellen 1990) – is measurably mediated by language. Eibl-Eibesfeldt (1989, pp. 525–26) summarizes quantitative studies of what some tribal groups talk about:[3]

> In both cultures [!Kung and Eipo], a great deal of talk centered on food, comprising 59% of all !Kung conversations. . . . Some discussions concerned where food is found, which products are found in individual gathering areas, and in which gardens one should

work. A larger part of the food conversations are concerned with the social aspects of nutrition. Topics include who gives what to whom, and criticisms of those who do not share their food. Three quarters of all words of the Mek-speaking In are based upon giving and taking.

Premack energetically expresses disagreement with us but we feel it is based on a misinterpretation. We concluded our discussion of hunter-gatherer use of language by saying that technologically primitive humans "could no more live with a me-Tarzan-you-Jane level of grammar than we could." Premack interprets this as arguing that language is necessary for complex social cognition and behavior. This was not our point. We simply meant that given the existence of complex social behavior, once it becomes mediated in part by language there are selective pressures on individuals to develop better and better language skills, so as to benefit the most, and be exploited the least, in such cooperation. It was an answer to Premack's (1985) rhetorical question (not discussed in his commentary) of why complex grammar would be useful to mastodon hunters. As such, we thoroughly agree with the conjectures Premack makes here to the effect that social competence can exist without language and probably preceded and coevolved with it.

We are surprised at **Premack**'s stated lack of interest in evolutionary theory. He discusses his research showing that children are acutely sensitive to recipients of benefits who fail to reciprocate. We would think he would be at least a bit curious as to why it is this ability that is well-developed in all children, as opposed to, say, the calendrical calculation ability of idiots savants? Could it be just a coincidence that precisely this cognitive ability – possibly with powerful counterparts in adult reasoning (Cosmides 1989) – was predicted almost 20 years ago by evolutionary theory to be a prerequisite to the unusual degree of sociality found in the human species (Trivers 1971)? Although we would be the last people to suggest that one can understand the evolution of language and social competence without studying them, we believe, contrary to Premack, that evolutionary theory does have a role to play in answering the question that concerns him, namely, "What is the nature of human competence?" As G. C. Williams (1966, p. 16) put it, "Is it not reasonable to anticipate that our understanding of the human mind would be aided greatly by knowing the purpose for which it was designed?"

NOTES

1. Note in this regard that it would behoove **Piattelli-Palmarini** and **Otero** to read Lewontin more carefully before invoking him as an authority on discussions related to "the theses we hold dear."

2. We are unaware of any criticisms of Chomsky's ideas that were explicitly motivated by adaptationism. The only evolutionary critiques we know of are based not on adaptation but on current variation (**Lieberman**) and phylogeny (**Bates**) – the latter motivated by Gould's notions of exaptation that **Piattelli-Palmarini** himself puts so much stock in.

3. We thank Nancy Etcoff for bringing this to our attention.

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