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Pangloss to Cyrano de Bergerac: »nonsense, it's perfect!«

A reply to Bickerton.

Im folgenden Aufsatz wird gezeigt, daß soziale Attribution und Sprache voneinander getrennte Fähigkeiten darstellen.

Bickerton has raised various points: some substantive, e.g., that language is the sole basis of human uniqueness; some curious, e.g., that I am a Lamarkian; and some that are perhaps neither, e.g., how did language evolve? In this reply, I deal mainly with »2« and »3«. The reply to »1«, requiring a more extensive argument, is presented elsewhere (Premack, submitted).

Language: Ontogeny and Phylogeny

Dismayed by the preoccupation with syntax in the sixties, Premack and Schwartz (1966) proposed the Britannica experiment. In this Gedanken experiment, a young father (swathed in a blue terricloth robe) stands at the foot of his son's crib reading to him from Britannica Encyclopedia. The infant, of course, has no situations to understand and »put into correspondence« with the text; nonetheless the average infant speaks grammatically somewhere in the vicinity of »Marsupialia« (superorder characterized by premature birth), precocious infants just after »Abelard« (exemplary 11th century French logician), while regrettable few do not commence speaking until after Zansibar - even Zulu. »Britannica« was intended to satirize the prevailing obsession with syntax and to bring into focus the desperate need to take »understanding of the situation« into account.

Today, the issues have been reversed in importance. Even, for instance, in the Wexler-Culicover model, a child derives b from a combined consideration of the words in the sentence and his understanding of the situation in which the sentence is uttered. In other accounts, the emphasis on situation is far greater.

Bickerton proposes a model for the evolution of language which consists of, »>proto language«, a trained-ape-like stringing together of mean-

ingful symbols on a ... pragmatic basis; a computational system which imposes structure and automaticity on the output of the first component« (Bickerton). Nowhere do we find »understanding the situation« entering the model. The signs the ape makes and the order in which it produces signs may be determined entirely by pedestrian factors, e. g., by the arrangement of objects in the room, the salience these objects have for the ape, the »strength« of the signs in the ape's repertoire (how recently he was taught them and how frequently he has used them). Much of what we presently know concerning the ape's ability to »understand situations« comes from three small studies: Sarah's limited ability to put markers on objects, agents, and instruments; the language-trained ape's ability to recognize and complete visual representations of action; and Sarah's apparent attribution of mental states to others. These are fragments in a puzzle. How should we build a theory of »understanding the situation« from such fragments? Perhaps if the ape had a greater capacity for »understanding situations«, it would not have so meagre a »proto language«. Or does the ape's inability to develop a »proto language« of any consequence come from other sources? We must not give »proto language« away too quickly.

Current models of language acquisition seek to do justice to both syntax and »understanding the situation«, placing them together early, for mutual assistance, as it were. Here is an alternative approach - in which the main factors develop independently and come together only later. Consider that the child must acquire three skills - words, syntax, an understanding of situations - and ultimately put them together. Words will combine to form sentences, and sentences will be placed into correspondence with appropriate situations. Current views of language acquisition consider that these are synergistic skills. But they may only seem to be synergistic. Were we to impose separations between experiences that are normally overlapping, we would see that the abilities derived from these experiences are not interdependent at all.

Suppose we were to teach the child words in whatever way we choose, and then expose it to »meaningful situations«; but we never exposed it to sentences. Of course, we provided the child normal love and affection. Suppose that despite the fact that the child heard no sentences, it developed an essentially normal »first language«. This »first« language would bear no relation to the adult system - and would be the child's limit if we did not subsequently expose it to adult sentences. However, when we did expose the child to sentences, the sentences would not be accompanied by situations. They would be presented in the total isolation of accompanying situations (as in the Britannica experiment). The child so exposed would nonetheless go on to develop normal adult language.

The point of the experiment is simply to argue that the first phase of

child language only appears to be, but is not actually a prerequisite for adult syntax. Adult syntax can be »triggered« (once the child reaches an appropriate age) by positive exemplars alone, unaccompanied by any situational support. In contrast, the language the child normally picks up in the first year or two is not triggered but *learned* - just as the ape's language is learned (and the two systems differ vastly only because the child is far brighter than the ape). But the only contribution early language makes to »true« language is as a source of vocabulary.

The ability to »read« both the causal and intentional relations »contained« in a situation can be acquired without language. It is so acquired by nonhuman primates. Although the animal's ability to distinguish different causal and intentional roles is slight compared to the human's, this is not because the animal lacks language. The human, even *without* the assistance of language will acquire a strong interpretative system; whereas the nonhuman, even if taught language will acquire a weak system. Finally, mapping sentences to situations requires learning, but not learning in which the language and nonlanguage abilities need develop together. In fact, if the two systems were complete before the mapping began, both having developed independently, the mapping might be far more efficient. Obviously, this is not a view we are likely to test. But we may wish to keep an eye open for the implications of this view for they may emerge unexpectedly in certain »experiments« of nature.

I comment in »Gavagai« that there are no intermediate systems between animal call systems and human language, and Bickerton, after some quibbling - the speech of Genie, the signing of apes (he might have added the speech of an intoxicated teenager) - agrees. Indeed, not only agrees, but goes on to urge that not only is there no difference, »... *there cannot be* (any difference) between them.« (italics his). He seeks to support this claim with two points.

First, in dismissing McNeil's proposed language for Washoe as an example of an intermediate language. McNeil proposed, the reader may recall, that the ape, because of its alleged gift for social relations, had a rule for requests (but for no other sentence types). For example, »you give me banana«, »you me banana give«, »banana you me give«, etc. are all true to McNeil's rule: »You« (addressee) precedes »me« (addressor). In contrast, »you eat banana«, »banana eat you«, »you banana eat«, etc. would carry out no rule - words would drift freely - because this string is not a request. Bickerton warns that a division of this kind would entail endless complexity. For »If there is anything we have learned in the past decade« it is that syntactic rules apply exceptionally.

The blind character of present syntactic rules permits us to deduce little about the character of possible earlier rules. Consider the analogous problem of deducing the character of possible earlier somatic cells from

present-day cells of people and bacteria (which are the same). Did earlier versions of the cell have the same character? As Monod points out, we cannot say, because there are no fossil cells. Similarly, the absence of fossil languages leaves us with the same problem. One cannot reconstruct the past simply by equating it with the present.

The real problem Bickerton has with McNeil's rule comes from a simpler source, he does not understand McNeil's rule. Bickerton asserts that to carry out McNeil's rule, the individual would have to learn a bi-partite division between nouns - and this would entail endless complexity. But McNeil's grammar does not entail any division of nouns. McNeil's rule applies to intentions or topics, not lexicon. Notice the overlap of nouns between, e.g., »you give me banana« (to which the rule applies) and »you me eat banana« (to which no rule applies). Can McNeil's rule be dismissed categorically as a forerunner of present grammar, as Bickerton urges? I really cannot say. My intuitions suggest that a speaker could easily know when he wanted something (and was opening his mouth to make a request), and when he did not (and was opening his mouth for other purposes). Intentions as basic as these do not seem difficult to discriminate, and given two such easily discriminable states, the next step would seem equally easy. One of the states would lead to the use of a rule and the other would not. Should I confess that in proposing McNeil's system as a possible example of early language, I was being fanciful, never dreaming the topic would take so serious a turn?

Bickerton advances a second and, judging from its length and detail, more important argument to prove that »things are as they are and could not be otherwise.« He shows that in order to be able to make all human distinctions - e.g., to use all human predicates - one would require a language of roughly human complexity. A semantic analysis, such as Chomsky's theta roles, could not by itself parse the sentences that these distinctions would entail. I regret that Bickerton devoted his obvious talents to this demonstration. I would prefer that he had done more nearly the opposite - assisted the non-linguist by asking whether or not there is a »basic« set of predicates that could be handled by a semantic analysis. If we knew what such a list might look like, we could then better estimate how well a species would fare if confined to such a list. Could it carry out basic business - scavenger, hunt, gather, and the like - or would such a list hopelessly cripple any species confined to it? The examples (of difficulty for semantic analysis) that Bickerton provides are not as convincing as he may suppose. »Basic business« need not be impaired if the species were denied many of the predicates that he mentions, e.g., »seem«, »undergo«, »be tall«, etc.; if it could not distinguish mode (slow/fast); and even if it did not distinguish alienable from inalienable possession (though I see no reason why this distinction could not be made on a non-syntactic basis).

In showing that the use of all human predicates requires a language of human complexity, Bickerton is extinguishing fires that were never lighted. Certainly it was never my point that all human predicates could be realized by an »intermediate« language. My point was rather simpler. Why use all human predicates? In the simple protohuman world of flints, skins and moldering mastodons, how would the use of these predicates benefit the individual? Would a protohuman using language of human complexity end up butchering more mastodons?

Attribution in nonhumans

Do only humans attribute intention, or is such attribution found in other species? Tests we have carried out on chimpanzees indicate that they make social attributions, but attributions that are limited in several respects. First, the list of mental states that humans attribute is incomparably long - guess, trust, doubt, hope, etc.; we have no reason to suppose that apes attribute any of these states. Second, the human child by the age of four can distinguish what he believes from what someone else believes. In apes and children below four, this is apparently not so; they can attribute to others only beliefs that are the same as their own.

Bickerton would explain this limitation of both the young child and ape as a limitation in language. »Although Premack rhetorically asks us to hypothesize species which have >social specialization< but lack language, it should be pretty clear that this particular list of >social specializations< could not even get off the ground in a languageless species. Take social attribution, for example - >the disposition to attribute beliefs and intentions to others< (290). Premack notes that such a disposition exists in apes and three-and-a-half-year-old children, but that the power to attribute to others *beliefs and intentions different from one's own* emerges only after four in humans and perhaps never in apes (*italics his*). It does not, apparently, occur to him that there is no way in knowing that others have beliefs and intentions different from one's own unless they are able to tell you about those beliefs and intentions and you are able to understand them. Since talking about what one thinks and feels demands language a little more complex than that within the grasp of the ape or the three-year-old, the facts that Premack notes are hardly surprising, and are fully consonant with the position that language is not just a necessary prerequisite for social attribution but also a sufficient prerequisite - that any species with true language will automatically develop social attribution.«

The quotation is hard to resist because it provides so sumptuous an example of an extreme view: Mind is essentially equated with language. Every mental trait of consequence is made to depend on language. While

Bickerton is hardly alone in taking this path, he treads it with a heavy foot. I have met linguists (and philosophers) before who thought the world began with language, but they had all heard of visual perception and believed in it. Bickerton is singular, he cannot even conceive of information being acquired by any other means than language. I must now introduce him to visual perception. Though an ancient process, it is still used to acquire information about virtually everything, including the beliefs of others. Consider this example.

One looks at a scene and sees a mother hiding chocolate in a red bowl while her son George watches. George leaves and Dotty enters. She spies the chocolate and transfers it to the green bowl. Exit Dotty and reenter George. Now, according to Bickerton, no one viewing this scene could possibly know anything about what George believes, for George has never talked to anyone, telling them about his beliefs. Somehow, however, you and I know where George thinks the chocolate is hidden. And so do even four-year-old children who, when asked by Wimmer and Perner (1983) »where will George go to find the chocolate«, answer correctly. It is only three-and-a-half-year-old children who do not. They regularly send George to where they (not George) know the candy to be.

As Wimmer and Perner have shown, the inability of the three-and-a-half-year-old to answer questions about what George believes is not attributable to a language deficiency, to an inability to understand »where will George go to find candy?« Three-and-a-half-year-olds answer questions of equal or greater complexity, »where was the candy originally?« »Where is it really now?« Their failure is specific to one topic: the content of the other one's mind.

Recently, the specificity of this failure attained additional interest with the demonstration that the autistic child, known for a deficiency in social-emotional behavior, exhibits the same failure (Baron-Cohen, Leslie & Frith 1985). Autistic children do not answer correctly »where will George go to find candy?« and differ therefore from both normal children and children with Down's Syndrome who, despite having a *lower* mental age than the autistic child, answer this question correctly. Finally, autistic children no less than the controls, answer correctly questions that do *not* deal with the other one's mind: where is the candy really now? Where was it originally? Social attribution, the theory one constructs of the other one's mind, has great complexity, yet children acquire this theory without formal training, and their ability to do so appears to be independent of general intelligence. The more we learn about this competence the more it appears to be an integrated unit, a module.

The Snares of Representation

Bickerton would explain the ape's inability to deal with »ideal form« on the grounds that it cannot represent »ideal forms«, because to do so requires language. To quote him, »but representation requires a language of some kind, and indeed the function of language as a means of forming internal representations is ... prior to its function as a means of communication ...« In other words, the ape (and other language-less creatures) are walking about without internal representation - badly lost no doubt! Bickerton has it turned around. It is not internal representation that depends on language, but vice versa. If it were internal representation that depended on language, not only the ape but also the pre-language child would be in trouble. For the child could not acquire language without internal representation, but having no language it would have no internal representation. The child would be as stuck as the ape would be lost.

On the other hand, perhaps what Bickerton is really trying to say here concerns the usual distinction between »mentalese« and speech. But then he must go on to say something quite different, such as: The ape does not have language, yet, judging from its other performances, it appears to have some kind of internal representation or mentalese. But if the ape has mentalese then it should be able to internally represent »ideal forms«. Bickerton cannot have it both ways. A final point on behalf of the data. The adult ape appears to have at least some ability to judge »ideal forms« (Premack 1982).

Lamarck Deposed by Pangloss

Moving from the substantive to the curious, I had thought Lamarkians were extinct, the last one having passed away quietly some time in the late thirties, shortly after the meeting of naturalists, geneticists and other biological specialists that ushered in the great compromise or »modern synthesis« (Mayr 1976; Mayr and Province 1981). This synthesis provided a role for both major parties: The geneticists' mutations became the »raw material« of evolution, natural selection the process that selected among the mutations. Lamarkians were given no role in this new division of labor. And with the growing recognition of the causal role that behavior can play in natural selection, Lamarkians also lost their sting. Now what a creature *learns* can (without requiring the inheritance of acquired traits) affect the evolutionary fate of his species.

Currently, the Lamarkian has been replaced by a new villain, the panglossian or adaptationist. This individual looks out on a commodious world, pronouncing every trait he observes an »optimal adaptation«.

Bickerton provides a reasonable facsimile when asking rhetorically, »Is language too powerful?« and answering, »You might just as well ask, is a giraffe's neck too long? Are elephants too big?«

A variety of non-adaptive factors discussed by Gould and Lewinton (1979; Lewinton 1980) - genetic drift., pleiotropy, allometry, multiple adaptive peaks - suggest that neither the giraffe's neck nor human language need be optimal adaptations. In principle, the neck could be not only too long but also too short, as human language could be not only too powerful but also too weak. Who but Pangloss leads Bickerton by the hand into the following tautology: if a mutation is not mal-adaptive, »then the most anyone can say is that it must have been adaptive.« In other words, if not not-A, then A. At last, a proposal from Bickerton with which I fully agree.