

LANGUAGE AND COMMUNICATION IN PRIMATES

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Our century has seen a number of attempts to perform a language transplant on non-human primates, in particular chimpanzees. As is well-known, the earliest attempts foundered on account of the modality used: the phonic character of human language resisted a transplant. In the mid-sixties new efforts began to be made to achieve greater success by using other modalities. To date we know of mainly three sets of experiments. There are, first, the experiments initiated by Allen and Beatrice Gardner, where the American Sign Language of the deaf (ASL) was used with the subject Washoe. Then we have the experiment done by David and Ann Premack with Sarah, where an equivalent of writing was used, and, thirdly, Duane Rumbaugh's work with the chimp Lana, where a computer-linked keyboard was used. The results of these experiments are dramatically different from those where spoken language was used. So much so that it was felt that some form of linguistic communication had been established. It is difficult to escape from the impression that, for the first time in history, man has communicated linguistically with an animal.

There can be no doubt that, to a large extent, the enormous interest aroused by the recent linguistic experiments with chimpanzees is due to the feeling that something 'linguistic' is happening here, even though the true nature of that word, as well as its implications has remained largely obscure. One would expect professional linguists to have been on the spot right from the beginning, and to have been the first outside the world of zoology to show an active interest. This, however, did not happen: the linguistic world remained largely uninterested. This lack of interest and participation must be explained in the light of recent developments in linguistics, mainly in the context of transformational grammar, where the chief concern has become to define the universal features of natural language, as opposed to all mathematically thinkable kinds of non-natural, or artificial (computer), language. 'Natural' was then equated with 'human', since no non-human evidence on language was available when this development began to gather momentum in the early sixties.

There is also the fact that in present-day linguistics there is no room for a phylogenetic view on language. Current linguistic thinking is caught in a paradigm that allows for only one dimension of variation, the one along which fully-fledged human languages can differ from each other.¹ General linguistic theory, as it exists today, simply lacks the terms to capture any notion of 'primitive', or 'sub-human', or 'rudimentary' language. No provision for gradability is built into the theory. It is as though a description were given of the anatomy of the human body, without any indication as to the ways in which other species can have different bodies. Such a description would be inadequate for the phenomenon 'body' in general.²

In studies on language acquisition by young children one notices that descriptions of given developmental stages tend to be less integrated in current linguistic theory as the stage is earlier, i.e., further removed from the

eventual, fully-fledged, product. Although this glaring inadequacy should be a major worry to linguists, they have, apparently, chosen to ignore it and rather concentrate on the wealth of problems that can be solved within current theory. This has led to a corresponding lack of openness to facts or developments that disturb the paradigm. Hence, one is inclined to think, the absence of linguists or their hostile, even derogatory, reaction when a quick look is taken (Chomsky, 1976, 40).

The exceptions are very few. Rumbaugh was assisted by the linguist E.C. von Glasersfeld, who co-authored Rumbaugh et al. 1973, but the only published analytical assessment to date of the linguistic achievements of Washoe and Sarah is Mounin (1976). Mounin proposes that the question 'Is there language?' is the wrong question at the present stage of our knowledge. In his view one should rather ask 'Is there communication?' and then proceed to see what kind, if any. Mounin's preference for the latter question is based on the idea that language is an abstract system but communication is a form of behaviour, and therefore more directly open to observation. This distinction, however, does not help us much here, since both language and communication have their behavioural as well as their systematic sides. Language (or 'competence') is to actual linguistic behaviour (speech, or 'performance') what communicative competence is to actual communication. A more useful distinction is made by taking events of linguistic behaviour as a sub-class of the more general class of communicative events. 'Is there language?' is as good or as bad a question to ask as 'Is there communication?'. What we need is clear observational criteria.

Current linguists has developed some theoretical apparatus to specify human language, but not so far anything wider than that, as we have seen. If the term 'language' is by definition restricted to human language only, then the question if other animals have language answers itself: no other species than man is known to possess human language, not surprisingly. One does not need to look at Hockett's (1963) or anybody else's list of putative universals for human language to give this answer. Taken this way, the question is virtually tautologous.

One might say, at this point, that the matter is merely terminological. If 'language' is defined as 'human language', then animals do not have it. But if we allow for a wider definition, which includes all signalling systems, then, under that definition, certain animal species do have language. Yet, to say that nothing more than terminology is involved really is missing the point. The point is that there may well be valid reasons for speaking of a unified and definable phenomenon 'language' across species. Is there a linguistic continuum in nature, we may ask, a phylogenetic scale of language that has a position for every relevant species? At what point in the origin of species did language arise?

We can regard all language experiments with chimpanzees, including the earlier ones, as explorations of the properties and criteria of such a linguistic continuum. To do this is very far from merely establishing terminology. No-one will accept that it is simply a terminological decision to use the term 'vision' for a vast variety of ways of reacting to light waves, including the specifically human way of doing so. It is not just terminology that we do not speak of vision when we see plants react to light. There is a phylogenetic scale of vision along which all species can be compared. Likewise we distinguish sexes, usually two, in a vast number of botanical and zoological species, even though anatomy, physiology and behaviour show enormous differences. No-one will accept that this is no more than a terminological issue. Many more examples, of course, could be given.

Are we any better off for communication? There is no science of communication, as there is for language. But Mounin, who calls himself a semiologist, presents (Mounin, 1976, 3) a definition of communication as a form of behaviour, in terms that are meant to provide a procedure for deciding whether or not communication is taking place:

'(a) there must be someone transmitting and someone receiving, (b) the transmitter must be aware that his target is the receiver, (c) the receiver must be aware of being the transmitter's target, and (d) the receiver must be capable of becoming the transmitter by using the same channel (as a rule, using the same code or one equivalent to it).'

This, however, will hardly do. There is no mention or analysis of the notions 'message' or 'understanding', nor are criteria given for the required awareness. Furthermore, one-way communication would, under this definition, be a contradiction, which it is not. Mounin's attempt at providing operational criteria for deciding on the occurrence of communication can, therefore, be no more than a, perhaps helpful, guide.

The difficulty with both language and communication is that, although we have deep-rooted intuitions about them, these intuitions are too dependent on introspection to serve as a decision procedure. We might say that in the case of language and communication the observable output is small compared with what happens behind the scenes. Yet our intuitions largely relate to the latter. If we wish to speak unequivocally of linguistic or communicative events in the case of animals, we must build up a set of satisfactory observational criteria. However, hardly any such criteria have been found and it is, therefore, premature to expect a quick answer to the question if there is language or communication in the case of our chimpanzees.

We must, anyhow, investigate precisely *what* our chimpanzees do. To do that we must accumulate as much data and experience as we can, in experiments as well as in nature, and make ourselves as familiar as possible with the species. Only then can we feel confident that we are not misinterpreting their behaviour. When we say that we want to find out exactly *what* our chimps do when they make hand signs or move symbols about or press keys, we are referring not to the outwardly visible components of their actions, but rather to the internal, unobservable processes on whose nature the appropriate use of the terms 'communication' or 'language' depends.

Here lies a major methodological hitch. In specifying what is actually happening in the language experiments with the chimps we already look for language-like things, and the selection is guided by some primordial intuition of what constitutes language or communication. There is, contrary to what Mounin proposes, no formal decision procedure to determine exactly what is happening when we interpret behaviour. All we can do, in principle, is formulate interpretations as to the nature of the observed behavioural processes. The best interpretation will be the one that accords most naturally with known behavioural observations and has the greatest predictive power with respect to unobserved events. Any certainty in this area is of an inductive nature: it requires a rich experiential base and it stands as long as no better (i.e., more natural) interpretation is offered. When we interpret behaviour as communication we do so because there is overwhelming evidence to rule out any alternative. A formal proof does not exist. To say that this is 'unscientific' is as unscientific as to deny the possibility of recognizing anger, aggression, attention, play, or what have you, in animal behaviour.

Thus, in the end, we have no better questions to ask than 'Is there language?', 'Is there communication?', and no better grounds for giving answers

than experience and interpretation. So let us ask them. For language, it certainly seems a necessary, though not a sufficient, condition that it should enable those who possess it to engage in communicative behaviour. We can specify to some extent what is implied by this notion. For communication to take place one of the participants (the sender) must produce a perceptible form in some modality. He must do so because he has decided to convey a message. Another, or the other, participant (the receiver) must understand the message, i.e., form a cognitive structure analogous to what the sender had in mind when producing his perceptible form. One further essential aspect, usually overlooked, is the fact that there has to obtain some form of social reality among the participants for communication to take place. That is, the participants' world must contain such elements as obligations, rights, authority. In short, there must be the possibility of a role system valid for the members of a communicative setting. This aspect is closely linked with the primary purpose of communication, which is not to convey meanings, but to take on a role by means of the conveyance of a message. In order to elaborate this point at all satisfactorily, a combined study of the theory of speech acts (Austin, Searle), of linguistics and of social theory would be called for.

If we realize that the outline analysis given here of communication in more or less phenomenological and introspective terms is still highly deficient, and that, moreover, in most cases we lack the operational means to determine whether or not they are correctly applied, we begin to see how far removed from objective and scientific knowledge we are in this respect. We are lucky when it can be established, for example, that some behavioural process, which looks like communication, turns out to be crucially controlled by sub-cortical sections of the animal's brain, whereas what we wish to call communication crucially involves cortical processes (or so it seems). But usually no such clear falsification is possible. In most cases we have to rely on a large corpus of real life observations, on rich experience and familiarity with the subjects. Ultimately, that is, we are caught in our own web of interpretative patterns. The more coherent and the richer they are, the greater certainty we gain.

It is on such grounds, experiential and un-laboratory-like as they are, that we decide that the watchdog's barking is not communication between him and his master. At most it could be communication between him and any possible intruder, although we may have our doubts as to the dog's free choice whether to bark or to remain silent. But the dog's scratching at the window when it has been outdoors long enough and sees me sitting indoors, strongly suggests communication.

A further necessary, but still not sufficient, condition for us to speak of language in an intuitively satisfactory way implies that the perceptible forms used to convey messages are not entirely ad hoc, but systematized in the sense that the subject has acquired a repertoire of signs and a set of rules relating these to certain internal (mental) states or processes. In the simplest cases there would be a one-to-one mapping relation between certain 'global' (i.e., internally unstructured) signs and certain recurrent mental states or processes, which are, in their turn, related to a certain class of situations that are relevant in a given social context. We thus observe, in animals, global signs for 'be careful: danger', or 'I want food', or other mental states or processes. For this to take place the subject must possess sufficient cognitive apparatus to recognize similar forms as well as similar situations; it must be able to match members of either set (the 'grammar'); and it must be capable of conceiving social roles. This implies a fairly high

degree of cognitive and perceptual generalization and abstraction.

Yet we feel that an animal that has achieved this level of systematized, but necessarily limited, communication cannot be said to possess language. We are prepared to credit it with a signalling system, but not language. We begin to be prepared to speak of language when the signs that are uttered are internally articulated or structured, so that a larger variety of messages can be formulated by putting together categorized elements out of a limited repertoire in systematic ways. The mappings are now no longer of the simple one-to-one kind. There now are mapping rules that begin to take on the complexity of what we know as grammar. It is also felt that the categorized building elements must be meaningful in something like the sense that they express generalized abstract cognitive categories and are not limited to categories that are so narrowly defined that they crucially depend on a certain specific type of situation (such as a test situation). If we then have reason to conclude that the internal make-up of the animal's utterances corresponds to the typical structures we find in human language, the position that there is no phylogenetic continuum in nature (even though it probably sprang up relatively recently) becomes quite untenable.

It is no doubt on grounds of internal, grammar-like structure of their animals' signs, as well as the often free and spontaneous occurrence of the signs that the experimenters have believed, on mature reflection, that their subjects' behaviour should be interpreted as an incipient form of linguistic communication. Their interpretation is shared by large sections of the academic community as well as by many outsiders. Washoe and Lana, in particular, behaved in every way to convince their observers that they produced perceptible forms as a result of their own decision to do so; that in so doing they assumed a social role; that they intended a message to get across; that they were using not only systematized global signs but also constructions that followed patterns of well-known human linguistic constructions. Washoe is known to have used adjective-noun combinations, as well as compounds, such as 'water-bird'. At the moment she seems to have stolen the show. This does not mean that her achievements must be more convincing than those of other chimps: her success may be due to a lack of adequate information. If reports circulating, but as yet unpublished, are correct, we must credit Lana with very high achievements too. It is reported by direct observers that Lana regularly asks for names of objects; that she refuses requests by answering 'no'; that she uses descriptive phrases for objects she does not know, or has forgotten, the name of ('Please open that which is shut', having forgotten the word of 'door'); that she produces uncalled for, i.e., spontaneous, descriptions of objects in sight ('That there be shoe, blue').

It is, therefore, difficult to agree with Chomsky, who insists (Chomsky, 1976, 40) that

'free, appropriate, and creative use of language as an expression of thought, with the means provided by the language faculty, is ... a distinctive feature of the human species, having no significant analogue elsewhere.'

It should be noted that his term 'creative' (although he tends to use it in non-technical, philosophically oriented contexts and for a non-specialist audience) implies the technical notion that a grammar of a human language characterizes a denumerably infinite set of sentences.³) Let us suppose that chimps can only manage grammars that characterize finite sets of sentences. Would that mean that there was no 'significant analogue' of language? One might just as well argue that the ancient Greeks, who had no concept of de-

numerably infinite sets, did therefore not practice arithmetic. As the evidence stands, it is humanly reasonable to accept that man has achieved linguistic communication with animals.

Yet, as has been stressed before, this can be no more than a matter of interpretation. Enthusiastic observers might, on occasion, jump to conclusions a bit too quickly. Lenneberg (1975) has reported how he trained two normal high school students with the procedures described by Premack. Although his subjects quickly mastered the system and achieved a lower error score than Sarah ever did, it never dawned on them that they were using a system that could be employed linguistically. 'They were under the impression that their task was to solve puzzles', says Lenneberg, and 'were unable to translate correctly a single sentence, completed by them, into English'. He suggested that, therefore, Sarah's comprehension ability should be tested by more general and objective methods than heretofore.⁴

Given all this, there is a further, more specifically linguistic, question that poses itself naturally here. It has become clear that human language, as opposed to other mathematically definable forms of language, is characterized by a highly specific and quite complex set of restrictions. A substantial part of the efforts made by theoretical linguists these days consists in trying to detect and formulate them, as well as to determine the implications of this fact for theories of human cognition and human learning. Allowing for some divergence of opinion one can say that there is widespread agreement that these linguistic restrictions reflect a species-bound, innate set of strategies and expectations with respect to language, which forms part of the cognitive equipment of every new-born human being. Humans, in other words, are prewired for human language, as they are, for example, for walking upright.⁵ This linguistic prewiring is often designated by the term 'language faculty'. Although it allows for sufficient variation for there to be many different languages, it is so narrowly limited that it will help to explain how any child manages to learn the language of its environment, i.e., to select the correct grammar for that language, infallibly, within a few years. To what extent the restrictions on language are unique to language or follow from more general principles, cognitive or other, still is an open question. There is some point in speculating that, at least in part, they are a consequence of the structure of the underlying thought processes on the one hand, and of the physical and physiological properties of vocal production and auditory perception on the other.

In this context, the linguistic achievements of chimpanzees give rise to the following question: To what extent is any chimpanzee language faculty defined by the same, or analogous, constraints? Is the chimp only *capable* of acquiring something approaching human language, as a result of laborious training, or is he also *predisposed* towards strictly analogous forms of language?⁶ Any non-trivial similarities would make the linguistic continuum stand out more clearly.

Given the present state of inquiry into chimpanzee language faculty it may seem totally unrealistic to even begin to think of comparing grammatical constraints. For most constraints this is indeed so. Yet there is at least one human linguistic universal that seems testable, in the given circumstances, for chimp language. It has to do with the connective rendered as *or* in English.

Or in human language behaves in many ways problematically. It is often felt to be 'exclusive', as opposed to the 'inclusive' *or* known in logic. Its logical properties in language sometimes conflict with those in logic. It is generally a puzzling word, but, no matter how puzzling it is, its behaviour is remarkably

similar in all different languages. One of its typical features is that in some cases it corresponds to semantic *and*.⁷ It is well known that full propositional structures linked with *and* can be reduced in certain regular ways. Thus for

(1) Take peanuts *and* take chocolate.

we get:

(2) Take peanuts *and* chocolate.

If, however, both propositions are negative, *and* is naturally changed into *or*. Thus,

(3) Don't take peanuts *and* don't take chocolate.

is naturally shortened to:

(4) Don't take peanuts *or* chocolate.

When *and* is used, the meaning is distinctly different:

(5) Don't take peanuts *and* chocolate.

The switching does not work conversely. We do not naturally shorten

(6) Don't take peanuts *or* don't take chocolate.

to (5). It is to be noted that the *and*-to-*or* switch is restricted to those cases where the negation precedes the connective in the reduced form. Where the connective precedes, it may remain *and*:

(7) Jim mustn't take peanuts *and* Jerry mustn't take peanuts.

may be reduced to:

(8) Jim and Jerry mustn't take peanuts.

But even here a form such as:

(9) Neither Jim nor Jerry must take peanuts.

seems preferable. It appears, quite generally, that *and* in human languages has a tendency to resist negation within its scope. It tends to be changed into *or* and be itself in the scope of the negation. This is one example of a universal restriction on the form of human language. It has remained totally obscure, as yet, why this should be so. Logic provides no explanation, nor has psychology been able to provide one. But the fact remains that *or* behaves this way.

At the Institute of Psychiatry in London, Miss D. Brown, Prof. G. Ettliger and myself are currently engaged in investigating to what extent chimpanzees make a crucial use of temporal order of linguistic elements to distinguish meanings in a systematic way. The medium used is hand-signing, based on the Paget-Gorman system for the deaf, practised in Great Britain.⁸ If it is established that order does, or can, act as a semantic discriminator, then, provided some form of simple negation, straightforward *and* and straightforward *or* can be taught them, attempts are to be made to see whether sentences of a structure analogous to (4) are interpreted in a way analogous to (3), whereas sentences such as (5) will be interpreted as 'not (p and q)'.⁸

Given the few months that the experiment has been going, it is difficult to claim any significant results at this stage. What is needed first is a fairly advanced stage of learning: the subjects must be able to handle *not*, *and* and *or* in simple ways. Only then can we put them to the crucial test.

Any positive result will be significant with respect to the notion 'linguistic continuum'. Human linguistic universals will then be seen to have replicas in lower species. One specific question, however, will then become urgent. If chimpanzees are in certain ways predisposed towards learning a specific form of language, why do they not seem to develop and exploit this faculty spontaneously in the wild? It is true that not nearly sufficient research has been done to find out how rich their natural gesture repertoire is.⁹ Yet it seems safe to assume that apes in the wild come nowhere near our experimental chimps in their communicative achievements.

Various answers suggest themselves. One might think of the possibility that an innate language faculty is a necessary, but not a sufficient condition for the actual appearance of language. Some other necessary factor, such as 'drive', 'urge', or 'curiosity' might also be required. But it is also possible that man's innate language faculty is not itself implanted as such in chromosome structures, but is the result of certain more general principles of cognitive functionality which, when called upon in specific ways during certain stages of the individual's development, generate the universal restrictions that characterize human language. The chimp might to some extent share these general principles, and apply them to linguistic material when called upon to do so in an appropriate way, i.e., by using signs and not vocal signals, in the right stage of development and in the right environment. But the time has not come yet to force an answer to these questions.

NOTES

1. The grammars for individual languages, in their turn, do not allow, in current theory, for any internal variation. The inadequacy of this monolithic view of grammar has now begun to worry linguists, in the wake of renewed interest in sociolinguistic and dialectal variations within the same language area. The difficulty is that, in spite of the obvious empirical success of transformational theory, the grammars defined in its terms lack the apparatus to deal with internal variation. And it is not clear how they could be extended or supplemented to fill this gap. (Cf. Dittmar, 1976, 132-3.)
2. Note that, like language, the notion 'body' presupposes a mind. In general terms, this complicates the definition of 'body' as much as it does for language.
3. See, e.g., Chomsky, 1972, 100: 'Having mastered a language, one is able to understand an indefinite number of expressions that are new to one's experience, that bear no simple physical resemblance and are in no simple way analogous to the expressions that constitute one's linguistic experience; and one is able with greater or less facility to produce such expressions on an appropriate occasion, despite their novelty and independently of detachable stimulus configurations, and to be understood by others who share this still mysterious ability. The normal use of language is, in this sense, a creative activity. This creative aspect of normal language is one fundamental factor that distinguishes human language from any known system of animal communication.' Note that gradable terms are used ('*simple* resemblance', 'in no *simple* way analogous', 'with *greater* or *less* facility'); yet no values are defined for 'normal use of language'. It may well be that for certain high values on these scales Chomsky is right with respect to animals lacking this creative aspect of language. But it follows by no means that there is 'no significant analogue'.
4. Cf. also Davis and Gardner, 1976. They have developed a new form of aphasia therapy consisting of a visual communication system intended to replace spoken language. They write: 'Even when patients appear to have mastered the basics of this system, the extent to which they appreciate its communicative potential, as opposed to regarding it merely as a sophisticated card game, remains indeterminate.'

5. For a popularized but adequate exposé of these questions see Aitchison, 1976, 51-143.
6. Aitchison (1976, 50) pre-empts the question by asserting: 'But although intelligent animals seem *capable* of learning language in a rudimentary fashion, they do not seem *predisposed* to learn it.'
7. See Seuren, 1974, for an attempt at grammatical explanation of this particular phenomenon.
8. The Paget-Gorman signing system is essentially a form of morphemic spelling, following closely the succession of English morphemes, including morphological endings. In the experiment only lexical signs are borrowed from the P-G system, and consecutive order of elements is maintained (simultaneous signing being avoided), whereby roughly English word order (subject-verb-object) is followed.
9. Thus, e.g., Dr. A. Kortlandt of Amsterdam pointed out to me that there is a natural and universal hand sign among chimpanzees to indicate caution and uncertainty while proceeding: they raise one hand and hold it horizontal but curved, palm down, above the nose and just under the eyes. Apparently, this had not been noticed before by anyone working with chimpanzees, either in the wild or in institutes.

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