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Intentionality, syntactic structure and the evolution of language[1]

NEIL TENNANT

INTRODUCTION

> . . . imagine a class of beings who were capable of having intentional states like belief, desire and intention but who did not have a language. What more would they require in order to be able to perform linguistic acts? Notice that there is nothing fanciful in the supposition of beings in such a state, since as far as we know the human species once was in that state. Notice also that the question is conceptual and not historical or genetic. I am not asking what additions would need to be made to their brains or how language did evolve in the history of the human race . . .
>
> The question needs to be made narrower, because there are all sorts of features of actual languages that are irrelevant for our present discussion. Presumably such beings would need a recursive device capable of generating an infinite number of representations, they would need quantifiers, logical connectives, modal and deontic operators, tenses, color words, etc. The question I am asking is much narrower. What would they need in order to get from having intentional states to performing illocutionary acts?
>
> The first thing that our beings would need to perform illocutionary acts is some means for externalising, for making publicly recognisable to others, the expressions of their intentional states. A being that can do that on purpose, that is a being that does not just express its intentional states but performs acts for the purpose of letting others know its intentional states, already has a primitive form of speech act. (Searle 1979a: 193–4)

I beg my reader's forbearance for beginning my own paper with such a long quotation from another. It happens to mention almost every topic I shall be discussing here. I hope to show that the phenomenon of recursive syntax may bear on the intentional ingredient in human communication. I hope also to say something useful about the structure of primate brains as well as the mechanisms of linguistic evolution, both issues that Searle sets to one

[1] The earliest version of this paper was read to the Epistemics Research Seminar in the University of Edinburgh in 1980. A longer and rougher version was the basis of the Thyssen Group discussion. Its present form owes much to the criticism offered on that occasion. John McDowell challenged the privileged status of what I have here called the first scenario; part of the paper now responds to that challenge. I am grateful to Peter Lamarque, William McGrew and Arnold Chamove for drawing my attention to useful references. Less directly, but no less importantly, this paper is the result of Florian von Schilcher's encouragement to think in evolutionary terms.

side as irrelevant to the conceptual question he is pressing. This paper accordingly falls into two not wholly unconnected parts – one philosophical, the other biological.

Language has obviously evolved. But it has left no fossil record. Deficiency of sufficiently diachronic facts ironically testifies to the tremendous selective advantage language must have conferred upon its users.

Evolutionary theorising is a matter of making inferences to the best explanation. Today such inferences about the origins of language can draw on a vast assemblage of data and hypotheses in neighbouring disciplines. Those who belittle the value of new integrative speculation are, in a phrase of Bennett's, dogmatically defeatist. To be sure, the Linguistic Society of Paris in 1860 banned publication of the theorising about the origins of language then in vogue. But in the century since we have come to understand better the structure of natural language, and have made some conceptual progress in the philosophy of language. We have learned much about our own brains and vocal tracts, and those of our primate cousins. Ethologists have studied many different systems of animal communication. Darwinian theory has been synthesised with modern genetics. We have attended closely to how children learn to speak. In the light of all this, I do not think it premature to put forward an evolutionary scenario for natural language.

1

It is a philosophical commonplace that we cannot learn very much about human language from the singing of birds or the dancing of bees. Systems of animal 'communication' are held to be mere signalling systems, systems for which there is some doubt as to the meaningfulness of the messages contained therein as opposed to their characteristic causal efficacy in evoking or triggering certain responses.

The dance of the bees has a repertoire of wiggles and tilts and speeds. Certain features of their performance are found to be correlated with and hence to convey information about direction and quality and distance of food source. But do the movements 'convey information' to fellow workers in the hive in any more *semantic* a fashion than, say, a bruise on a child's face conveys to his mother the information that he has had a nasty bump?

At first sight, it is unclear what the answer ought to be. The bruise is a direct causal consequence of the bump. Knowledge of human physiology would enable witnesses of the bump to predict the appearance of the bruise. Similarly, detailed knowledge of bee physiology should enable one to predict that the bees' locating a food source will have as a causal consequence (even if only statistically) the subsequent pattern of wiggles and tilts

in the hive. For bee dance is inflexible, and can be manipulated genetically (cf. T. Eisner & E. O. Wilson 1977: 241).

So is it just a matter of degree? Can one argue that the bees' dance is as much a causal consequence of this earlier experience as the bruise was of the bump? Compare Armstrong:

> ... utterances of sentences in the communication situation are signs in exactly the same sense of the word 'sign' that black clouds are a sign of rain ... an analysis of linguistic meaning will be given in terms of this basic, black cloud, sense of 'signify'. (1971: 429)

If he is correct, then the 'semantic' status of apine dance, if we suppose the bruise to lack such status, is to be recovered from certain distinctive features of the underlying causal story.

Indeed this is a challenge that even the materialist should be willing to accept. He can begin to analyse the distinctive features of communicative interactions while still using the language of the mentalist. That is, he can attribute beliefs, desires, intentions, and so forth in giving a simpler account of a complex matter. The complexity of the account will increase vastly when replaced by ultimate 'physical' talk, if this indeed be possible. Nevertheless, the complexity imparted by the faithful physicalist version should not blind us to the features that distinguish the causal processes in the bees' brains or whatever from those involved in the swelling after the swipe. The difficulty lies precisely in spelling out what these distinctive features are, and how they are pertinent to the contrast between conveying information on the one hand, and, on the other, being black and blue.

It may turn out that the distinctive features of a causal story associated with semantic content are precisely those that require the language of belief, desire and intention, in order for their isolation to have any point within the overall causal explanation of what happens when the speaker communicates with another. Ironically, too, it could turn out that much less goes on physically when John sees Mary and tells Dick about it, than when John gives Mary a black eye that tells Dick of his blow.

So what is there to linguistic meaning, over and above mere causation? One might offer the following reason why one could regard apine dance as having communicative significance lacking in the case of the bruise; that the dance is a somehow 'arbitrary' or 'rule-governed' causal product of the sighting of food. It is arbitrary in that the rules (consciously followed or not) in accordance with which this behaviour is produced, *could have been otherwise*, while yet serving the same communicative purpose. That is, as a piece of adaptive behaviour, whether wholly instinctual or partly learned, it may very well *now* follow as a causal consequence of the sighting; but that precisely *this* sort of dance should have come to serve this purpose is, in a

phylogenetic perspective, quite accidental. Selective pressures for a system of communication of this kind might have produced, through different mutations for new behaviour, a different dance or indeed wholly different behaviour types, serving the same function. In fact, another species of bee that communicates about food sources (*Apis florica*) does so in the same way except in so far as it wiggles in the horizontal rather than the vertical plane; and so can indicate directly the angle of the food source from the sun. *Apis mellifera* (the common bee) has to have a convention as to which angle in the vertical plane corresponds to this latter angle. *Apis mellifera*, if put on a horizontal honeycomb, dances just like *Apis florica*.

This 'conventional' behaviour is described with scare quotes because it is not clear that one would be justified in reading into it the constellation of reciprocal beliefs and intentions that someone like Lewis takes as constitutive of conventional behaviour. The bees' behaviour has developed phylogenetically and of course may now be regarded as a more or less necessary causal consequence of their finding food. The analysis is to hold whether or not the striking similarity between the dances of the two species is the result of their common descent from a dancing ancestral species, or the result of this sort of dance's being an optimal solution to the 'evolutionary problem' of efficient communal food use. With enough genetic mutations at hand, the behaviour could perhaps have evolved independently in each species. This phylogenetic perspective *appears* to separate nicely what is essential in the dance to its being a signal or message of some kind. The dance constitutes a rudimentary system of communication, whether or not it is entirely instinctual, entirely learned or the result of a mixture of the two strategies.

But to see communicative significance as deriving from the phylogenetically arbitrary status of apine dance, as a solution to their coordination problem, is open to a serious objection.[2] This is that the dance might have been the only way, given apine physiology immediately before its evolutionary debut, to solve that problem. If a certain behaviour pattern were, in some suitable sense, phylogenetically necessary, what would justify one in attributing content to it?

Now although in evolutionary terms, given the amounts of genetic variability usually at hand, it is likely that such behaviour has been arbitrary in the required sense (witness *Apis mellifera* v. *Apis florica*), one must concede the conceptual point here. Armstrong even goes as far as asking 'is it not conceivable that the whole of syntax and semantics should have been innate so that all mankind spoke the one, wired-in, nonconventional language?' (1971: 437).

What, then, justifies the attribution of content to exercises of evolved forms of behaviour? A promising line of thought, due to McDowell, is that

[2] This objection I owe to David Hull.

all one requires is *transfer of information* simpliciter. This holds regardless of whether the mechanism, behavioural, chemical or otherwise, might have evolved arbitrarily or might have been the only possible one phylogenetically. The natural function of this communicative behaviour, whether wholly instinctual or not, is to impart to fellow creatures 'cognitive stand-ins for the states of affairs they represent'.

> ... communication is the instilling of information. Consider, first, instinctive communicative behaviour like that of the [bees] ... The function of such behaviour is to furnish information about the environment to [bees] which witness it; here 'function' occurs in something like the sense in which it is the function of the heart, say, to circulate the blood. When what gets transmitted is misinformation, there has been a malfunction of a natural process. A malfunction is as such a defect ... Aims pursued in communicating do not enter the story. There are no such aims since the behaviour is instinctive. But in an account of the (no doubt rudimentary) notion of content which seems undeniably applicable in this case, the natural function of the behavioural repertoire can serve, as it were, instead; it can occupy a position analogous to the position which was supposed to be occupied in an account of the notion of the content of an assertion, by the alleged fact that in making assertions, we aim at truth.
>
> When the communicative process functions properly, sensory confrontation with a piece of communicative behaviour has the same impact on the cognitive state of a perceiver as sensory confrontation with the state of affairs which the behaviour, as we may say, represents; elements of the communicative repertoire serve as epistemic surrogates for represented states of affairs ... elements of such a repertoire represent states of affairs by virtue of standing in for them in a creature's cognitive dealings with the world. (McDowell 1980: 133–4)

Compare now the case of the bruise. Bruising, it is reasonable to suppose, has never served the adaptive function of informing others that the body has been hurt. It is rather the kind of causal concomitant of the blow that has wholly to do with internal processes of tissue regeneration. So far, so good; we have separated bees from bruises. What about us? Is not a belief/intention ingredient essential to the notion of linguistic communication in the human case? Certainly the Gricean analysis would have it so. Grice himself might assimilate apine dance to the category of phenomena that have what he called natural meaning as opposed to the non-natural meaning of expressions of natural language. The question that McDowell is pointing to, however, is whether, in the analysis of human communication, Grice's contribution concerns more what it is to be human than what it is to communicate. Let us for the time being not take a stand on this issue but address ourselves to the whole phenomenon in its strong sense.

Whether a system with finitely many basic signals is a system of communication in the strong sense – that is, a linguistic system – depends on whether the characteristic behaviour within which and against which it

is interpreted is complex enough to sustain attributions of higher-order beliefs and intentions to the creatures concerned. If one has to resort to such an intentional framework in order to make sense of the behaviour, in order to explain what the creatures are doing and why, and has so to resort after exhausting all possibilities of more austere, less anthropomorphic frameworks of explanation, then one is on the route to linguistic meanings. Wittgenstein invites one to consider a primitive signalling system as the system of communication of a whole tribe. We can aquiesce in this thought experiment only because we implicitly assume that enough behavioural complexity will be discovered for us to regard tribe members as having beliefs and intentions, even if, *ex hypothesi*, their very simple language does not permit them to express these beliefs and intentions. Provided a tribe member can recognise an intention that he should respond appropriately to a signal in the simple system, we are happy to regard the signal as linguistically meaningful to him, rather than simply causing or triggering certain behaviour – as was the case with the bees.

When radically interpreting the speech of a totally alien tribe (or species), one has to consider all the evidence there is concerning mental states. Their attributions go hand in hand with, and must be adjusted to, our attributions of meaning to utterances. One requires a background of considerable behavioural complexity before one is justified in attributing to any creature beliefs, intentions and so on.[3] Davidson (1975) would even maintain that this requires that they have mastery of a language; that it is wrong even to think in terms of quasi-beliefs or proto-beliefs when trying to explain even quite complex behaviour of languageless creatures. This insistence, however, does not mesh well with an attempted evolutionary account of language, which it is my concern to sketch. It appears unduly insistent on the primacy of certain conceptual connections between cognition and language.

Nevertheless, it is easy to underestimate just how complex behaviour has to be before attributions of attitude and meaning pass methodological muster. Davidson rightly enjoins the radical interpreter to be *nasty* in thinking up as many competing interpretations of observed behaviour as possible. A good example of how nasty one can be is to be had from the

[3] There is little force in the objection that we presently have no precise way of measuring or rank ordering degrees of behavioural complexity. The same is true of deductive complexity in mathematics, but every mathematician can intuitively judge that one proof is deeper or more complex than another (or at least there are clear cases where this is so). Advances in cybernetics and our study of the complexity of programmes may one day yield the sort of measure or ordering one seeks. The complexity of a behavioural 'flow chart' might depend on its number of sensory inputs and motor outputs and on how feedback loops are nested, and so on. Alternatively, one might study the logical complexity of the experimental tests that we intuitively require an animal to pass before attributing various intentional states to it.

chimpanzee language experiments. A scholarly audience will forgive this quote from *The Listener*:

> The Gardeners first taught Washoe by shaping her hands for each sign. Hands together means – 'more'. When Washoe got close, she was rewarded by getting more of what she wanted. Soon she was using the sign for more of anything – food, rides, and so on – proof that she had grasped the general concept of the word.

Let us be nasty towards Washoe. The word 'more' has its meaning disclosed through combination with terms telling one what it is of which 'more' is in question. No doubt the word as used by infants will occur on its own, without the added term, and adults satisfying the need will work out the missing term from context. But adults respond linguistically too, supplying the missing substantive with the wanted substance. The child in due course in new contexts uses the modifier with substantives, thereby expressing his desires more clearly. The process of learning words, learning their grammatical categories and acquiring them in correct combinations is very much a two-way affair. It involves requests, solicitous enquiries, offerings, forbiddings and factual commentary. But does Washoe use the hand signal, allegedly meaning 'more', in this way? Does she offer more of things to her interlocutors when 'asked', as well as demand things from them? Does she perform several different speech acts with the word, questioning, commanding, wishing, stating? Watching the film of Washoe with Leakey's commentary, the nasty radical interpreter is struck by any number of competing interpretations of Washoe's signing. Why not interpret her as expressing something tantamount to 'I have an unfulfilled desire', or, 'make me feel nice!'? Either of these – quite unconnected with 'more' of anything – would cohere with the facts just as well as the 'more' hypothesis and explain the same range of behaviour. If Washoe has an unfulfilled desire, one can rest assured that the eager and attentive experimenters will gather from context what it is and fulfil it if at all possible. It is not unlikely either that the formation of the desire will be closely linked with current experiences and will therefore probably be fulfilled by providing more of whatever is salient in the context – porridge, or rides in a plastic tub, etc. Likewise the hand sign might mean 'continue' or 'would that things remain roughly as they are now, in lumps or in intervals'.

We have so far been considering the extent to which the intentional ingredient in natural language distinguishes it from other systems of animal communication. Another point that some have raised in this connection is that animals cannot – or do not – lie when using their communication systems. The force of this objection depends, again, on the complexity of the attendant beliefs and intentions which analysis demands in the case of lying. Let us disregard the moral overtones which are irrelevant anyway to

the concept of lying (what makes lying bad is what is bad about what makes it lying).

In communicating that 'P', one is lying just in case one believes 'not P' but intends one's audience to believe 'P'. A communicative act is a lie if by ascribing the appropriate belief and intention to produce in the audience the opposite belief, one achieves the best explanation – within one's theory of the agent's beliefs, desires, etc. – of his act. Consider now the case of a bird 'warning' its flock members near a food source that a predator is nearby. The other birds fly away leaving the warner with a temporary monopoly of the food. Has this bird lied? We must first enquire whether the bird believed there was no predator. That he did not himself fly away is strong evidence for this belief, especially if predators prefer to attack lone birds, and we have further reason for thinking that the bird believes *that*. Furthermore, we could investigate whether he behaves likewise when there is no food source to be monopolised. Secondly, we must ask whether he intended his fellows to believe that there was a predator nearby. To test for this intention we would have to establish whether he would act as he did in situations where he had no reason to believe that he could thereby induce the false belief in question. But this is almost impossible to do, even by recourse to highly contrived situations not encountered in the wild. To this extent, the status of the call as a lie is under-determined. To the biologist, it may seem unnecessarily convoluted to eschew the simple hypotheses that the bird is lying, given its useful role in making broad predictions about the future.[4] And at least one philosopher (Routley 1981) is prepared to defend them in this view. But for the radical interpreter, this would be to throw in the towel at a crucial conceptual conjuncture, imputing our intentional framework to another, dumb species.

Yet it is arguable that the child's first experiments with what adults would regard as linguistic deceit are on a causal and behavioural par with the bird's fraudulent warning cry. Both might just by accident hit on the fraudulent behaviour eliciting the desired response, and subsequently adopt it as a strategy in such situations.[5] The same holds true of any other strategy adopted as a result of operant conditioning. A significant difference might arise when the child learns about the possibility of detection and punishment after lying and its various moral implications. The latter, though, might just be assimilated to the factors that the subject has to take into account under operant conditioning. The trouble by this stage, however, is that an adequate account of the effects of

[4] Cf. also n. 2. Premack and Woodruff (1979), however, are well aware of the complicated controls needed before attributing to chimpanzees an intention to deceive.

[5] I owe this point to Florin von Schilcher.

admonishments, threats and exhortations would enmesh one further in the semantic question posed by the original problem.

Some may see the bird's warning cry as falling short of lying for a further reason. This is the absence of any higher intention, or evidence for such, that the receiver should believe something by recognising this very intention. This extra Gricean ingredient in the conceptual analysis of linguistic meaning applies not just to cases of lying but to linguistic communication in general.

So far we have been discussing the intentional aspect of language in our comparison of human with other animal communication. But we have not lost sight of the important role to be played by considerations concerning range of vocabulary items, methods of syntactic combination, discreteness of signals, and the associated semantic possibilities. Among these are reference to spatio-temporally remote items, generality and tense – to name but a few. They are among the most important semantic features of human language for which one would understandably be hard put to find correlates in other known systems of animal communication. One would be hard put also to find grounds for the attribution of the beliefs thus expressible to creatures that might be thought to have but not express them:

> No doubt past beliefs are also caused by past experience; but what I am denying is the converse claim that any belief which is caused by the past must be or involve a belief about the past ... sometimes ... the attribution of a past belief can be effectively challenged by a lower-level, more economical attribution in which the past is introduced only causally and not epistemically. That is why I prefer more demanding standards for what it is to count as a behavioural manifestation of a past belief. (Bennett 1976: 103)

Considerations concerning syntactic and semantic structure are not unconnected with those about the beliefs and intentions involved in linguistic communication – beliefs and intentions which McDowell suggests are quite secondary once one has accepted that communication in general (from simple instinctive types through to natural language) has first and foremost to do with the transfer of information, or the instilling of knowledge. Let us put aside for the moment the misgiving that the latter phrase ushers in the intentional ingredient once more. No doubt McDowell could make do with first-order 'quasi'-knowledge in a theory of cognitive representations for 'lower' species (in something of the form suggested by Field 1978), stopping short of the special problems posed by the diagnosis of the higher-order beliefs and intentions involved in a Gricean or sub-Gricean mechanism (cf. Bennett). In doing so he might find himself in the company of evolutionary epistemologists such as Riedl (1979), whose over-arching theory of life as an 'erkenntnisgewinnender Prozess' seems to require a unitary notion of knowledge or information, information that can be stored in a genome at

one end of the evolutionary spectrum, as well as be expressed, at the other end, by scientific theories that make the world a less strange place to live in.

But what I would wish to suggest is that other considerations of an evolutionary kind lend weight to the view McDowell is challenging. To do so I must sketch the bare outlines of the opposing views on the conceptual issues, to see which one harmonises more with evolutionary conjectures about language. (In this paper I cannot go very far into the available wealth of fact and conjecture in the neurophysiology of language, psycholinguistic theories of language acquisition, and comparative studies of man and other primates. Fuller justice to these topics, as well as the ape language experiments and various glottogonic theories, is to be done in a longer study.) The first evolutionary scenario, which places more emphasis on the intentional ingredient than McDowell would allow, is as follows.

Following Bennett, we regard it as admissible to frame theories of perception (or 'registration') and of 'proto'-belief and desire to explain the actions of certain creatures, provided their behaviour is complex enough, and subject also to the usual caveats of holistic method. But importantly (as behaviour proves more complex), the theory allows attribution of belief and intention *at higher order*, even in the absence of language. Then, when higher-order beliefs and intentions of interactants engage in a suitable way – by instantiating a sub-Gricean mechanism, say – certain actions (token events) can have *occasion meaning*. This is meaning in as rich an intentional sense as one is likely to get – far more than the mere instilling of (first-order) knowledge with which McDowell deals. Now, says Bennett, we can move from meaningful event tokens to conventional meanings of event types. For we are dealing, *ex hypothesi*, with creatures who can grasp the salience of certain features of past occasions of meaningful exchange. Lewis's theory of convention is then applied to account for the acquisition of conventional meaning by certain action (or utterance) *types* in the developing linguistic life of the group. But what is of crucial importance in the account thus far is that these action types are syntactically and semantically *unstructured*.[6] At this point in Bennett's exposition there is an interesting leap (between chapters 7 and 8). Having accounted for how action types might acquire conventional but unstructured meanings, he advances straightaway to a discussion of how one might come into an alien community and find

[6] It is worth noting here that the action types need not involve speech. One of the most promising theories about the origin of language – and surely the one most strongly supported by the results of various ape language experiments – is that natural language had a gestural origin. Moreover, the structure of spoken sentences need not have derived entirely from unstructured words grafted onto single gesture types. Gesture types, as achievements of deaf children indicate (Goldin-Meadow 1975), might already have had a rudimentary grammar allowing their combination into longer messages, before speech overlaid the substrate of gestures. But the 'problem' facing the deaf children was to find a channel of communication. The problem facing our forebears was to 'discover' linguistic communication.

evidence that their linguistic interactions are *structured* (syntactically and semantically). Yet he offers no account of how the language of the group could have attained such complexity of structure from the humble (albeit highly intentional) beginnings he had been discussing earlier. Let us call this the structural lacuna of the intentional scenario.

Compare now McDowell's account – in one way more austere, in another much more radical – which suggests a second scenario.

> A more attractive line of thought is that the linguistic repertoire retains, through the alteration of nature involved in the onset of self-consciousness, a form of the characteristic which was essential to its pre-linguistic ancestor: in suitable circumstances (to be spelled out in any fuller elaboration of this idea) its exercises are cognitive stand-ins for the states of affairs which they represent. An assertion will actually have that epistemological role only if the circumstances are right. But all standard assertions – excluding, that is, special cases like irony – purport to have it. Thus their possession of content – their capacity for representing states of affairs – is intelligible in terms of a suitable modification of the simple idea which seemed appropriate in the case of instinctive communication. (1980: 135)

This opens the possibility (and one acknowledged in conversation) that communication could involve *structured* messages, perhaps even structure of the sort producing infinite generative capacity, without the creatures ever having passed the 'Gricean hump' that Bennett put them past even before occasion meaning had made its evolutionary debut.

It is worth pausing here for a moment to reflect on the weight of traditional thinking that McDowell is seeking to shift:

> ... the utterance '(There is yellow fever on board)' ... is a sign that (the speaker has the objective that an audience should have reason to believe {that the speaker believes [that (there is yellow fever on board)]}) ... A, let us say, utters the phonemes that encode the sentence 'There is yellow fever on board'. B hears the phonemes. B *infers* (just as one infers from any sign) that A made this utterance with the object that an audience should have reason to believe {that A believes (that there is yellow fever on board)} ...
> if B makes [this] very first inference ... then *linguistic* communication, at least, has been achieved ... uptake has been secured. An illocutionary act has occurred. B has understood A's words. (Armstrong 1971: 432–3, 435)

> ... in order to make an assertion a speaker must represent himself as believing what he says. (Davidson 1979: 12)

> Language is derived from Intentionality, and not conversely. The direction of *pedagogy* is to explain Intentionality in terms of language. The direction of *analysis* is to explain language in terms of Intentionality ...
> ... in the performance of each illocutionary act with a propositional content, we express a certain Intentional state with that propositional content, and that Intentional state is the sincerity condition of that type of speech act. Thus, for example, if I make the statement that p, I express a belief that p ... All of these

connections, between illocutionary acts and expressed Intentional sincerity condition of the speech act are internal; that is, the expressed intentional state is not just an accompaniment of the performance of the speech act. The performance of the speech act is necessarily an expression of the corresponding Intentional state ... (Searle 1979b: 75, 78)

What McDowell is challenging is the notion thus unanimously upheld that the belief-expressive feature of assertions is essential to our understanding of the extra-linguistic purpose of statements of a natural language: challenging it, indeed, even for statements with syntactic structure. Indeed, in the first of his two papers cited here, Searle had wavered:

The primary extra-linguistic purpose of having the institution of assertion is to give information ... our beings would be capable of making a primitive form of assertion when they could perform actions *which were expressions of belief* for the purpose of giving information ... (1979a: 194, my italics)

Is the italicised clause incidental or essential? On McDowell's view it is not essential insofar as one is concerned just with explaining how language evolved to something like its present state of syntactic complexity, as selection favoured its communicative successes.

His view would be strengthened by scotching the appeal made by both Searle and Armstrong to Moore's paradox. (This appears to be the only independent support, apart from introspection, that they adduce for belief-expression's being essential to assertion.) But surely on the more austere view one can easily explain why it should be so peculiar to assert 'p but I don't believe that p'. The object language is already taken to contain attitude operators and the first person pronoun.

On McDowell's account, it would presumably be either as the language acquired such new structure, developing something like a pronoun system and (iterated) belief attributions or as, quite independently, *behaviour* became more complex, calling for higher-order indexical belief states for its explanation (cf. Mellor 1980a) that consciousness dawned: in particular, consciousness of what was happening when one *communicated*. With the evolutionarily intrusive possibility of linguistic deceit, as opposed to mere malfunction of communication, communicative exchanges would then come to be understood as having truth as their aim; and the Gricean template would come to be true of the pursuit of those aims. So too would utterances of Moore's form acquire their paradoxical force. It is only necessary, for the paradox to have its force, that an assertion that p *provide grounds for believing* (rather than: *express*) that the speaker believes that p. Now whenever I speak, this provides grounds for believing that I have vocal chords; but it does not *express* that fact. Is it not just as strange for me to state out loud 'p, but I don't have any vocal chords'? If one believes the second

conjunct, one must invoke an unusual explanation for the sounds emanating from my mouth. Likewise in the case of Moore's paradox.

What I wish to suggest is that filling the structural lacuna makes structured talk without higher-order thought look much less likely. Even if one were to concede to McDowell that there could be traffic in simple message types before the Gricean hump, I wish to maintain that one would have to be over the hump (or at least capable of being in such states as are involved in the hump) before one's language could evolve syntactic structure of the kind yielding infinite generative capacity. I stress 'of the kind yielding infinite generative capacity' because it is the peculiar combinatorial potency of expressions in natural language which, it seems to me, could not have emerged by any evolutionary mechanism unless the users of the system were capable of the sort of second-order intentional states involved in the Gricean mechanism. I am quite willing to countenance the possibility that structure might usefully be discerned in signals whose segments thus descried were iteratively impotent. Different temporal segments of a mating display, say, might convey information about different aspects of the physical prowess and motivational states of the performer. Likewise different simultaneous aspects of a dance, such as wiggle and tilt with the bees, might separately convey information about different aspects of the world (distance, direction and quality of food source). But expressions of a recursive language, I maintain, could probably not have acquired their categorial valency as it were, their powers of combination into new but readily understood messages, unless their users were on the second rung of the intentional ladder and therefore, potentially, past the Gricean hump.

To make this claim plausible – a claim which, it seems to me, lies in a grey zone between the conceptual and the empirical – I wish first to offer what seems to me the most likely account of the evolution of syntactic structure. Even if my assessment of its implications concerning the relative order of emergence of the intentional ingredient and of syntactic structure were held to be incorrect, the mechanism of that evolution might be of independent interest, and be seen as bearing on other problems besides (especially in developmental psychology and theoretical linguistics).

Recently Sampson has challenged the status of many of the alleged linguistic 'universals' put forward by Chomskians. He has argued that 'independent explanation, more plausible *a priori* than nativism, is available for the universality of the trait'. His attack is therefore two-pronged: to argue that there are far fewer genuine universals than had been thought, and to explain away the remaining ones in ways more plausible than Chomsky's.

The main syntactic universal upon whose existence both Chomsky and Sampson are agreed is that all sentences of natural language are *hier-*

archically structured. It is with respect to this universal that we can illustrate the novelty of Sampson's contribution.

Any sentence can be broken down into immediate sub-units which themselves have reasonably independent status as coherent wholes, whose combination produces the original sentence. Each of the sub-units may in turn be decomposable into sub-sub-units and so on, until non-decomposable words or morphemes are encountered. Thus the revealed hierarchy of expressions is a tree-like structure with branchings determined in a definite way. It was precisely this analysis of how certain expressions lower down in the tree thus fell within the scope of others higher up on a branch that launched modern logic on its powerful and sophisticated account of logical relations among sentences.

Chomsky and Sampson claim that this tree-like decomposition of a sentence into its parts is a feature common to all human language, and is indeed the basis upon which we determine the meaning of a sentence from the way it is built up out of its parts (an insight going back at least to Frege). In a Chomskian grammar it is the base component, consisting of phrase structure rules, that generates the sort of revealed structure just explained. According to Chomsky, we are genetically pre-programmed to search for just this sort of structure underlying the sentences that we hear as infants. The strict sense in which such structures underlie the sentences is of course enshrined in the transformational component of the grammar. All this imposes a powerful constraint on language acquisition from the allegedly scanty data available to any child. The alleged deficiency is compensated for by a neurologically based preference for *Chomskian* grammars. That is, the child is pre-programmed to arrive at *this* sort of representation of his mother tongue rather than any of the many other mathematically possible kinds. The representation is, of course, implicit and we would not expect any child to be able to state explicitly the phrase structure rules and transformations generating the sentences of his language. There is also considerable evidence (as marshalled by Ingram 1975) to suggest that up to the age of six years it is mainly short, simple but still hierarchically structured sentences that the child is able to produce. Transformations that have to do with embedding one sentence into another (such as subordinate clause or relative clause formation) are only acquired between the ages of six and twelve. This would imply at least that the early fragment of language poses the problem of hierarchical analysis in a reasonably pure form.

Sampson's attack on Chomsky's account of how the child overcomes this problem is ingenious. He advances a quite different explanation of why hierarchicality is a feature to be found in all languages. In so doing, he avoids any appeal to neurological pre-programming of a specifically linguistic kind as claimed by Chomsky.

Sampson applies an evolutionary model due to Simon, originally devised for quite different domains than the present one. These include the evolution of human institutions, and certain kinds of pre-biotic evolution. The idea is strikingly simple. If one considers hierarchical assemblies in general, their evolution from their constituents is far more likely if they consist of relatively stable sub-assemblies which themselves are evolutionary products of an earlier period. Evolution proceeds by the accidental combination of already existing stable sub-assemblies, thereby producing new stable assemblies of higher complexity. A new stable assembly establishes its credentials as a useful whole, and is co-opted into the ever-growing network of items with burgeoning structure. If we have a principled way of discerning the stable sub-assemblies, thereby distinguishing them from merely arbitrary collections of parts, then we can as it were see the evolutionary pedigree of a complex structure.

Let us apply this account to the syntactic structures of a natural language undergoing the process of evolution which it is only reasonable to suppose has taken place. The systems of communication of our primate ancestors presumably consisted of words and short sentences (but what I have to say would hold even if only gestural sequences were involved). It is reasonable to suppose that these language users might (even accidentally) hit on new combinations of phrases to produce slightly longer sentences than had hitherto been the rule: sentences, moreover, whose newly-coined significance derived from both the context of their first use and the pre-established significance of their components.

Now there are two ways that the new composite sentence token will be of a type that is eventually to acquire a constant significance. (Note here that I am doing my best to avoid speaking of conventional meaning, for I do not wish to beg the question yet against the second scenario.) The first is a highly fortuitous and austere way: namely that the compound might have the same effect – the transfer of some particular kind of information – sufficiently often and sufficiently advantageously for there to be some selective advantage in its coming standardly to possess that significance: that is, for it to acquire the biological *function* of transmitting just that kind of information.

In the case of instinctual communication the fortuitous nature of this process is only too apparent; for it requires the formation of new closed genetic programmes for both the production and processing of the complex signal. Another observation is crucial here. Remember we are concerned eventually to account for the repeatable contribution constituents make to the significance of signals involving them; this being of the essence when syntax is recursive, or creative. Thus we want to see the representational role of the newly juxtaposed elements as recognisably preserved within, and

thereby helping to determine the new representational role of, the new signal in which they occur. (I use the phrase 'representational role' rather than 'meaning' here to hew as closely as possible to the line of the second scenario.) Now in a language of potentially unlimited generative capacity, I fail to see how this could be achieved for all expressions across all patterns of combination in the first way described above. Insofar as any qualitative argument about improbability of emergence could have any force, surely this is one such case.

Compare now the vastly more plausible second way in which the new composite sentence token may be of a type that is to acquire constant significance. And let us no longer worry about begging questions against the second scenario, and simply say 'conventional meaning' rather than 'constant significance'. The second way makes the emergence of syntactic combinations seem much less fortuitous.

Consider an intriguing claim by Schmitt (1955). A proto-language might contain one-word sentences used appropriately (in a variety of speech acts) for things and events. There might be words for 'man', 'seal', 'booty', etc., and perhaps also words for events such as killings or harpoonings. Bearing in mind constantly the radical interpreter at one's shoulder, one could regard these as mainly nominal in character. Facial gesture and general demeanour could easily signal the force with which the utterance of such nouns was to be taken. Truculent and threatening utterances would be demands, plaintive ones requests, excited but friendly ones declaratives, and so on. Schmitt's claim is that even in present-day Eskimo the verb translated into English as 'kill' is a peripheral modification of the noun for 'booty'. One can imagine the first fortuitous utterance of the three word string

Man seal booty

as having the occasion meaning (via suitably diagnosed Gricean intentions etc.) of something roughly like 'The man killed the seal'. New strings like this, in a highly salient context, could readily be understood as new messages, of a new level of complexity, by an audience who already grasped the components from their isolated usages in the past. We may expect new conventions governing syntactic combinations – in our example the Subject-Object-Verb complex – to establish themselves quickly in the evolving language of any group whose members are bright enough to tumble to the meanings of such innovations. But this crucially involves their being past the Gricean hump. The linguist Charles Li, writing in his introduction to a highly speculative volume on the mechanisms of syntactic change (Li 1977), claims that with a few exceptions the only documented

types of word order changes that are not due to language contact are SOV to (VSO) to SVO. Being documented they are possible; and possible, despite word order change being, as Li himself puts it, 'the most drastic and complex category of syntactic changes'.

As new syntactic categories settle down by innovative accretion along the lines suggested, the complex mesh of grammatical relations becomes the cloth of an ever-changing community coat. By the time transformational rules (if they are really operating – which is theoretically contentious, as we shall see in due course) enter the picture, we can expect even more linguistic material to be available for reshaping, re-ordering and relocating. In this way the surface output, being as it is so critically dependent on transformational pedigree, becomes a highly labile and volatile product.

If our account of innovative accretion is reasonable, we have a basic model within the second scenario of how the earliest linguistic structures were born. A few salient juxtapositions would confer upon words a new relational potency over and above their semantic directedness to the external world. They would acquire a certain potential for juxtapositions, a grammatical valency, which is now captured by the idea of the *category* of an expression, due to Frege and Ajdukiewicz. Phrase structure rules would implicitly have been adopted, legitimating certain combinations that have successfully occurred, as well as others that had not yet occurred but might very well have, and with similar success, had the choice of words and message had to be different. Thus even the earliest and most modest collection of phrase structure rules would have been pregnant with new output; and as the rules became entrenched so too would innovative effort and diagnostic insight become more relaxed and automatic, as words appeared newly combined in well-understood syntactic contexts. Transformational rules also, presumably, emerged at this time – at first, perhaps, with mainly abbreviatory effect. Later they could have wedded sentences in constructions such as causal conditionals, being both a spur to and the reflection of our ancestors' growing conceptual mastery of the world about them. Premack's tests (Premack 1976) to establish that chimpanzees make rudimentary connections between 'states of affairs' (in order not to beg the more refined question of objectuality within them) which to us appear closely connected by virtue of a cause and an effect, or by virtue of an implicit goal or problem and a means or stratagem, seem to the layman to support attributions to chimps of protean correlates of human categories of thought. It is not too fanciful to see a connection here with a point Ingram has made about the acquisition of linguistic transformations by children (to dwell within the Chomskian paradigm a little longer). It invites one to see Haeckel's principle at work once more, with ontogeny recapitulating phylogeny:

> . . . most of the transformations of English are only acquired between the ages of six and twelve. These are the transformations that have to do with the embedding of one sentence into another. The evidence for this comes from both theory and facts. The facts come from the observation of children's complex sentences between two and twelve. The structures up to four are primarily simple sentences. Those from four to six show complex structures, but most propositions simply juxtaposed to each other. Piaget's theory of cognitive development predicts that this should be the case. To relate two structures to each other, the child needs to be capable of performing concrete operations. This ability is only developed between the age of six and twelve. (Ingram 1975: 99)

As Sampson observes, the phylogenetic account above of the simultaneous growth of phrase structure and transformational rules yields a prediction agreeing nicely with a principle formulated by the Chomskians. This principle states that transformations apply cyclically, and that later transformations, in tinkering with the transforms already produced, deal with them as completed units undergoing at most peripheral changes in this later re-arrangement. That is, later transformations tend not to interfere with or undo the effects of earlier ones in the generation of a surface from a deep structure. This is precisely what one would expect if the above evolutionary account were true. One would expect existing sentence patterns to be stabilised in use, and to be only minimally disturbed upon being combined to form more complex syntactic units. According to Sampson, many other features of transformational grammar can be explained away in a similar fashion once one adopts this evolutionary perspective.

Although Sampson himself does not point to independent evidence or hypotheses about syntactic change, what he suggests coheres well with the speculations of Chung (1977). From evidence concerning Pukapukan and Samoan, she forms the following conjectures. Syntactic change affects simple sentences before it affects the action of superficial rules; and when transformational rules are affected, the more superficial ones are affected first, the major cyclic ones last.

Also worth noting here is that Hankamer (1977) has put forward the possibility of competing grammars for a body of linguistic data as a force inducing syntactic change. The re-analysis of linguistic structure (once well developed) afforded by a different grammar may impel new structural developments and innovations. Thus the old grammar could become obsolete and it might no longer be possible to extend it to deal with relatively simple sentences of the language resulting from the 'actualisation of the re-analysis'. But Hankamer's idea is more appropriate to the 'evolution' of fully fledged languages, a process of interest in its own right but somewhat peripheral to the project of accounting for how more complex linguistic structures might have evolved from simpler ones.

The structural lacuna in the intentional scenario is, I am suggesting, to be filled by something like Sampson's account of the evolution of hierarchical arrangements of stable sub-assemblies. Innovative combinations become part of a familiar repertoire precisely because they offer a form of solution to recurring co-ordination problems whose salience the system users are quick to discern. And the accompanying growth and refinement of categorial conventions is possible only because the system users have higher order beliefs and intentions.

A further suggestion that flows from this is that such evolutionary considerations lend support to a competing grammatical paradigm – that of Montague grammar. Whereas in Chomskian grammar the basic approach is 'top–down', with transformation rules sometimes applying in ways that require one to consider syntactic environments beyond the immediate focus of application, the picture in Montague grammar is simpler, in a way more congenial to our evolutionary picture. In Montague grammar various categories of expression are simultaneously defined in a recursive fashion. One may think of similarly structured expressions within one category – differing only in lexical items – as stable sub-assemblies with an independent communicative value of their own. This may derive not only from repeated occurrence within wider syntactical contexts, but also from frequent usage 'in isolation', as it were. One thinks here, for example, of noun phrases being used in response to wh-questions. Indeed, one empirical reason why a certain category within a Montague grammar should be basic may be precisely a high pragmatic probability of its isolated use in fruitful exchanges: here the grammar of conversation rather than of the sentence may become crucial. One is inclined to ask here just how well argued is the frequent claim that 'the *unit of communication* is the sentence' (Armstrong 1971: 428).

The way a sentence (or indeed any complex expression) is generated within a Montague grammar provides a possible synchronic fossil of how, diachronically, the language acquired the complexity making such a sentence possible. For in Montague grammar one begins with lexical items (of known categories) and 'assembles' lower-level structures. Within these one can still discern the 'earlier' items, whose syntactic combination in accordance with Montague's formation rules involves only relatively minor peripheral modifications (just what one would expect on the Sampson-Simon model). The new lower-level structures are then themselves syntactically combined, again with peripheral modifications not obliterating their essential unity, into yet higher ones; and so on, until the sentence in question is produced. Importantly, there are no transformations capable of wholesale disfiguration of the by-products of any stage of the generative process.

Montague generation, proceeding as it does through all categories of expression simultaneously, promises recognition of the conversational integrity of parts of speech in a way that sentence-focussed Chomskian grammar does not. Moreover, there is a further independent reason for finding it attractive, apart from whatever success is to be had in the future in generating likely looking fragments of natural language. This reason is that it is mathematically much less powerful than Chomskian grammar. Peters and Ritchie (1971) have shown that every recursively enumerable set of strings is the language generated by some Chomskian grammar. Montague grammars, by contrast, characterise at most context-sensitive languages, and therefore yield decision procedures for grammaticality.

This is especially desirable in the light of our manifest ability to parse – that is, to produce judgements as to grammaticality, and not just judgements that are grammatical. It also means that whatever part of the brain it is that deals with matters grammatical can be regarded as relatively low in the so-called sub-recursive hierarchy of computing machines – putting it within easier reach, perhaps, of the evolution of cognitive capacities 'from below'. But these are technical considerations not to be dwelt on here.

On the account advanced by Sampson, especially with the substitution of Montague grammar for Chomskian, there is no need to appeal to innate linguistic abilities any more specialised than those required for general problem solving. The Chomskian might advance the speculative thesis that any mutation causing children to search immediately in the right class of grammars would have a great selective advantage, and that such evolutionary change might well have taken place, producing human beings who are now pre-programmed to process linguistic data in a specific way. Certainly the selective pressure for advanced linguistic competence would be very great – as witness the probable absence of any human language, in the intermediate range of the evolution that Sampson describes, that lacks, say, some of the syntactic and semantic functions and resources of known languages. But whether genes for hierarchical supposition would confer a sharp selective edge on language learners carrying them depends very much on how satisfactory an account of language acquisition is to be had from a suitably sophisticated (non-behaviourist) learning theory. Chomsky places faith in special neural mechanisms blossoming in the brain. He is ungroundedly pessimistic over the prospect of developing a powerful enough learning theory to account for language acquisition. His argument, if it counts as such, is a dogmatic admission of defeat, unsupported by quantitative evidence. We simply do not yet know enough about the capacity of learning strategies that have been discovered or perhaps remain to be discovered.

2

So far we have considered how natural language might have developed that complexity which sets it off so dramatically from the signalling systems of other species. What now of the claims that other primates can communicate with systems in all important respects as complicated as ours?

Several experimenters have maintained that chimpanzees and gorillas can be taught to communicate by means of artificial systems not involving speech. For anatomical reasons, chimps and gorillas cannot produce a range of sounds sufficient for speech. But some investigators claim that they have taught them to use systems approaching human language in their versatility and complexity. The implicit claim to be considered is that apes are, qualitatively speaking, a quantum jump ahead of other animals in this respect (although admittedly not many others have been investigated): that they are 'on our side of the divide'.

There has recently been some highly critical re-assessment of the claims initially made by ape language experimenters on behalf of their subjects. The criticism is of various kinds. Some concerns the basic methodological pitfalls surrounding any project of this kind – the lack of proper controls, over-readiness to read richer interpretations into bits of behaviour than a more rigorous viewpoint would endorse, even experimenters' proneness, given their wishful thinking, to be manipulated by their hairy charges into taking up certain attitudes not properly grounded in the available evidence. Another set of criticisms concerns the basic approach, given the desired object of establishing communicative contact with and among the apes concerned. Anyone can see that artificial keyboard or lexigram languages can be imparted, if at all, in only the most impoverished social situations – quite unlike the nexus of warm and intimate physical bonds that probably prevailed at the dawn of language millennia ago. Normal communication is free and spontaneous, with parties taking turns, with an equal balance between initiative and response. A human child quickly expands his vocabulary, and the mean length of his utterances increases accordingly. He uses already mastered words in new combinations to which he has not hitherto been exposed, and responds to such combinations appropriately as well.

Other criticisms focus on massaging of the data, in the form of simplification of ape utterances in their reporting, so as (probably unwittingly) to make them look more like human utterances than the hodge-podges they really are; and re-analysis of unedited films of 'discussions' with the apes, showing just what a high proportion of the exchanges embody no more than unconscious cueing by the human experimenter, slavish imitation by the ape, or his redundant expansion and embroidery of their exchanges without

commensurate amplification of information conveyed. More philosophical criticism is to be found along Davidsonian lines. Apes may well have produced new two-sign combinations that their trainers have been inclined to interpret as appropriately invented for some feature in the context. A celebrated example is Washoe's signing of 'water bird' in the presence of a swan. But the radical interpreter could re-interpret the dyadic sign as 'bird on water' or 'bird and water' or 'wet feathers' or 'reflection of bird' or in any one of many ways. The dyadic sign need not be endowed with any unitary significance, or be regarded as a newly constructed compound of previously mastered components. On this view it would be overcharitable to credit ape strings with syntactic structure when they may be nothing but sequences of single 'word-sentences'.

One reviewer of the present state of the debate (J. L. Marx 1980) ably summarises all the points of failing in the collection of data and their interpretation, but makes so bold as to write:

> Despite the controversy over whether or not apes can produce sentences, there seems to be agreement that they can use *words* the way we do – that is, as symbols *representing some object* that can be used to convey information to another individual. (Marx 1980: my italics)

Yet even this would be disputed by a Davidsonian who insisted on the primacy of a language of identity, reference and quantification before crediting anyone with even the general notion of an object.

One of the most important points of difference between language acquisition by human beings and the deficient parallel process in the case of the ape language experiments is, of course, that in the latter case one is not dealing with the acquisition by the young of a system employed already by conspecific elders. The apes being taught are therefore without an evolutionarily conferred advantage that human children enjoy – that of employing learning techniques, and being initiated by their elders, in a way that has presumably been refined by selection pressures over a very long time. Recent studies of language acquisition reveal that children are very attentive, and actively process evidence in ways perhaps not fully appreciated at the time when Chomsky was championing a theory of innate linguistic universals that depended heavily on an alleged gap between the scanty data available to the child and the rich system that he eventually masters in response thereto. Children are highly motivated learners of language, a disposition no doubt by now 'wired in' to our species. In this respect they are probably quite unlike the apes, who have not yet been subjected to evolutionary pressures for rapid acquisition of symbol systems. Children benefit also from more than usually grammatical speech from adults who address them in the early stages in a fashion tailored to their learning needs.

This tendency on the part of adults could no doubt also be selected for, once language was entrenched enough to exert the required pressures. Parents repeat and reinforce their children's utterances, and produce slightly more complicated versions of things already mastered in a gentle advance up the ontogenetic slope.

> In the heyday of 'nativist' account of language acquisition (in the early 1960s) it was widely assumed that the speech heard by children was a haphazard collection of sentence fragments, mistakes, backtrackings, throat clearings, and other kinds of unintelligible gibberish. This assumption appears to have been derived from analyses of adults talking to each other at psycholinguistic conferences. There is now, of course, a considerable body of evidence showing that the speech addressed to young children is typically very different from that addressed to older children and adults. Many of these variations are structural and seem to reflect something other than differences in topic and semantic content. Mothers, some at least (and other adults), frequently speak slowly to children, leave physical gaps between words or phrases, use an extended pitch range, give very heavy stress to lexical items, use short sentences, simplify the syntax, expand and even correct their children's utterances. (Marshall 1980: 115. Marshall refers in this connection to C. Snow and C. Ferguson (eds), *Talking to Children* (Cambridge, 1977).)

These observations on human language learning, coupled with evolutionary speculations make one realise just what a formidable accomplishment it would be if apes could be taught the use of a communication system remotely approximating a natural language in creativity, recursiveness, and the extreme conventionality revealed in such matters as reference to spatio-temporally remote items or counterfactual conditionalising or universal generalisation about an unsurveyed domain. They may well have certain cerebal pre-adaptations that subtended vocalised speech linked with gesture, which responded in some ancestral line to pressures of selection for an ever more complex code of communication.[7] The ape's failure to acquire a 'language' from us (spoken or gestural or plastic-symbolic) in no way discredits the very reasonable evolutionary claim that they, or a common ancestor of ours, had the rudimentary cerebral beginnings that are now our speech centres. The language faculty is closely tied up, both conceptually and empirically, to other cognitive faculties such as silent foresight, planning, anticipation, reasoned fear, etc. It may be impossible for the apes to master a code that we would translate into our own terms. Admittedly we can discern glimmerings of intelligent thought involving the location of sub-goals and the execution of sub-strategies, a

[7] Jaynes (1976) claims that this happened as recently as the late Pleistocene. The proliferation of tools in the Pleistocene, the making of apparently 'non-utilitarian' and symbolic objects, the practice of burying the dead ceremonially, the construction of shelters and the marked increase in the size of the frontal lobe of the brain with Broca's area, all point to language's being reasonably developed by the end of the Pleistocene. See the papers by Isaac, Marshack and Holloway in Harnad *et al.* 1976.

grasp of 'causality', perhaps even a grasp of the internal motivational states of others, as disclosed by their behaviour; and some experimenters have even looked for signs of a sense of self, and even of intimation of mortality. But too much mist obscures the question what it is like to be a chimp for even the best-meaning efforts to make them make the best of meaning.

Our interpretation of the results of the chimp language experiments points to recursively structured language as a unique accomplishment of our own species. Lenneberg (1966) has argued that a great deal of evidence about deafness in children, aphasia, environmental deprivation of various kinds, muscular debilities and so on supports the hypothesis that language acquisition by children follows a definite maturational path, passing certain milestones of achievement in a certain order. Moreover these milestones correspond broadly with others in the course of sensory-motor development, being broadly in step with them even when the whole process is slowed down, as in the cases of mental retardation. Both linguistic and sensory motor development are then co-ordinated further with the 'brain maturation curve'. This is a graph of the growth in degree of organisation and certain chemical concentrations, which gives a crude measure of growth. There is no evidence from all this of cerebral 'rubicons' that correspond to the different stages of language acquisition, but one cannot avoid the impression that the linguistic skills acquired by the growing child are orchestrated by a physiological score; and one moreover that has its own peculiar, species-specific crescendoes.

In a survey of the evidence for the species-specificity of speech, Dingwall (1975) has reached broadly similar conclusions. He concludes that 'the ability to produce vocalisation which is articulated as opposed to holistic in nature, which is mediated by the neo-cortex as opposed to the limbic system, is unique to the genus: homo sapiens'. Of course, he is concerned here with *speech*, and not with language in general, which may of course encompass gestural systems or systems such as those used by the ape language experimenters. When two closely related species have similar behavioural patterns or capacities, and this similarity is due to their having a common origin, then the behaviours are said to be *homologous*. By contrast, similarities (even across unrelated species) that are not due to common origin, but rather to force of environmental circumstance, to conditions which elicit one narrowly constrained adaptive response from the two species, are called *analogous*. Dingwall concludes further from what he describes as 'abundant evidence' about structure and function, that human and ape vocalisations are not homologous. As for other aspects of the communication systems, the 'evolutionary, ontogenetic and neurological evidence currently available tends to support homology rather than [analogy]'.

Apes and monkeys employ a limited number of calls. Some may serve to

warn fellows about very specific kinds of predator, and therefore have quite specialised communicative significance (Seyfarth *et al.* 1980). But they are not formed into sequences having different significance according to their manner of combination and the basic items they contain.

There are some noteworthy differences in brain physiology, apart from the massive increase in brain size, as one passes up through the other primates to man. Chimpanzees lack Broca's area (for muscular co-ordination in speech), as well as the neighbouring arcuate fasciculus. The latter is the fibre bundle connecting the auditory area with Broca's area, which some believe to be crucially involved in the imitation of sounds. It is little wonder then, given these handicaps of sheer physiology, that chimps are incapable of speech. A more delicate question to assess, however, is whether one could reasonably maintain that they are pre-adapted for *Language* (not necessarily vocal, but symbolic and semantic in an appropriate sense). We appear to share with chimpanzees a strong rooting of emotional cries in the limbic system – the so-called 'emotional' brain. But for the rest of spoken language, our neo-cortex is heavily involved, with strong evidence furthermore of lateralisation for speech even as early as birth. (By 'lateralisation' here we mean that the functions are based on one side – usually the left – rather than the other.) Infants appear, on certain experimental evidence, to be 'wired' for the recognition of speech sounds as opposed to others. And yet, paradoxically, the evidence for lateralisation of the 'higher' and more abstract processes of language apart from vocalisation, is less conclusive.

Geschwind once proposed a theory concerning association areas in the brain (Geschwind 1974). According to him, language could only evolve once the auditory and visual *association areas*, that in monkeys as well as man convey impulses from the auditory and visual areas to the limbic system, *themselves* acquired suitable connections via yet another association area, thereby making possible so-called non-limbic, cross-modal associations. This 'association area for the association areas' is the angular gyrus, and it is absent in monkeys. The reason why its mediation of modes is so crucial is that learning and understanding names of objects probably requires the association of visual with sound images (assuming, of course, that we are dealing with names from a *spoken* language). Of course, naming is not the whole of language, as the opening passage of Wittgenstein's *Investigations* makes us so well aware; yet it is no doubt a central part of language, and still an important and controversial topic in the philosophy of language. Geschwind's express intention was to find physiological correlates of linguistic capacities, in order to bring language within the scope of scientific materialism. His conjecture, then, is just one way of making sense of certain anatomical facts across species, evolutionary hypotheses, and observations

of impairments of linguistic functions on the part of human patients who had suffered different sorts of damage to their brains.

Surprisingly Dingwall states categorically that Geschwind's theory, 'while ingenious, is most assuredly incorrect . . . ' He cites experiments that have shown that

> Chimpanzees are able to match visually presented to tactilely presented stimuli . . chimpanzees can transfer from auditory to Ameslan signs in the absence of referents . . . Thus, the inability to produce cross-modal associations is not a barrier to the acquisition of language in chimpanzees . . . Not only is there evidence of similarity in cross-modal association behaviour in chimpanzees and man but also, as pointed out . . . there is no marked difference in these two species in the angular gyrus which Geschwind holds to be important in mediating this behaviour. (Dingwall 1975: 43)

This is extremely puzzling argumentation. Chimpanzees have angular gyri. But given the copious evidence that chimps simply cannot get their lips and tongues around enough sounds, their cross-modal associations pose no threat to Geschwind's theory. On the contrary, the very successes claimed by the chimp language experimenters, to which Dingwall himself is sympathetic, would bear out that theory even further. For Geschwind had been careful enough to note that the angular gyrus is needed even for visual–visual (indeed, for any non-limbic) associations. So perhaps it is because they have angular gyri that chimpanzees have been able to sign successfully.

I say 'perhaps' because rhesus monkeys have recently been claimed to be able to make cross-modal associations.[8] Nevertheless, so far only chimpanzees have displayed any ability to make visual–auditory associations.[9] And it remains to enquire to what extent other associative abilities experimentally revealed in other species approach those to be expected of a creature able to use linguistic symbols. Von Glaserfeld has argued that

> . . . the semanticity of a *linguistic* sign is constituted, not by a tie that links it to a 'thing', but by one that links it to a representation or concept. The fact that a sign, be it verbal or non-verbal, has acquired symbolicity, does of course not preclude that it still be used as a perception-bound sign whenever there is a perceptual input that corresponds to the representation it designates; nor does it preclude that it be used by the sender to trigger a conventional active response in the receiver (as in the case of an 'imperative'). But what gives a sign the status of symbol is that it *can* be used without such a stimulus and without triggering the active response. (Von Glaserfeld 1976: 222; his italics)

In the same Lockean spirit, Davenport reaffirms the relevance of non-limbic associations when he asks

> Of what relevance is cross-modal perception to the origin and evolution of speech and language? First, it appears that multi-modal information extraction of environmental information is likely to result in more veridical perception, and may facilitate

[8] See Cowey and Weiskrantz 1975.

[9] See Fouts *et al.* 1976.

> cognitive functioning. Second, in my view, cross-modal perception requires the derivation of modality-free information, a 'representation'. That an organism can have the same representations, concepts or percepts, regardless of the method of peripheral reception, confers a great advantage on that animal in coping with the demands of living . . .
> To the extent that an organism has a 'tag' for the representation, be it sound, gesture or combination, the process would be greatly facilitated. (Davenport 1976: 147–8)

Just as there appear to be no rubicons in a child's cerebral development, so also there were probably no such rubicons phylogenetically. Evolutionary pressures would have gone to work on whatever genetic variability there was in the ability to make the relevant cross-modal associations underlying linguistic competence. The importance of the former for the latter is not at all diminished by exhibiting languageless creatures that can associate across modes. For, that one must be able to do X in order to do Y does not entail that if one can do X then one should be able to do Y.

Are the recursive resources of language an innate endowment of Homo sapiens alone? Our earlier discussion inclines one to say so. But we must not support the innateness claim with the wrong arguments. Universality is no guarantee of innateness.[10] This is so even though it might be true that any infant, regardless of race or family niche, can acquire the tongue of any community. This acquisition claim has never been rigorously tested. Physiognomical differences between racial types might affect pronunciation of the foster mother tongue. But setting purity of pronunciation aside, could any child master any first language so well that native speakers would regard him as fully competent in matters lexical and grammatical? Should languages have diverged sufficiently radically in our evolutionary past, and should cerebral organization have adapted constantly to the need of parsing, it is quite possible that counterexamples to the claim of eventual competence might be found. But this would not count in any way against language's being innate. Indeed it is its very innateness, given sufficient divergence between different languages, that make such counterexamples seem possible.

Hewes (1977) counts twenty-four different theories concerning the origin of the first lexical items. (Perhaps 'theory' is too grand a label for these suggestions.) He concludes plausibly that the best account will probably incorporate several different suggestions about the origin of words in different 'lexical domains'. Onomatopoeia and adult imitation of salient infant babbling are just two such suggestions. But by far the most likely theory looks to gesture as the raw public material from which human communication grew. Speech could have grafted onto gesture once the vocal tract had developed sufficiently, with selective advantages that

[10] Compare a similar point about ethical theorising in Tennant 1983.

Darwin had remarked on. The hands are free when one talks; and one can be heard in the dark and in all directions, under cover and so on. Consistent with this theory is the synchronisation reported by MacNeill (1979) between hand movements and speech segments. He observes also that gestures can extend and replace full imitation, while yet being sufficiently iconic to secure uptake. Finally, although Neanderthal man had once been thought incapable of producing a reasonable range of vocal sounds, the fossil reconstruction on which this claim was based has been severely criticized recently (Du Brul 1979).

Complex sentences can be *parsed*. In the case of natural language, natives' pre-formal agreement on the boundaries of grammatical division is the most important kind of evidence the linguist can glean as he searches for the grammatical recipes and ingredients of sentence meaning. One attraction of the gestural theory of language origin is that significant structure does not have to await words. Susan Goldin-Meadow's subjects were unacquainted deaf children; but they had normal parents who did not try to communicate with them by gesture, or at least not in *sequences* as the children did:

> ... the deaf children were seen to have devised their own manual signs. Many of them were based on actual movements. And they combined signs into sequences with relatively stable 'word' order preference. The most rapidly developing child eventually made use of such sophisticated devices as clausal embeddings. The steps in the development of these sign systems occur in a regular sequence, and at times which resemble the development of vocal speech by hearing children. (MacNeill 1979: 720)

Of course, these children might have been interpreted according to overly lax canons – compare our discussion of chimp gesture above. But *prima facie* the generous semantic account of their project is much more plausible, given that they are agents just like us except insofar as they cannot hear. We should not regard their 'invention' of a structured gestural language as grounds for believing that earlier hominid handwaving could thus rapidly have attained parsable results. After all, these children are presumably 'wired' for Language. They will possess the same cognitive structures and whatever maturational schedules thereof that underly hearing children's language acquisition. As we remarked earlier, their 'problem' was to find a channel of communication. The problem facing our forebears was to discover communication.

Gestural messages, so it seems, can have significant structure. Thus the structure of spoken sentences need not have derived entirely from combination of words that had replaced single gesture types. Instead, structured gestural sequences, or *syntagmata* as MacNeill calls them, might already have had a rudimentary grammar before they were overlaid by speech.

Whatever the relative order of gesture, speech and structure thereof, my quasi-conceptual evolutionary thesis would still be pressing: iterable contribution to significant structure across the whole language, be it gestural or symbolic, is almost certain to have needed a shrewd intentional grasp by communicators of what they were up to. Note that this claim would not be undermined by showing that infants 'mindlessly' master the communicative content of structured sentences before it dawns on them gradually that assertions express beliefs and that deceit is possible. For selective pressures for linguistic ability could easily reverse in ontogeny the order I maintain would be needed in phylogeny. Selection would have operated within the environment of an already structured code. Mastery of that code by distant descendants could therefore become more 'mindless' in the early stages (even those involving significant structure) as brains were shaped by natural selection for more and more rapid language acquisition.

Interest in language origin theory, as the heavy volume edited by Harnad and others testifies (Harnad *et al.* 1976), is being revived after a long period of disrepute. Hardly any writer has looked, linguistically, beyond unstructured lexical items. My attention here has been held far more by the enigma of syntactic structure, and how its development might mesh with the conceptual analyses of meaning and intentionality offered by philosophers of language. My thesis can best be summarised as follows:

> There could be staccato talk without thought.
> There had to be thought before structured talk.
> Once established, structured talk could be mastered with less thought.
> Once mastered, structured talk makes for more thought.

REFERENCES

Armstrong, D. M. 1971. 'Meaning and Communication', *Philosophical Review* 80, 427–47.

Bennett, J. 1976. *Linguistic Behaviour*. Cambridge.

Caplan, D. (ed.) 1980. *Biological Studies of Mental Processes*. Cambridge, Mass.

Chomsky, N., and Caplan, D., 1980. 'Linguistic Perspectives on Language Development', in Caplan 1980:97–105.

Chung, S. 1977. 'On the Gradual Nature of Syntactic Change', in Li 1977: 3–55.

Cowey, A., and Weiskrantz, L. 1975. 'Demonstration of Cross-modal Matching in Rhesus Monkeys, *Macaca mulatta*', *Neuropsychologia* 13, 117–20.

Dato, D. P. (ed.). 1975. *Georgetown Roundtable on Languages and Linguistics: Developmental Psycholinguistics*. Washington, D.C.

Davenport, R. K. 1976. 'Cross-Modal Perception in Apes', in Harnad *et al.* 1976: 143–49.

Davidson, D. 1973. 'Radical Interpretation', *Dialectia* 27, 313–28.

Davidson, D. 1975. 'Thought and Talk', in Guttenplan 1975: 7–23.

Davidson, D. 1979. 'Moods and Performances', in Margalit 1979: 9–20.

Dingwall, W. O. 1975. 'The Species-Specificity of Speech', in Dato 1975: 17–62.
Du Brul, E. L. 1979. 'Poor Old Neanderthal Man: Response to a Response', *Brain and Language* 8, 251–7.
Eisner, T. and Wilson, E. O. 1977. 'The Insects', *Readings from Scientific American*. San Francisco.
Field, H. 1978. 'Mental Representations', *Erkenntnis* 13, 9–61.
Fouts, P. *et al.* 1976. 'Translation of Signed Responses in American Sign Language from Vocal English Stimuli to Physical Object Stimuli by a Chimpanzee', *Learning and Motivation* 7, 458–75.
Geschwind, N. 1974. *Language and the Brain*. Dordecht.
Goldin-Meadow, S.J. 1975. 'The representation of semantic relations in a manual language created by deaf children of hearing parents: a language you can't dismiss out of hand', PhD thesis, Department of Psychology, University of Pennsylvania; quoted in MacNeill 1979.
Grice, H. P. 1969. 'Utterer's Meaning and Intentions', *Philosophical Review* 78, 147–77.
Guttenplan, S. 1975. *Mind and Language*. Oxford.
Hankamer, J. 1977. 'Multiple Analyses', in Li 1977: 583–607.
Harnad, S. R., Steklis, H. D. and Lancaster, J. (eds) 1976. *Origins and Evolution of Language and Speech*, Annals of the New York Academy of Sciences vol. 280.
Hewes, G. W. 1977. 'Language Origin Theories', in Rumbaugh 1977: 3–53.
Holloway, R. L., 'Paleoneurological Evidence for Language Origins', in Harnad *et al.* 1976: 330–48.
Ingram, D. 1975. 'If and when Transformations are Acquired by Children', in Dato, 1975: 99–127.
Isaac, G. L. 1976. 'Stages of Cultural Elaboration in the Pleistocene. Possible Archaeological Indicators of the Development of Language Capabilities' in Harnad *et al.* 1976: 275–88.
Jaynes, J. 1976. 'The Evolution of Language in the Late Pleistocene', in Harnad *et al.* 1976: 312–25.
Leakey, R. 1981. 'The Birth of Language', *The Listener*, 28 May 1981.
Lenneberg, E. H. 1966. 'The Natural History of Language', in Smith and Miller 1966: 219–52.
Lewis, D. K. 1969. *Convention*. Cambridge, Mass.
Li, C. (ed.). 1977. *Mechanisms of Syntactic Change*. Austin, Texas.
MacNeill, D. 1979. 'Language Origins', in von Cranach *et al.* 1979: 715–28.
Margalit, A. (ed.) 1979. *Meaning and Use*. Dordrecht.
Marshack, A. 1976. 'Some Implications of the Paleolithic Symbolic Evidence for the Origin of Language', in Harnad *et al.* 1976: 289–311.
Marshall, J. C. 1980. 'On the Biology of Language Acquisition', in Caplan 1980: 106–48.
Marx, J. L. 1980. 'Ape-Language Controversy Flares Up', *Science* 207, 1330–3.
McDowell, J. 1980. 'Meaning, Communication and Knowledge', in van Straaten 1980: 117–39.
Mellor, D. H. 1980a. 'Consciousness and Degrees of Belief, in Mellor 1980b: 139–73.
Mellor, D. H. (ed.) 1980b. *Prospects for Pragmatism: Essays in Memory of F.P. Ramsey*. Cambridge.
Montague, R. 1974. *Formal Philosophy*. New Haven.

Peters, P. S., and Ritchie, R. W. 1971. 'On Restricting the Base Component of Transformational Grammars', *Information and Control* 18, 483–501.
Petitto, L. A. and Seidenberg, M. S. 1979. 'On the Evidence for Linguistic Abilities in Signing Apes', *Brain and Language* 8, 162–83.
Premack, D. 1976. *Intelligence in Apes and Man*. Erlbaum, N.J.
Premack, D. and Woodruff, G. 1979. 'Intentional Communication in the Chimpanzee: The Development of Deception', *Cognition* 7, 333–62.
Riedl, R. 1979. *Biologie der Erkenntnis*. Berlin.
Routley, R. 1981. 'Alleged Problems in Attributing Beliefs and Intentionality to Animals', *Inquiry* 24, 385–417.
Rumbaugh, D. M. (ed.) 1977. *Language Learning by a Chimpanzee*. New York.
Sampson, G. 1978. 'Linguistic Universals as Evidence for Empiricism', *Journal of Linguistics* 14, 183–206.
Schmitt, A. 1955, 'Der nominale Charakter des sogenannten Verbums der Eskimosprache', *Zeitschrift für Vergleichende Sprachforschung* 73, 27–45.
Searle, J. R. 1979a, 'Intentionality and the Use of Language', in Margalit 1979: 181–97.
Searle, J. R. 1979b. 'What is an Intentional State?', *Mind* 88, 74–92.
Seidenberg, M. S., and Petitto, L. A. 1979. 'Signing Behaviour in Apes: A Critical Review', *Cognition* 7, 177–215.
Seyfarth, R. M., Cheney, D. L. and Marler, P. 1980. 'Vervet Monkey Alarm Calls: Semantic Communication in a Free Ranging Primate', *Animal Behaviour* 28, 1070–94.
Simon, H. A. 1962, 1969. 'The Architecture of Complexity', *Proceedings of the American Philosophical Society* 106, 467–82; reprinted in *The Sciences of the Artificial*. Cambridge, Mass.
Smith, E. and Miller, G. A. (eds.) 1966. *The Genesis of Language – A Psycholinguistic Approach*. Cambridge, Mass.
Tennant, N. 1983. 'Evolutionary v. Evolved Ethics', *Philosophy* 58.289–302.
Van Straaten, Z. (ed.) 1980. *Philosophical Subjects*, Oxford.
Von Cranach, M., Foppa, K., Lepennies, W. and Plogg, D. 1979. *Human Ethology: Claims and Limits of a New Discipline*. Cambridge.
Von Glaserfeld, E. 1976. 'The Development of Language as Purposive Behaviour', in Harnad *et al.* 1976: 212–26.