7 The Use of Natural and Linguistic Concepts in Psychological Explanation[1]

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The contemporary discussion of rationalist v. empiricist theories of learning, particularly among linguists, seems to me to suffer from a particular misconception concerning the nature of the alternatives. Empiricist views are characterised as equating human learning with animal learning and the theories of animal learning which are taken as paradigmatic are of a restricted kind. In analysing this position and its consequences I will develop three main themes. The first of these is that changing views about animal learning are leading to more complicated theories of animal behaviour. Secondly I will illustrate the way in which cognitive models of human behaviour are developing and will suggest that the conceptual frameworks so derived are a more appropriate tool than everyday language for discussions of the philosophy of mind. Finally I will consider in some detail certain features of the current debate concerning the nature of language learning and will indicate that the use of a particular set of concepts to define a debate can lead to inappropriate conclusions. What these themes have in common is the stress placed on the types of theory which can be seen as an alternative to the rationalist position.

[1]I am deeply indebted to David Routh for spending many hours in helping to clarify the issues raised in this paper. Residual faults are in spite of his efforts.

The excessive polarisation which is my main concern is well illustrated by Chomsky's attacks on empirical psychology. The following quotation is representative:

> A system of rules for generating deep structures and relating them to surface structures, in the manner characteristic of natural languages, simply does not have the properties of an associative net or a habit family. (Chomsky, 1967a, p. 10)

If we are faced with a choice purely between Skinnerian psychology or simple S-R theory on the one hand and theories of innate ideas on the other, then we might prefer the latter, rationalist position if for no other reason than it leads us into more interesting topics. However, there are much richer concepts available to empirical psychologists than associative nets or habit families. I would consider myself to be an empirical psychologist, but I do not view my task as simply that of gathering data and formulating general laws concerning that data, nor that of the prediction and control of behaviour. There are schools of psychology which do see their task in this way but I do not think that Hamlyn is quite correct in suggesting that such views are a result of over-preoccupation with animal learning. It is rather a preoccupation with a particular approach to animal learning which creates this view, and this approach is not the only one available. There is a strong feeling nowadays that the general laws of animal learning are qualitatively inadequate, and there is an increasing tendency to attempt descriptions of the mechanisms underlying animal behaviour rather than simply describing the behaviour.

The traditional aim of studies in animal learning has been to formulate general laws on the basis of carefully controlled experiments with the belief that these laws will generalise to real-life conditions. As Seligman (1971) observes, the methodology has involved creating arbitrary situations which are 'uncontaminated by past experience the organism might have had or by any special biological propensities which the animal may bring to it'. (p. 407) The danger in this is 'that the laws so found will not be general, but peculiar to arbitrary events'. (p. 407) Seligman challenges the traditional

idea that the laws of learning apply equally to the pairing of any stimulus with any response. He develops instead the notion of an animal's 'preparedness' for a particular learning situation. He argues that animals in general, and in certain cases specific species, will readily associate together stimuli which belong to certain classes and will readily learn to make particular classes of responses to certain classes of stimuli. These 'prepared' associations contrast with others which are 'contra-prepared' and some which are neutral in this respect. In support of his position he cites a number of studies in each of the classical, instrumental, discrimination and avoidance learning paradigms. Thus, in the realm of classical conditioning, Garcia and his associates have shown that rats will associate the taste of saccharine-water with nausea induced by X-radiation at the time of drinking even though the nausea does not occur until an hour after the event. No association was learned, however, between auditory or visual stimuli occurring at the time of the X-radiation and the subsequent nausea. On the other hand, a relationship was learned between lights or noise and subsequent electric shock, but not between taste and shock. Seligman quotes other studies showing similar restrictions in instrumental learning, such as the difficulty in persuading a cat to scratch itself in order to escape from a box. In training a dog to discriminate between sounds a spatial separation of the sounds can be learned if the response is a choice between going left or right but not if the choice was between making a response and doing nothing. The reverse is true, however, if the sounds differ in frequency but not position. Finally he observes that it is difficult to train rats to press a bar or pigeons to peck a key in order to avoid shock, compared with learning an escape response.

These restrictions on learning make sense in the context of a real organism. Thus, Seligman cites Bolles who has recently suggested that only responses chosen from the natural, species-specific defensive repertoire can be used as avoidance responses. It would be a mistake, however, to imagine that with a few such qualifications the general laws of learning can be rescued and maintained in their dominant position. In order to adequately formulate the restrictions on the laws a

model of the natural behaviour of each species will have to be constructed within which the dynamics of the particular situations in which the laws apply will have a relatively small and uninteresting role, compared with a description, say, of the strategies open to a species in a learning task and the decision mechanism leading to the choice of a strategy.

THE ROLE OF MODELS IN HUMAN EXPERIMENTAL PSYCHOLOGY

The same conclusions are inescapable for the study of human learning. The enormous amount of activity which has gone into the investigation of the learning of nonsense syllables can now be seen to have produced only marginally interesting results. The drawback with such work is that the situation is arbitrary – nonsense syllables may be thought of as 'contra-prepared' stimuli for humans. The most potent influence on human learning is 'effort after meaning', which studies in nonsense syllable learning specifically intend to exclude. It is scarcely surprising, then, that such work has had little application to real teaching and learning. It must not be supposed, however, that the answer is simply to use more natural stimuli in our studies of learning. A different approach is needed as well, one in which the complexities of a mechanism such as the brain are faced. It is no longer possible to understand data or to evaluate experiments without a model of brain function. For example, even with such an apparently simple task as remembering a list of digits over a short interval, the conditions of presentation and the mode of responding can affect performance dramatically. It is necessary to postulate at least four functionally (though not necessarily anatomically) distinct memory stores to account for such phenomena (Morton, 1970). When one considers the effects of instructions and the way in which different strategies which the subject might adopt can affect the results, the theoretical framework required for an adequate account of performance becomes very complicated.

In addition it is gradually being realised that topics such as 'memory', 'perception', and 'psycholinguistics' cannot be studied in isolation. This is because our models of one function become contingent upon performance limits in

another area. This realisation has affected the methodology we use. Psychology can rarely work nowadays on the basis of a restricted hypothesis from which a prediction is made and tested with the subsequent acceptance or rejection of the hypothesis. The kind of prediction which is now being made is more like, 'If this part of the system operates in the way *X* thinks it does, then the properties which *Y* suggests for that part of the system would lead me to predict such-and-such a result in this situation.' The falsification of the prediction might be due to an error in the characterisation of either of the relevant parts of the system or simply because the subjects discovered a strategy which avoided them having to use either. The kinds of converging operation which are required in consequence are rarely made explicit by experimenters but are apparent from the way in which discrepant results are often discussed.

The power of a psychological model lies in its ability to accommodate data or observations from a wide variety of situations. In doing this, sets of data which had led to seemingly contradictory conclusions might be reconciled. As an example we can consider the phenomenon that words of high frequency are recognised more easily than low frequency words when they are presented acoustically in noise or visually for a brief time. At one time there was debate as to whether such results were due to stimulus or response effects (Rubenstein and Aborn, 1960). Evidence that words which people *used* more frequently or had given previously in a task as erroneous responses were easier to recognise could be taken as supporting the influence of response factors. The finding that the visual presentation of, say, PHRASE led to facilitation of the subsequent recognition of the word but had no influence on the recognition of FRAYS, for which the response is the same, strongly supported the influence of stimulus effects. The postulation, for other reasons, of a construct located between stimulus mechanisms and response mechanisms allowed the resolution of the dilemma (Morton, 1964). This construct is supposed to be involved in both the recognition and the production of words and, as part of its original specification, dealt with homonyms separately. In the light of the resulting model all the data made sense. The

model made it possible to refer to a process, termed the 'logogen system', for which there was previously no single term in the psychological literature. The concept to which 'logogen' refers can only be defined in terms of the other parts of model and their relationships. In time we will find that this concept, too, will become inadequate. Initially the model will be modified to accommodate new data but in the end, as the overall model becomes more complex, it is highly likely that new concepts and new terms will be required in order to maintain the precision of our descriptions. This process seems to me to bear a resemblance to the invention of new operators in mathematics. The power of such operators lies in their ability to express concepts whose description was laboured in previous systems.

THE LIMITATIONS OF NATURAL LANGUAGE

I have attempted to show that empirical models do not have to be simple and have indicated the way in which they can develop and the reason for this development. The reason lies, as I said, in the continual need for our conceptual framework to expand and in the need for new terms which describe the components of such conceptual frameworks. We should expect, then, to find that the use of natural language has acute limitations as a tool for discussing psychological phenomena. Professor Hamlyn has illustrated some of these difficulties in his attempt to give a precise definition of 'learning'. He notes that some psychologists have taken 'learning' to include any modification of behaviour in an organism as a result of experience or stimulation, and argues that this is too general a concept, suggesting instead a definition based on 'the acquisition of knowledge'. In analysing this definition he is forced back to a concept of 'knowledge *simpliciter*' which he regards as under-pinning 'the whole range of disparate cases which fall under the general heading of "having learned to . . ." '. Unfortunately the term 'knowledge *simpliciter*' remains undefined, except by exclusion and we are left with the feeling that the term makes sense, that the concept might be useful, without really being able to justify such a faith. I would argue that some of the difficulties that Hamlyn faces are due to an approach which

assumes that natural language terms are appropriate for the task as long as we hedge them around with sufficient qualifications. The restricted sense of 'knowledge' which Hamlyn needs to use has no word to describe it, nor, I suggest, will it have until psychological models of brain function have been developed well beyond their current state. We will need to develop ideas about the way in which information is structured in the brain, and the way in which this information can be modified before we can begin to give the concept a precise definition. A concept can only be adequately defined in terms of the environment within which it applies and the environment appropriate for the concept in question is that of brain function. Natural language terms tend to subsume a number of psychological functions or constructs and we should not be surprised to find that certain distinctions, intuitively clear or clear within the context of a psychological model, are clumsy to express.

RATIONALISTS AND LANGUAGE LEARNING

Hamlyn devotes the body of his paper to a discussion of rationalist theories of language learning. I would like to complement his paper by considering the way in which the issues have been raised recently by certain psychologists. Broadbent (1970) has recently published a paper entitled 'In defence of Empirical Psychology' which has been the subject of a critique by Shotter and Gauld (1971). I do not intend to discuss here the particular theory Broadbent puts forward since it was only a sketch of how some of the rationalist objections might be met. My concern is more with the principled attack which Shotter and Gauld make on the empirical approach to language. Their argument is based on two main premises which I will dispute in turn. First, that the nature of the distinction which linguists make between competence and performance models implies that empirical tests of grammars are impossible. Second, that the distinction made, within generative grammar, between deep structure and surface structure, provides us with logical objections to empirical theories of language acquisition. Such premises, and the associated arguments, are by now familiar to us through the works of Chomsky (*passim*) and other writers. Shotter

and Gauld's endorsement (as psychologists) of such views appears to me to be symptomatic of a widespread failure to appreciate some of the ambiguities inherent in the underlying conceptual framework.

LEVELS OF DESCRIPTION IN LINGUISTICS AND PSYCHOLOGY

In this section I would like to argue that the two-way distinction between competence and performance models, whereby attempts have been made to characterise the different aims of linguists and psychologists, is both inadequate and misleading. Instead I want to suggest that the debate can be made more fruitful in terms of a three-way distinction. For present purposes the three kinds of model may be characterised as follows:

1. An 'idealisation' which is notionally abstract. This is a description of a body of data, gathered in a specified way, which has as a criterion of success some notion of parsimony, but which need bear no resemblance to the mechanism which produced the data.
2. A *Model of Potential* specific to language, which represents the working of that part of the brain concerned solely with the understanding and production of language.
3. A *Model of Realisation* which includes the Model of Potential and indicates, at least in qualitative terms, the way in which the working of the Model of Potential is influenced by other parts of the cognitive apparatus, (which might themselves be considered in terms of the three levels).

The words 'Potential' and 'Realisation' may not be ideal but the introduction of new terms does seem to be required in view of the unwanted and consequently ambiguous connotations now surrounding the terms 'performance' and 'competence'. Thus a 'grammar' has been equated with a competence model, sometimes with the intent of an idealisation and sometimes with the implication of a Model of Potential. The term 'performance model' has sometimes been used with the connotations of the Model of Potential and sometimes with the connotations of a Model of Realisation. The ambiguities can be seen from an examination of the analogy, sometimes drawn, between linguistic and numerical

ability (e.g. Chomsky, 1963). The point of the analogy is ostensibly that it illustrates the way in which limitations of such things as memory and attention distinguishes between competence and performance. In the analogy our competence is supposed to be represented by the set of multiplication tables and performance limitations are illustrated by our inability to multiply two 5-digit numbers together in our heads; with pencil and paper, however, we can approximate our competence in performance. As I have discussed previously (Morton, 1968), this analogy seems to be ill-formed. If the grammar is an idealisation then it corresponds not to the set of tables but rather to a theory of Natural Numbers, which would not include such statements as 'two x two = four'.

There need to be no objection to the linguist regarding his grammar as an idealisation. Nor should there be any objection if a psychologist attempts to determine whether there is any direct correspondence between the rules of the grammar and mechanisms in the brain. What is required is consistency. The linguist cannot, on the basis of his idealised description, draw any conclusions about language learning or brain function and the psychologist cannot conclude anything about the idealisation (except, perhaps, its relevance). Parenthetically, it should be emphasised that the restrictions on psychologists are more severe than this. I have already noted the methodological problems which confront the experimental psychologist. The failure, or for that matter the success, of any one test, say an attempt to detect the operation of a grammatical rule in an experimental situation, is not necessarily crucial for the main hypothesis. Not only is the notion of a crucial experiment properly suspect, but the level of model at which a result may be interpreted is not always transparent. Thus, competing interpretations of a number of psycholinguistic experiments are confounded because of the possibility that, in the particular task, the subjects were able to adopt a particular strategy whereby our normal language processing system (i.e. the Model of Potential) was bypassed. Thus, in an experiment involving memory for sentences, the subjects may be able to recall the actual sound pattern of the stimulus, or treat the sentence word by

word rather than analysing the syntax and meaning of the sentence as a whole. In this case, if we failed to detect any effects of a grammatical variable, we might conclude something about the flexibility of the Model of Realisation rather than concluding that the Model of Potential operates in a manner different from that suggested by the grammar.

In arguing that empirical tests of generative grammars are impossible in principle, Shotter and Gauld move somewhat freely between the three levels of description. They support their position as follows:

> Chomsky regards his 'generative' grammatical rules as idealisations, resembling the rules by which theorems are derived from axioms in mathematico-logical systems. These rules – as for instance those of logic – may be implicit in performance before they can be formally characterised. As Ryle puts it 'Efficient practice precedes the theory of it'. Persons who have 'mastered' such rules may sometimes make mistakes, but their errors would have no tendency to show that the rules had been mischaracterised. Somewhat analogously we may distinguish between a speaker's grammatical competence (his mastery, revealed by his 'grammatical intuitions', of the rules of his language) and his performance (his successes and failures in applying the rules in particular cases). (p. 462)

The confusion between the three levels of model is revealed in the way in which the authors use the term 'rules'. Notwithstanding the 'somewhat' which qualifies the analogy in the above quotation, we may examine the way in which 'rules . . . may be implicit in performance before they can be formally characterised'. It is clear that they intend the rules to be idealisations. But in this case we can ask the question 'Characterised by who?' Scarcely by the learner since the ability to *formalise* rules is irrelevant to the ability to learn or use them (by the rationalist view), and scarcely by an observer since idealisations are always implicit in the data (unless by 'implicit' they imply the sense of the Meno dialogue, in which case they use a rather special sense of 'performance'). So, contrary to their intentions, the authors'

statement only makes sense in terms of a contrast between Models of Realisation and Performance.

There are similar difficulties when we ask in what sense someone may be said to have 'mastered' the rules even if it is in principle possible for the rules to have been 'mischaracterised'. If by 'rules', we are to understand throughout the sense of idealisations, then the statement is tautologically correct, since there can be no distinction between the rules and their characterisation. In this case the later description of a speaker's performance as 'applying the rules' is inappropriate as it is not one of the properties of idealisations that they can be part of a mechanism. If instead we are to take 'mastering the rules' as synonymous with 'learning the language' then, in the analogy, the speaker's grammatical competence would be on the level of a Model of Potential rather than that of an idealisation.

In either of these alternative interpretations the implication is clear that the learner must always end up with the same rules as those of the adults in his environment. Such a stance has not been adopted by all linguists working within the framework of generative grammar. Postal, for example, in considering sound change in a language observes that rules may be added to an adult grammar. He then suggests:

> Since the addition of rule R to a grammar G_1 may define a language of which the optimal grammar is not $R + G_1$, it follows that children in the next generation will, in such cases, learn not this but the optimal grammar. (Postal, 1968, p. 270)

King makes a similar point, claiming that a child:

> can come up with a competence – an internalised grammar – that is simpler than an adult grammar yet underlies a speech output either identical with adult speech for all practical purposes or different in relatively minor ways. (King, 1969, pp. 74–5)

It is clear that these authors are talking about a mechanism not an idealisation, for in the conditions specified, the *idealisation* of the adult's grammar will change as soon as he adopts the additional rule. Thus the linguist, given no

historical information, would characterise the adult grammar in the simpler form. Historical information could then be used to distinguish between the grammar of the adult and the competence which underlies his performance, or, in the terms I have suggested, between the Models of Potential and Realisation.

The three-way distinction can also be employed to analyse a passage where Shotter and Gauld argue that the development of functional models is dependent upon the existence of structural (idealised) descriptions. They say:

> It is difficult indeed to see how a functional theory, a theory of the underlying mechanism of sentence production and comprehension, could be developed without an adequate ('structural') characterisation of the grammatical 'rules' which essentially characterises that mechanisms's output. One could hardly hope to design a chess-playing machine before one had a 'tidy' description of its output in terms of the rules of chess. For, without such a description, it would be impossible to decide whether a move made by the machine actually was a permissible chess move. (p. 463)

To start with, if one wishes to compare chess with language the appropriate analogue for a human being would be a chess-*learning* machine rather than a chess-*playing* machine, the difference being that the former would have to learn the *rules* for itself. Such rules would be differentiated from tactics and strategies in that the rules could be indicated to the machine by a feedback statement indicating that a particular move was not permitted. (It would, of course, not matter whether the teacher had a list of forbidden moves or a set of rules.) We have to make sure that the machine understands such feedback (or, rather, that the feedback we give has the required effect on the particular learning mechanism), just as we have to start with a machine that can generalise both from the opponent's moves and from its own. But such contraints scarcely constitute 'innate ideas' of chess. The ideal rules would be intrinsic in the mechanism + environment combination but that is not the same thing. For

example the machine may formulate a rule such as:

> A pawn may move either one or two spaces forward except on the 3rd, 4th, 5th and 6th rows.

This will result in more complex computation than is strictly necessary but would not be detectable to the observer without special experimentation (such as switching to a 8 x 10 board). One may observe that many children do not consider a game of chess completed until one of the kings has actually been taken – a mischaracterisation that does not impair their game. It will also be apparent that the difficulty with a chess-learning machine would not be with the rule-learning but with the development of the strategies that increase its ability at the game.

I have tried to indicate how a chess-learning machine could develop internalised rules the nature of which could not be inferred from the rules of chess. Similarly, moving back to language learning, it should be evident how a theory of 'the underlying mechanism' of a speaker-hearer could be completely independent of particular rules which characterise the mechanism's output. We do not even need to make the rules explicit to decide whether or not a particular utterance is grammatical – all we need for that are our intuitions plus some criterion for evaluating these intuitions. (Though we should not forget that our intuitions about grammaticality will also be influenced by the Realisation Model.) The feedback given to children as they learn their language is more often by example than by a simple correction of an ungrammatical utterance. But neither of these require an explicit idealisation. The task of the psychologist is not to describe how the child learns a particular set of grammatical rules (even ignoring the question of which particular set of rules) but rather to suggest a mechanism which accounts for the acquisition of a particular skill. The utility of grammatical idealisations for this endeavour is an empirical matter.

In summary then, I have tried to show in this section that discussion of the relationships between linguistic and psychological models is often based on the assumption that there are only two levels of description whereas in reality three such levels are both necessary for an understanding of the issues

and apparent in the arguments used in the discussion of the issues. Because of this, the case made by rationalists against empirical theories of language learning is considerably weakened.

THE LEARNING OF DEEP STRUCTURES

We may now examine one of the arguments, which Shotter and Gauld repeat, to the effect that linguists have provided *logical* objections to empirical learning. These logical objections seem to be based on the implicit concept of an ideal learner whose resemblance to a child is similar to the relation between an idealised grammar and the Potential and Realisation models. Shotter and Gauld point to Chomsky's distinction between deep and surface structure and comment, with respect to the language user's mastery of the former: 'The child *cannot* learn his language solely from observations of surface structure, and so needs, from some other source, information showing how the utterances are organised.' (pp. 464–5, their italics) This claim echoes Chomsky, and Katz and Postal (1964, p. 173) among others, who use similar statements in favour of an innateness hypothesis:

> [the underlying deep structures] are part of the conceptual apparatus [the language learner] uses to specify the form of the language to which he is exposed, and not something to be acquired. It is fortunate that this postulate is tenable, since it is difficult to imagine an alternative. (Chomsky, 1967, p. 81)

An alternative is that the 'other sources' constitute non-linguistic information from the environment. There may be empirical theories which require that *only* linguistic information need be used by the language learner but not all empirical theories have such a requirement. At the current stage of development of generative grammars, there is a self-imposed restriction to regard as relevant only linguistic data, and moreover the data is restricted to isolated sentences. The child, however, is unaffected by these constraints. The deep relationship between active and passive sentences, for example, could be induced from their occurrence in similar environments. In this way utterances which

differ widely in their structure, such as 'The dog bit Alan' and 'Alan was bitten by the dog', could be related through the event. Pairs of utterances which more nearly resembled each other in their surface structure, such as 'Alan was bitten by the dog' and 'Alan bit the dog' could be distinguished. In ways such as this the *principle* of underlying structure could be established. Of course the language learning device would have to be able to distinguish between descriptions of an event and comments on it, to avoid trying to form a structural relationship between, say 'Alan bit the dog' and 'The dog died'. Fortunately it is not necessary for us to assume that the child learns the regularities of language in a single trial or that incorrect inferences are never drawn. It is clear that in the early stages of language acquisition the speech of individual children can be characterised by grammars which differ both in deep and surface structure (Bloom, 1970). The explanation of such individual differences would seem to present difficulties for rationalist as much as for empiricist theories. The incompleteness of current empirical accounts of language learning (see also Marshall, 1971; Morton, 1968, 1971) does not seem to justify their cavalier rejection.

SCIENCE AND ART IN EDUCATION

While Hamlyn's conclusions concerning the notion of innate ideas agree with my own views on language learning his conclusions concerning human learning in general seem to be unnecessarily pessimistic. In the middle of his paper he comments, 'I do not see it is possible to say anything both significant and general about human learning processes' and he concludes that the variability of individuals and of learning situations means that guidance on learning 'is likely to be a matter of compromise and art'. Consider, however, the possibility of developing the notion of preparedness in relation to human learning. Seligman, in the article already cited, suggests that the acquisition of language is prepared, using the operational criterion that 'minimal input should produce acquisition' (p. 414). This notion is fruitful in at least one respect – it forces us to reconsider the position of children who do not acquire language in the normal way.

Rather than thinking of them in some sense absolutely incapable of learning or simply as retarded normals we could regard them as being not prepared for language learning. This means that special procedures will be necessary to teach them. Gillian Fenn (personal communication) has been getting promising results with children who have a reasonable vocabulary and comprehension but who have not developed grammar. After extensive programmatic coaching(20 minutes a day for 75 days), these children, some mongols and others with undiagnosed aphasic disorders, had learned to use sentences with prepositional phrases, adjectival constructions and verb inflections. The mechanisms underlying their speech is probably different from that of a normal child (there is a suspicion, for example, that they are incapable of reordering established rules), but the important thing is that these children have come to use language creatively. It is too early to tell what the long-term effects of such training will be – we do not know, for example, whether language preparedness can be trained – but we do at least have the beginnings of a justification for the belief that merely to provide these children with a particularly rich environment is insufficient. They do not need *more* opportunity to learn than normal children, they need *different* opportunities.

'Preparedness' in relation to human learning is likely to turn out to be a complex concept. Some of its elements may have a strong genetic component but others are likely to be acquired. Strategies of learning and attitudes towards particular subject matter are both likely to be important. It is clear, though, that it is inefficient merely to require a poor learner to work harder.

One must agree with Hamlyn that the preconditions which must be satisfied before a particular being learns a particular thing are inconsistent in relation to our current theories and far from being understood. In our attempts to study learning we restrict the environment and in our urge to specify and control we may destroy the conditions necessary for the very phenomena we are trying to elicit and study. The consequences of this will depend upon what we do with our data. If we regard them as do adult scientists, believing in our power, then we will surely be misled. If we use them as do

children, treating them as yet another hint towards the attainment of a goal we have not yet fully comprehended then we may make significant steps. I would prefer however to stress what we might achieve rather than what might seem at this instant to be our possible limitations.

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