Some Remarks on the Nature of Linguistic Theory [and Discussion]

L. J. Cohen and R. B. Le Page

Email alerting service	Receive free email alerts when new articles cite this article - sign up in the box at the top right-hand corner of the article or click **here**

Some remarks on the nature of linguistic theory

By L. J. Cohen, F.B.A.

The Queen's College, Oxford OX1 4AW, U.K.

On questions about the relative roles of experience and heredity in the explanation of a person's linguistic ability, it is sometimes tempting to suppose that conceptual, analytical or grammatical arguments will carry us further than they in fact can. The crucial issue here is not one of empiricism against innatism. Instead it concerns the variety and specificity of the learning programs with which one needs to suppose that the neonate is equipped; four different modes of investigating this problem are currently distinguishable: grammatical, computational, psychological and neuro-physiological. The first two of these are inherently incapable of resolving the issue, since their results are open to alternative interpretations; and any eventual resolution must emerge from the latter two. Thus there is an important sense in which, *pace* Chomsky, linguistics cannot be regarded as a branch of psychology. Nevertheless native speakers' intuitions of grammaticalness have provided an indispensable source of data for linguistic description, and recent criticisms of this source have failed to construe correctly the function of intuitions.

1. The innatist issue

The problem of language straddles the frontier between the social and natural sciences. On the one hand, language functions as a primary instrument of human culture and social interaction; on the other hand, its genesis, operation and pathology is rooted in neural mechanisms. So it looks as though any attempt at a theory of language faces a certain fundamental dilemma. Should it aim to be a theory of the product, treated as a cultural artefact, or a theory of the producer, treated as a biological process? Either alternative seems equally one-sided and unsatisfactory. For example, in the previous generation, Bloomfieldian linguistics laboured at the accumulation, description and classification of spoken material, while behaviourist psychology studied the responses of speakers to spoken (and other) stimuli. As a result the psychologists did not properly appreciate the complexity of the material with which they were dealing, nor its richness of structure, since the constraints of their experimental methodology seemed to enforce concentration on rather fragmentary data. At the same time such a concentration on rather fragmentary data seemed perfectly legitimate within the context of a linguistic methodology that did not look for underlying patterns of uniformity beneath the apparent diversity of speech.

We owe it to Noam Chomsky, more than to any other person, that this dilemma no longer obstructs the advancement of knowledge. We have learned that the linguistic ability, for the development and exercise of which we require a biological explanation, must be characterized in terms of the best available grammatical theory; and we have also learned a lot about the variety of different kinds of grammatical theory that are possible. Nevertheless there is still a very long way to go. Not only is there still no settled consensus about the form that the grammar of a natural language must have, but as yet there is also extensive uncertainty about the relative

importance of experience and genetic programming in the explanation of a person's linguistic ability. On the latter issue, especially, it is sometimes tempting to suppose that conceptual, analytical or grammatical arguments will carry one further than they in fact can, and in this paper I shall briefly discuss the distinctions that have to be drawn to exorcize that temptation.

Consider the question whether a person's being able to learn the syntax of his native language is due to his being genetically programmed for the specific task of syntax learning. This question has sometimes been construed as involving a choice, in classical terms, between a rationalist and an empiricist theory of the mind or between a hypothetico-deductive epistemology and an inductive one. But these dramatizations of the issue do not present a correct description of the actual alignment between current theorizing and historical precedent. The choice is not – and never was – between specific pre-programming for syntax learning, on the one side, and a so-called 'bucket' theory of the mind, on the other, whereby knowledge somehow just collects in, rather than is collected by, the mind. Even Hume supposed that some forms of mental processing were innate, such as those effected by association; and Francis Bacon (whom Hume regarded as the founder of modern science) certainly thought of induction as presupposing a prior space of alternative hypotheses. The real question at issue now, as in the past, concerns the variety and specificity of the learning programs with which one needs to suppose that the neonate is equipped. In particular is it genetically programmed with a strategy devoted to the specific task of syntax learning, or only with certain rather more general learning strategies that are also applicable in some other learning tasks, such as in getting to know how to interact with the social or physical environment? I shall call this 'the innatist issue'.

In this context it is particularly interesting to consider what Professor Chomsky has just been saying about universal gramar, as a system of principles that determine the class of accessible natural-language grammars and their properties. Chomsky speaks of this system as a biological endowment that makes it possible for a grammar of the required sort to develop, when appropriate experience is given. And it is naturally tempting then to suppose, if one goes along thus far with Chomsky's framework of linguistic theory, that one is committed, by accepting the existence of some system of universal grammar, to accepting also that there is genetic programming for the specific task of syntax learning. The system of principles that determine the class of accessible grammars seems already to include an essential feature of such a program: it delimits a prior space of specifically syntactic hypotheses.

But one has to distinguish here between four importantly different dimensions of study for research into syntax learning, or indeed into any other branch of cognitive development. I shall call these the analytical, computational, psychological and neurophysiological dimensions, respectively.

2. The analytical dimension of research into syntax learning

In the analytical dimension we are concerned to describe the structure of a competence, such as the ability to learn the syntax of one's native language, and we shall in effect be characterizing the producer in terms of the product. So the syntax-learning competence can be described as an element in the possession of a function – universal grammar, as Chomsky calls it – that maps experience into grammar. But 'possession', in this context, means just a capacity to supply values to the function for given arguments. That is the nature of the hypothesis that can be checked against the actual facts of speech and the grammatical intuitions of native speakers. In particular, if we call this competence a biological endowment we say nothing to exclude

its being just a special case of some more general ability. Just as a grammar may be viewed as assigning structural descriptions to experience, so too a logic may be viewed as assigning structural descriptions (i.e. descriptions of logical form) to bits of reasoning, or ethics may be viewed as assigning structural descriptions (in terms of morally relevant features) to actions, concept acquisition may be viewed as assigning structural descriptions (in terms of salient similarities of appearance or function) to percepts, a scientific theory may be viewed as assigning structural descriptions (in terms of underlying causal factors and their relations) to phenomena, and so on. Now of course it is possible to think of each of these faculties (speech, reasoning, conduct, concept formation, cognitive enquiry) as the outcome of a separate learning-competence and therefore to attribute to a human being the possession of correspondingly distinct functions for mapping different aspects of experience into different bodies of knowledge. But that would not exclude the possibility of our being able to construct a much more powerful function that would embrace each of these – non-trivially – as a special case. We should be looking then for a general characterization of inductive learning, which would cover all its known forms, because in each case we should be assuming that, for any encounterable item of experience, there is some pattern or patterns of uniformity (grammatical, logical, ethical, perceptual, natural, etc.) to which it conforms.

For example, if the range of learnable rules of syntactic transformation is restricted by the requirement that the transformations should apply to structures, and not to unstructured sequences, it may be that this requirement would emerge as being implicit in any adequate function of a general kind for eliminative or inductive learning, in that all such learning assumes prior constraints on the range of admissible hypotheses and thereby on the concepts under which relevant data may be conceived. I select this example because the structure dependence of syntactic transformations is a principle that Chomsky has in the past (1976) considered particularly important in connection with questions about what belongs to universal grammar. But it is clear that *any* principle of universal grammar that is not to exclude children from being able to learn the syntax even of any existing human language, let alone of any possible human language, must have a highly abstract character, and to the extent that this must be so it becomes reasonable to suppose that such a principle might just reflect some even more abstract constraint which operates on inductive learning in general.

I suspect, therefore, that, whatever the function that finally emerges as the embodiment of universal grammar, we could always construct a much more powerful inductive function in terms of which, plus the relevant acquirable concepts – in particular, concepts of noun and sentence, as in a categorial grammar (see, for example, Lyons 1968, p. 227ff) – the grammatical function would be definable. The grammatical function's delimitation of a class of accessible grammars would emerge as being coextensive with the inductive function's delimitation of a class of admissible hypotheses over a certain area of its domain. But the fact that we have been able to construct such a function (which might, after all, turn out to be a rather complex one) tells us nothing about the mental mechanism – the device for actually acquiring a grammar – that underlies the competence. That fact is no more evidence for the existence of a general learning mechanism than the construction of the universal grammar is evidence for the existence of a linguistically specific one. It is one thing to describe a task that an organism can perform, such as supplying values to a certain kind of function, and quite another to detail a program for the execution of this task: we do not already have the program when we tell the programmer what he has to write a program for.

Accordingly the innatist issue is certainly not going to be resolved within the analytical

[23]

dimension of enquiry. This is the dimension in which intellectual competences of various kinds are mapped out, such as the competence to develop a tacit knowledge of syntax, phonology, ethics, concepts, logic, etc. Since the description of a competence is always the characterization of a producer in terms derived from a study of the product (or of the doer in terms of what is done), we can in principle always present a description of several intellectual competences either as an assembly of discrete elements, fragmented down to any desired level of specificity, or as a unified system, just as we can present the product – i.e. the knowledge – either as a set of heterogeneous rule systems or as different parts of a single system.

3. COMPUTATIONAL AND PSYCHOLOGICAL DIMENSIONS OF RESEARCH INTO SYNTAX LEARNING

I have been using the metaphor from computer science that has settled into cognitive psychology during the past generation. By speaking of the program for this or that information-processing mechanism one can pick out a distinctive field of theoretical investigation for cognitive psychology. While neurophysiology is occupied with the hardware of our faculty of language, for example, cognitive psychology is occupied with its software. But problems about the program for executing a competence have a computational as well as a psychological dimension, because the question about how a competence *could* be executed is obviously different from the question about how it is *actually* executed. Some progress in resolving the innatist issue might therefore be made within the framework of research in artificial intelligence. A program has already been constructed, for example (Power & Longuet-Higgins 1978), for a computer to learn how to correlate numerical expressions, in a given natural language, with numbers. If richer and richer programs for language learning and other forms of intellectual development can be developed, we may eventually be in a position to test out whether a general program for inductive learning can be constructed in such a way as to simplify the structure of the overall mechanism required, or whether the constraints on admissible hypotheses are so different from one field to another, over the different fields – syntactic, conceptual, ethical, etc. – in which children mature with experience, that a general program for inductive learning can achieve no worthwhile degree of simplification compared with an assembly of more specific programs.

But even if research in artificial intelligence eventually showed that the use of a general learning strategy here could achieve substantial economies of scale without involving inappropriate increases in computing time (on the time issue compare Chomsky & Miller (1963, p. 430), with Cohen (1970)), the innatist issue would still not have been conclusively resolved. Two further possibilities would still remain. Either Nature might have over-insured, as it were, and the neonate might be programmed both with an adequate general strategy and also with an independent back-up system of a more specific kind; or Nature might prefer a baroque assembly of relatively specific learning systems to a single, streamlined, comprehensive one. Evolutionary considerations about survival value seem insufficiently determinate to arbitrate between the three possibilities at stake in the innatist issue here. But additional light might be shed on the issue by investigations within the psychological dimension, i.e. by investigations into the actual learning performance of children under appropriately varied conditions. In particular it might be useful to investigate the extent to which, and the reasons why, the

achievements of the same child in different fields of inductive learning tend to show similarities or differences.

However, we already have quite a lot of experimental evidence for the hypothesis that the actually operative programs for speech production and speech comprehension may lack any obvious isomorphism with an analytically derived system of rules and principles for assigning structural descriptions to expressions. In particular, the transformation rules that figure as part of the latter system tend not to correlate with readily distinguishable procedures in the mental strategies that are inferable from experimental data. (The evidence is conveniently summarized in Greene (1972).) This need not surprise us, if we reflect on familiar differences in other areas between the characterization of a product (such as a pudding or a musical composition) in terms of its parts and their interrelations and the characterization of the processes that engender that product. Similarly, we might well find that the various routines that human beings actually operate for concept learning, syntax learning, ethics learning, etc., have no obvious one–one correspondence with the best analytically derived enumeration of the tasks that those routines are set to accomplish.

Finally, when we have gone as far as the methods of cognitive psychology can take us, with its computational metaphor for the mental mechanisms of information processing that it postulates, we must hope for some very considerable advances in neurophysiology. The innateness issue will not be completely resolved until we know just what happens, in chemical or electrical or physiological terms, when this or that component of a mental program or mechanism is operating. Meanwhile we must guard against the temptation to draw premature conclusions. For instance, evidence for the localization – in a particular part of the brain – of a particular element in a person's speech faculty is not necessarily evidence for the localization of the mechanism for acquiring that ability and is therefore not necessarily evidence against the hypothesis of a general learning strategy.

Thus the sense in which it is true to say that universal grammar (whatever its actual content may be found to be) is a biological endowment is quite far from being one in which a resolution of the innatist issue could be expressed. The thesis that human beings have the benefit of a biological endowment for universal grammar does not entail, and is not even evidence for, the thesis that they are genetically programmed with a distinct strategy for the specific task of syntax learning. Correspondingly the sense in which it is true to say, as has sometimes been suggested (e.g. by Chomsky 1968), that linguistics is a branch of psychology needs careful limitation. After all, by studying the moral judgements that people actually make, in different cultures and under different conditions, we might expect to be able to characterize a kind of universal ethics that delimits the range of admissible hypotheses for a child that is learning how to judge the behaviour of itself and others. So ethics, in this sense, is also a part of psychology, and so too are logic, probability theory, etc. That is, every intellectual competence falls quite properly within the field of psychology, in the sense that it sets a problem for psychology: how is execution of that competence to be explained? But in order to determine the content of such a competence we have to look elsewhere: at the product, not the producer. We have to use the analytical methods of linguistics, philosophy, logic, etc., not the experimental methods, or computational models, of psychology. So in the sense in which a discipline is defined by its problems, or by its methods, linguistics is as different from psychology as is ethics or logic.

4. The role of intuition as a source of grammatical data

Some further confirmation of this difference may be obtained if we consider the relative importance of intuition and observation as sources of the raw data against which grammatical hypotheses are to be checked. It has recently been argued (Sampson 1975) that no evidence either for the grammars of particular languages or for a general theory of language is obtainable from intuition, and that if any part of linguistics is based on intuition then it is not a science. But this argument relies on a number of misconceptions about the actual nature of the intuitions that are properly invoked in grammatical enquiry, and when those misconceptions are exposed the role of intuition in supplying data for grammatical analyses to explain or systematize becomes just as indisputable as the role of observation in supplying data for experimental psychology to explain or systematize.

It is easy enough to understand how the appeal to intuition gets itself a bad name. Some philosphers, for example, have thought of intuition as a distinct faculty of rational insight, which has a special claim on philosophers' attention. Spinoza, for example, and Bergson and Husserl, in their very different ways, all expounded doctrines of this kind. But the grand metaphysical issues on which those philosophers expected pronouncements from the oracle of intuition are of no interest in the present context. Again, we often appeal to intuitions or hunches about everyday issues that are in principle open to conclusive resolution either by the use of one's ordinary senses or by calculation or by the discovery of a mathematical proof. But at the points at which grammar or linguistic theory relies on the evidence afforded by native speakers' intuitions, no appeal to the senses or to mathematical operations would be relevant. Also the appeal to intuition has sometimes been taken to attach some special value to the data of introspection, so that a person who reports his intuitions is attributed a rather marvellous ability to read off the truth from the tablets of his inner consciousness. But the essential feature of an intuitive judgement for present purposes is that it is immediate and untutored, not that it is introspective. It must be immediate, in the sense of not being inferred from a more fundamental datum, because if it were so inferred that other datum would obviously have a better claim to be cited as evidence. And it must be untutored in relevant aspects, if it is not to reflect an already accepted hypothesis, in which case its claim to be cited as evidence would be question-begging. But so long as a person's inclination to judge a certain morpho-phonemic string to be well-formed is immediate and untutored, it counts as an intuition for present purposes even if it is not accompanied by any inward turning of attention. No introspectionist protocols are required by the grammarian from his or her native-speaker informant, only a judgement of grammaticalness.

Even so it may still seem plausible to think that there is something rather unscientific about basing a grammar on intuitively given data. Indeed it has been objected that utterances are hard behavioural data, concern with which can maintain the scientific status of linguistics, while intuitions are subjective acts of consciousness, concern with which can only serve to drag linguistics back into the dark ages. However, the comparison is wrongly drawn if it is drawn in these terms. A native speaker's intuition that the string S is grammatical is just his immediate and untutored (though in principle observable) inclination to take S as being well-formed, and in this sense he can have such an intuition if and only if he would be (equally observably) inclined to utter S whenever his circumstances, motivation, beliefs, etc., are precisely appropriate for a communication with the sense of S and also he is applying ideal standards of

care and attention in the linguistic formulation of his utterance. It follows that the difference between an utterance of S and an intuition of S's grammaticalness, as data for a grammar, is just that while the former constitutes an actual occurrence of S in human speech, the latter establishes a potential occurrence – i.e. a potential production by some speaker. Hence intuitions of grammaticalness can always provide a vital kind of data that actual utterances may often fail to present; and because of this it is exclusive reliance on the observation of actual utterances, not reliance on intuitions of grammaticalness, that fails to mirror essential features of scientific method. The point is that the experimental method in natural science normally requires us not just to rely on observations of the phenomena that happen to present themselves to our passive consciousness, but actively to manipulate circumstances so that we deliberately control the experimental situation, as far as we can, in respect of every factor that we think might be causally relevant. An analogue of this would occur in grammatical enquiry whenever we investigate what features of a sentence are dependent on what: at the simplest, for instance, we tell whether the form of a verb depends only on the number of the subject, or also on its gender, by experimenting with a native informant – we check out his intuitions over a suitably varied set of morpheme strings. (Chomsky's paper included several excellent examples of this kind of appeal to native speakers' grammatical intuitions.)

Of course, we may have been lucky enough to record actual utterances of a sufficient variety of strings, so that we do not need to have recourse to the contrivance of intuitions. But often that will not be so, or, even if an utterance of the required morpheme string is found, its validity as evidence may be somewhat dubious: perhaps it was uttered by a grammarian rather than by a grammatically untutored informant. Again, instead of being content to elicit the intuitions of native informants, it would theoretically be possible for a grammarian to try to elicit an actual utterance of each string in question. But, quite apart from the practical difficulty of contriving this even where the morpheme string is well formed, it would remain very difficult to determine in any particular case whether failure to elicit utterance of a particular string was due to the intrinsic ungrammaticalness of the string or to some misjudgement of the circumstances, motivation, beliefs, etc., to which utterance of the string would be appropriate. Negative evidence emerges naturally from the investigation of intuitions, not from that of utterances.

I therefore conclude that the systematic variation of relevant factors, which is so essential to the testing of scientific hypotheses, can be achieved in grammatical enquiry only by recourse to the evidence of intuitions. It is the attempt to rely instead on actual utterances that would be methodologically unscientific. Indeed, the outcome could only be a kind of natural history: a description of actually observed utterances in terms of their superficially evident features and a taxonomy constructed along corresponding lines. Attempts to discover underlying patterns of uniformity beneath superficial diversity would inevitably be frustrated by the inability to test predictions generated by novel theoretical hypotheses. It is no accident that the great advances in grammatical understanding that Chomsky's work has made possible have been associated with a readiness to make regular use of the evidence of native informants' intuitions, while the work of the earlier, Bloomfieldian period, which sought to build all its conclusions on corpuses of actually recorded utterances, was prevented by its methodology from achieving any depth of insight into grammatical structure. If anyone wants to combine approval for the new type of grammatical theory with disregard for the evidence afforded by intuitions, he is adopting an incoherent position.

Of course, there are problems about intuitions of grammaticalness that have to be faced. In

242 L. J. COHEN

particular, how far is it possible to distinguish intuitions of syntactic well-formedness from intuitions of semantic propriety? We may think that we can distinguish readily enough in English between the kind of syntactic deviance that is present in expressions like 'They comes here regularly', and the kind of semantic oddity that is present in, say, 'Husbands are often married to one another'. But our ability to draw this distinction may rest on the prior acceptance of some (admittedly inchoate) kind of grammatical theory, and informants in a more exotic or less educated speech-community might well be incapable of drawing the distinctions that the acceptance of such a theory makes possible. This is a well known crux in grammatical methodology. But philosophers of science will recognize its similarity to issues that arise in many other areas of experimental enquiry, where the breakdown of attempts to give a theory-free account of experimental data has not been taken to weaken the claim of experiment to be the ultimate touchstone of scientific success. It will suffice to say here that any attempt to rely on a corpus of actual utterances for grammatical evidence must face an analogous issue, since any description of the utterances, however preliminary, already presupposes some framework of descriptive terminology.

To sum up, then, what emerges here is that if we consider the four dimensions of syntactic enquiry with which the earlier part of this paper was concerned, neither the analytical nor the psychological dimension can dispense with the evidence afforded by native informants' intuitions about particular morpho-phonemic strings. But this evidence takes rather different forms in the two cases. So far as the analytical dimension of enquiry is concerned, what counts as a datum is the content of the intuition, i.e. the fact that this or that particular string is grammatical or ungrammatical, as the case may be. That is the fact that has to be predicted or explained, first by the grammar of the language (through its ability to assign a structural description to the string), and ultimately by universal grammar (through its admission of such a grammar for a particular language). But so far as the psychological dimension of enquiry is concerned, what counts as a datum is the existence of the intuition or the occurrence of the corresponding utterance. Nor is the difference here a trivial one. Any attempt to assimilate the two situations, and so to regard the occurrence of the intuition, rather than merely its content, as the datum for linguistics, would lead us back into the rather unrewarding conception of linguistics as a branch of psychology. Indeed the analogies with ethical or logical enquiry are quite plain here. There too it is the rightness or wrongness of a particular act, or the validity or invalidity of a particular piece of reasoning, which is the datum; the intuition of this rightness or wrongness, validity or invalidity, is a datum for the psychology of morals or of reasoning, not for ethics or logic itself. My claim is that clarity requires us just as much to reject the view that linguistics is a branch of psychology as to reject the view that ethics and logic are branches of it, and it should be remembered in this context that the psychologizing tendencies of many nineteenth-century logicians imposed a definite handicap on their research. But even though we reject the view that linguistics is a branch of psychology, we can still accept, as a legacy of Chomsky's profoundly original thinking, the intimate linkage that exists between the analytical and the psychological dimensions of language study.

REFERENCES (Cohen)

Chomsky, N. 1968 *Language and mind*. New York: Harcourt Brace.
Chomsky, N. 1976 *Reflections on language*. London: Temple Smith.
Chomsky, N. & Miller, G. 1963 Finitary models of language users. In *Handbook of mathematical psychology* (ed. R. D. Luce, R. Bush & E. Galanter), vol. 2, pp. 419–491. New York: John Riley.
Cohen, L. J. 1970 Some applications of inductive logic to the theory of language. *Am. phil. Q.* **7**, 299–310.
Greene, J. 1972 *Psycholinguistics*. Harmondsworth, Middlesex: Penguin.
Lyons, J. 1968 *Introduction to theoretical linguistics*. Cambridge University Press.
Power, R. J. D. & Longuet-Higgins, H. C. 1978 Learning to count: a computational model of language acquisition. *Proc. R. Soc. Lond.* B **200**, 391–417.
Sampson, G. K. 1975 *The form of language*. London: Weidenfeld & Nicolson.

Discussion

R. B. LE PAGE (*Department of Language, University of York, U.K.*). Mr Cohen said that the native speaker's intuition on which he wished to rely must be immediate and untutored. In my experience, however, the genuinely untutored native speaker is unable to answer such questions as 'Is this sentence grammatical in your language?', since he does not understand the nature of the question.

L. J. COHEN. As I said in my paper, the problem here is essentially the same as in other branches of experimental science: the source of data has to be interrogated under such refinement of controls that the source's response can be interpreted with appropriate cogency as an answer to the question at issue. It goes without saying, however, that those who have no explicit concept of grammar cannot understand the rather crude query 'Is this sentence grammatical?' The art of an investigation that started from scratch would be gradually to devise ways of distinguishing which of the occasions on which the informant treats a communication as bizarre are due to his surprise that the questioner has uttered it, to his belief that it is false, to his sense of semantic absurdity, or even perhaps to his criteria of syntactic well-formedness. But in practice such investigations rarely start completely from scratch: there are bilinguals, grammars of kindred languages, corpuses of analysed material, etc.