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Pseudo-controversy in linguistic theory

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Somebody once said that a person has only to speak, to be controversial. Certainly, in linguistics there is no shortage of controversial statement: rather, the problem for researcher and supervisor is to decide which of the many controversies is worth spending time on. There is nothing worse than a 'pseudo-controversy'—one which takes us no further forward in our theoretical understanding of a subject, because it turns out to be unresolvable in our present state of knowledge, or methodologically impracticable, or trivial in its empirical consequences, or wholly speculative, or vacuous. As an illustration of 'meta-controversy', such exercises might be instructive; indeed, one can always learn from the previous dead ends of research history. But no one wants to be the first to find a dead end for himself. One of the problems of contemporary linguistics is that it is becoming increasingly difficult to tell the difference between a fruitful and a fruitless controversy. When someone made a controversial statement in the 1940s and 1950s—so senior colleagues have told me—the consequences of the various positions adopted were on the whole clearer than they are today. But as the scope of linguistics has developed, and with the inclusion of psycholinguistics and sociolinguistics in particular, it has become increasingly difficult to anticipate the empirical, methodological or theoretical consequences of a controversial statement. In a linguistics which attempts to provide an integrated account of human behaviour, such as Pike's or Chomsky's, it is often impossible to trace the implications of an apparently straightforward linguistic argument so as to take account of its psychological, sociological, neurological or other consequences. The problem is at its worst when these broader issues are introduced into linguistic reasoning at the outset, as when theoreticians debate the general conditions which a linguistic theory is supposed to meet. Such a debate is inevitably controversial—but how fruitful a controversy is it?

As a starting-point, we may consider one of Chomsky's statements about the issue (1967, 100): 'it is going to be necessary to discover conditions on

theory construction, coming presumably from experimental psychology or from neurology, which will resolve the alternatives that can be arrived at by the kind of speculative theory construction linguists can do on the basis of the data available to them.' It would seem that neurophysiological and neuropsychological factors are to play a major role in developing our evaluation procedures for linguistic theories. The same point is made by Katz (1964, 133–4), along with some further implications which can usefully be quoted in full:

> since the psychologist and the mentalistic linguist are constructing theories of some kind, i.e. theories with the same kind of relation to the neurophysiology of the human brain, it follows that the linguist's theory is subject to the requirement that it harmonize with the psychologist's theories dealing with other human abilities and that it be consistent with the neurophysiologist's theories concerning the type of existing brain mechanisms. . . . Further, by subjecting a linguistic theory to this requirement we make it more easily testable. For the requirement enables us to refute a linguistic theory if we can find psychological theories or facts that are inconsistent with it or neurophysiological accounts which describe brain structure in a way that precludes the linguistic theory from being isomorphic to any of the structures in the human brain.

This general view, in due course, led to the development of the so-called APHASIA PARADIGM of linguistic enquiry, which has attracted a great deal of controversy since it was first propounded by Whitaker and others in the late 1960s.[1] The main aim is to take pathological linguistic data, such as that provided by aphasic patients, and analyse it in the expectation that it will give us insight into normal linguistic behaviour and into the nature of linguistic theory in general. How fruitful is this approach, and its associated controversy, likely to be?

Whitaker gives three reasons for the linguist's interest in pathological data. Firstly, there is a direct contribution to a putative neuroscience (1969, 135):

> Someday man's understanding of the brain and its behavioral mechanisms will progress far beyond the contemporary awareness of a few biochemical properties of neurons, a rough approximation of electrical events and partially specified functions for some of the neuro-anatomic structures. And when that day arrives, the biochemist, physiologist, anatomist, neurologist and all others concerned with brain functions will suddenly be in need of a specification of behavioral units that can be correlated with their information.

Therefore, he argues, we should avoid any 'artificial dichotomy between an abstract linguistic model of [the *actual* knowledge of language] and the neurological structures and functions and events which *are* that knowledge'. Secondly, there is an associated gain, in that such studies will help to provide an explanation for the qualitative differences between human and animal communication. Species-specificity, it is argued, implies 'genetic specificity or a structural-functional uniqueness in the brain' (1969, 8).

But the third, and main justification is to provide empirical evidence

[1] See Whitaker (1969; 1971), Weigl and Bierwisch (1970), Schnitzer (1974).

bearing on linguistic hypotheses (1969, 69). Whitaker argues that we must avoid creating models of language that bear no relation to neurological reality:

> the closer we get to the brain, the more likely we are to be discussing the realities of the structure of language (1969, 135). . . . there are *a priori* grounds for bringing neurological information to bear upon linguistic theory. . . . Ultimately we have to. Certain structures and functions of the nervous system are the substrate of both our 'knowledge' and our 'use' of language (1969, 7). . . . language is a product of man's nervous system—literally . . . [it] has physical reality in the human brain (1969, 17).

The argument may be summarized as follows: linguistic hypotheses represent underlying psychological reality; therefore they are hypotheses about normal brain structure and function; abnormal brain structure produces abnormal linguistic behaviour; it is, however, the same brain which produces both the normal and the abnormal behaviour; it should therefore be possible to relate the two types of behaviour, such that the same linguistic theory can account for both; if so, we have the neurological data acting as a constraint on linguistic theory (in addition to whatever other constraints linguistic theory itself imposes, such as descriptive adequacy).

There are, however, several difficulties with this line of argument. Whitaker makes it plain (e.g. 1971, 140) that there is no reductionism involved: linguistics is not to be 'reduced' to psychological or neurological states. Rather, the aim is to establish equivalences between subject areas or categories 'at similar . . . corresponding levels of abstraction' (cf. also the use of 'harmonize' and 'consistent' in the above quotation from Katz). But it is unclear how one defines 'a similar level of abstraction' between two theories, or places the claims of different theories in correspondence—a point made by Black (1970, 457) in a critique of Chomsky (though Black was talking about the relationship between mathematical and psychological premisses, not neurological ones). Is there any precise formulation that might be given to a hypothesized equivalence involving linguistic classes (such as noun), categories (such as tense) or rules (such as $S \rightarrow NP+VP$), on the one hand, and neurological 'realities' on the other? Moreover, how appropriate is the characterization of the nervous system in such terms as 'physical reality'? There are many models of neurological activity, involving electrical, chemical, molecular, information theoretic and other bases. A phrase such as 'structures and functions of the nervous system' (cf. above) is reminiscent of traditional controversy in linguistics, concerning the best way of representing the 'reality' of these notions. Given the possibility of alternative neurological models which attach different significance to concepts such as 'structure', 'function', 'substrate', and so on, the use of the term 'reality' becomes less meaningful, and the likelihood of our being able to specify clear equivalences more remote.

Nor are the above arguments any more persuasive if we investigate particular linguistic features. Here, the aphasia paradigm maintains that if a linguistic construct can be shown to be lost after a lesion, without other aspects of language being affected, then this is evidence for the

representation of such constructs in the brain and thus for their functional autonomy in linguistic theory. As Weigl and Bierwisch (1970, 13) put it, these constructs 'must be considered as relatively autonomous functional units even in normal performance'. On this basis, the 'neurological reality' of several linguistic constructs has been proposed: for instance, the distinction between the main modalities of speech/listening/reading/writing, and between the levels of phonetics/phonology/syntax/semantics (bringing together several claims from Whitaker, and Weigl and Bierwisch); also the reality of various aspects of deep structure, of semantic fields and features, and of some specific grammatical transformations and phonological contrasts, such as tense/lax (Whitaker 1969, Ch.4; Schnitzer 1974). The arguments are put both positively (e.g. Whitaker proposes a specific underlying structure for the noun phrase; Schnitzer proposes a copula-creation transformation) and negatively (one of Whitaker's patients 'does provide evidence against a linguistic theory which fails to distinguish semantic and syntactic aspects of language . . . and more significantly argues against . . . the generative semantics proposal/(1969, 100).

The trouble is, that it is possible using this rationale to hypothesize the neurological reality of far too many linguistic constructs—including several from incompatible theoretical backgrounds. It is rarely if ever going to be the case that aphasic data will unequivocably support a single linguistic analysis or theory. Even assuming that enough data has been analysed from a sufficiently large group of patients to enable a generalization about deficit to be made,[2] there would still be several alternative ways of identifying the deficit. For instance, lack of ability to use a phonological contrast still leaves open the question of whether distinctive feature theory, phonemic theory, prosodic theory, or whatever is 'correct', as all might be used to describe the lost contrast. Or again, to show that an aphasic has lost a syntactic form (e.g. the ability to use adjectives with a noun, or to use the passive) does not clarify whether a structuralist, tagmemic, transformational or other analysis of the category is going to be supported by neurological evidence. Linguistic controversy on these matters is not going to be settled by an appeal to neurological data; on the contrary, these data are quite ambivalent, as the main debate in this literature, over the competence/performance issue, demonstrated.

This debate focused on the question of whether aphasic data should be described as a disorder of competence or performance, in Chomsky's original sense. The debate now seems somewhat dated, given the criticisms that have been levelled at the usefulness of this distinction since (cf. Matthews 1979), but the issues it raised are worth reviewing (see also, Lesser 1978, 45.ff.). Weigl and Bierwisch (1970), amongst others, argued that aphasic language could be analyzed as a disorder of performance: competence was intact, the aphasia being 'a disturbance of the access to the knowledge of language' (*ibid.* 14). De Saussure (in passing) seems to have held a similar view:

[2] Whitaker avoids studying aphasic language behaviour statistically, preferring to use a single informant basis. But the variability between patients is such that some statistical reasoning cannot be avoided (see further below).

> What is lost in all cases of aphasia or agraphia is less the faculty of producing a given sound or writing a given sign than the ability to evoke by means of an instrument, regardless of what it is, the signs of a regular system of speech. The obvious implication is that beyond the functioning of the various organs there exists a more general faculty which governs signs and which would be the linguistic faculty proper.[3]

Weigl and Bierwisch's evidence is threefold: that some modalities remain intact within aphasia (e.g. speech may be affected; reading may not be), suggesting a 'single underlying competence'; that aphasics fluctuate in their linguistic skills, suggesting variable access to their ever-present competence; and that aphasics 'de-block', i.e. devise an alternative strategy to avoid a particular linguistic difficulty, which ultimately enables them to use a linguistic feature. By contrast, Whitaker argues that aphasic data bears directly on competence: competence is the ' "core" of the central part of performance . . . equated with the representation of language in the central nervous system' (1969, 11). A linguistic feature or system, in this view, is seen as part of competence if it is central, i.e. appears in all modalities of language; if a feature/system is present in only some modalities, then it is part of performance. Using this distinction, he attacks the Weigl and Bierwisch position, on two main grounds: that there are some permanent deficits, where there is no fluctuation in ability; and some of these do cut across all modalities. How is such a debate to be resolved?

In a sense, it does not need to be resolved, for it is a pseudo-problem—an artefact of the competence/performance distinction.[4] Neither approach can in the end decide on what is competence and what is performance. Whitaker points out that Weigl and Bierwisch cannot distinguish between competence which is lost as opposed to competence which is blocked. But likewise, if the central nervous system gives rise to both competence and performance (cf. above), there will be similar difficulties for Whitaker—for example, in deciding whether a linguistic problem is due to a limitation of competence or a limitation due to memory or attention. Whitaker in fact allows at one point that a competence deficit may be variable - when there are fluctuations in all modalities (1969, 71). But this makes it impossible to say anything unambiguous about the neurological basis of the competence/performance distinction. It is not particularly surprising, then, that one year after his 1969 publication, Whitaker stopped trying to refine the distinction; and in a later paper (1971, 145) abandoned it altogether. Likewise abandoned are such 1969 views as the proposal of a specific underlying structure for the noun phrase, the evidence for which is 'less than secure' (1971, 221), the support for the lexicalist position, which 'may have been a premature claim' (1971, 230), and the argument for the syntax v. semantics distinction—an issue which 'cannot be independently verified from this data' (i.e. the 1969 work) (1971, 215).

[3] 1916, 11 (trans. W. Baskin). Cf. also Critical Edition, Engler 3291, App. to Vol. 4.

[4] For pseudo-problems, see Abercrombie (1963). The conception of competence is in any case puzzling: on this view, any linguistic features which distinguish speech from writing would have to be considered performance features, by definition, e.g. almost the whole of intonation.

The more one reads in this literature, the more one feels that the whole theoretical debate is premature. Why should this be so? One reason is empirical. It would seem that insufficient aphasics have had their language systematically examined to warrant the generalizations which are being made about them. To take some basic empirical questions: just how much variation is there in the linguistic behaviour of aphasic patients? just how abnormal is this behaviour, compared with our everyday speech? The lack of published empirical studies has not stopped the formulation of major theoretical positions. Whitaker, for example, admits that there is some 'idiolectal variation' (1971, 168.ff.), but says that 'this fact makes the linguistic analysis of aphasia no less and no more difficult than the linguistic analysis of normal language behavior', and concludes 'aphasic language behavior is a subset of normal language behavior' (1971, 169; cf. 1969, 48–9). Yet all of this is on the basis of an analysis of only a few patient samples; and Schnitzer, similarly, bases all his claims on a few hundred judgements taken from a single speaker. One cannot assume representativeness: it is the representativeness one is trying to prove. One cannot even assume that one is dealing with an idiolect; it is the systemicness one is trying to prove.

The empirical weaknesses in the aphasia paradigm are due partly to the level of generality at which the characterization of aphasic data has been arrived at. Using only the selective and linguistically superficial criteria of the main aphasia tests, it is not too difficult to point to gross similarities across patients. But as soon as more detailed grammatical, phonological or semantic approaches are used, the similarities become far less obvious. Indeed, there are several basic aspects of aphasic language which have received hardly any study, e.g. the grammatical role of the patient's prosody, the paradigmatic or syntagmatic structure of the patient's semantic fields, and the discourse connectivity within the patient's grammar (see further, Crystal 1981). Above all there are the complex linguistic interdependencies between patient and clinician. It is a truism that the complexity of a therapist's linguistic stimulus will be a major factor in determining the nature of the patient's response—which makes it all the more surprising that systematic analysis of input and reinforcement language has not yet taken place. But until it has, theoretical conclusions such as those made above are undoubtedly premature. Failure to elicit a structure may tell us more about the limitations of our eliciting strategies than about the structure of the patient. Putting this another way, is the failure due to the patient's lack of competence, or the clinician's?

The urgent need for a meticulous linguistic analysis of patients' language behaviour is apparent, with reference both to the clinician's intervention strategies and the constraints imposed by the clinical setting in which the aphasic finds himself.[5] 'What then is a naturalistic environment for an aphasic patient?', asks J. M. Wepman appositely, in discussion following a paper of Jakobson's (1971, 326). On top of that, one might also ask what presuppositions an aphasic patient brings to the clinical setting. I am

[5] Cf. this point made in the context of child language disability (Crystal 1980).

reminded of the scene in *Wings,*[6] where the aviatrix recovers consciousness following her stroke to be greeted by the clinicians asking her such questions as 'Who is the President of the United States?' Only enemy intelligence officers would be asking her such questions, she reasons, and so she 'decides' to say nothing! Seeing the reason for a question is often part of the information needed in order to answer. The lack of such awareness may well account for some of the inadequacies found in patients who try to respond to question batteries in conventional tests. Putting the questions into full and motivating contexts often produces very different results. Again, we are faced with the question of whose competence we are studying—the test-designer's, in this case?

There can only be a handful of descriptions of samples of aphasic patients' language in print—linguistically sophisticated descriptions, that is, where proper attention has been paid to the need for a good transcription, involving intonation, stress, and so on, and with the analysis taken down to a depth of detail comparable to that found in other fields of descriptive linguistics. Those that I have seen, and those I have made myself, have so far indicated one major 'finding'—that the differences between patients are far more striking than the similarities. Doubtless, as more patients come to be analysed, we shall begin to see the broad outline of the wood, rather than the trees which at present take up all our attention. But it will be a far more complicated wood than has been suggested to us so far. The real controversies, perhaps, have yet to be discovered.

But it is not simply an empirical issue. Even if our aphasic data were reliable and representative, we would still be unable to use this as evidence in support of the claims of specific linguistic theories, for the evidence will logically bear any of the available alternatives. The early debate focused exclusively on issues within the transformational-generative paradigm of inquiry. More recently, other linguistic or psycholinguistic theories have been proposed as alternative candidates for neurolinguistic support. Schnitzer, for example, thinks that stratificational grammar might allow for 'greater clarity in determining whether data do or do not support a certain theoretical position' (1978, 359). He also mentions cognitive grammar (in the sense of Lakoff and Thompson (1975)) as a further alternative. Doubtless there are other neurolinguistic papers currently being written which will argue the merits of the several other theoretical positions to be found in contemporary linguistics. But one wonders if there is any point in continuing this research theme, in the absence of any principled way of resolving the competing claims. Ironically, Schnitzer's review of the uncertainty and tentativeness which has bedevilled the aphasia paradigm leads him to conclude: 'One thing is certain: if linguistics is to become a science, it will have to make use of hard data of the kind suggested, in choosing among proposed theories' (1978, 359). Yet this is precisely the conclusion which is least certain of all.

[6] This play, by Arthur Kopit, was first produced on stage at the Yale Repertory Theatre in February 1978. The play had its first production in June 1977 on American National Public Radio's *Earplay* project.

What then is the linguist, anxious to discover conditions for his construction of theories, to conclude? Whitaker, and the others, are not interested in linguistic theories which make no claims about neurological reality: 'Although linguistic theories can be abstract in this sense, as such they are not of interest to me' (Whitaker 1969, 80–1). But in our present state of knowledge (or in the foreseeable future), linguistic theories can make no testable claims about neurological reality, not simply because we do not know what neurological reality is, but because there are too many variables intervening between language and the underlying factors in a patient's behaviour. There is plenty for the linguist to do, in trying to tease apart these factors, and in attempting to think predictively about patients' linguistic behaviour. But when the linguist works in this way, he is not using aphasic data to test his theories; rather it is the other way around: he is attempting to impose some system on aphasic data using whatever theory he has been brought up to believe in. In this field of study, it is the 'hocus-pocus' linguist, and not the 'God's truth' one, who seems likely to prosper.

Are the problems raised by this debate unique to neurolinguistics? I do not think they are. Chomsky, in the above quotation, mentions experimental psychology as another source of constraints on linguistic theory construction (though he is not optimistic about the prospects (1967, 100)). One might go further than this, in searching for extra-linguistic factors with which linguistic theories might need to be in tune, and which would accordingly form part of any evaluation procedure. Why not cite sociological, semiotic, anthropological and social psychological factors, for instance? There could also be a whole range of pragmatic factors, relating to the use which the proposed theory would be put—presumably a major consideration in applied linguistics, yet by no means excluded from 'linguistics proper'. But the 'realities' underlying these other areas seem as chimerical as the neurological one, as soon as we start to investigate them. At a recent conference on physiological psychology, a psychologist colleague was bemoaning the way in which he felt contemporary psychology seemed no longer concerned with facts, but only with methods. He spoke as one bemused by the increasing statistical, computational and electronic sophistication required of him, and by the proliferation of models generated by other disciplines than his own. To go to psychology or biology for definite 'conditions' on linguistic theory is to go on a wild-goose chase: psychologists and biologists, no less than linguists, are looking for conditions on theory construction too. Indeed, ironically, these days the goose chase leads us back home again, in view of the way in which these other disciplines have constructed new models for their endeavours in which major roles are played by such notions as deep structure and competence. The moral is plain: as linguistics attempts to develop its concept of explanatory adequacy, it would do well to adopt a narrower rather than a broader frame of reference, if it is not to avoid pseudo-controversy. Evaluative criteria for linguistic theories must come from linguistics itself: there currently seems only irrelevance or ambiguity to be had from outside.

References

Abercrombie, D. 1965: Pseudo-procedures in linguistics. In D. Abercrombie (ed.), *Studies in phonetics and linguistics.* London: Oxford University Press.

Black, M. 1970: Comment on N. Chomsky's 'Problems of explanation in linguistics'. In R. Borger and F. Cioffi (eds.), *Explanation in the behavioural sciences.* Cambridge: Cambridge University Press.

Chomsky, N. 1967: Discussion of I. Pollack's 'Language as behavior'. In C.H. Millikan and F.L. Darley (eds.), *Brain mechanisms underlying speech and language.* New York: Grune & Stratton.

Crystal, D. 1980: Research trends in the study of child language disability. Paper given to the Symposium on Research in Child Language Disorders, Madison, Wisconsin.

———1981: *Clinical linguistics.* Vienna and New York: Springer.

de Saussure, F. 1916: *A course in general linguistics.* New York: Philosophical Library, 1959.

Jakobson, R. 1971: Linguistic types of aphasia. In *Selected writings* **2.** The Hague: Mouton.

Katz, J.J. 1964: Mentalism in linguistics. *Lg.* **40,** 124–37.

Lesser, R. 1978: *Linguistic investigations of aphasia.* (Studies in Language Disability and Remediation **4.**) London: Edward Arnold.

Matthews, P.H. 1979: *Generative grammar and linguistic competence.* London: Allen & Unwin.

Lakoff, G. and Thompson, H. 1975: Introducing cognitive grammar. In C. Cogen, H. Thompson, G. Thurgood, K. Whistler and J. Wright (eds.), *Proc. 1st. Ann. Meet. Berkeley Ling. Soc.* Berkeley: Berkeley Linguistics Society, 295–313.

Schnitzer, M.L. 1974: Aphasiological evidence for five linguistic hypotheses. *Lg.* **50,** 300–15.

———1978: Toward a neurolinguistic theory of language. *B & L* **6,** 342–61.

Weigl, E. and Bierwisch, M. 1970: Neuropsychology and linguistics: topics of common research. *FL* **6,** 1–18.

Whitaker, H.A. 1969: On the representation of language in the human brain. Working papers in phonetics **12.** Los Angeles: University of California.

———1971: Neurolinguistics. In W.O. Dingwall (ed.), *A survey of linguistic science.* University of Maryland Linguistics Program.