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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 statements. 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 unobservable 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 early 1940s—it is argued—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 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 which a linguistic theory is supposed to meet. Such a debate is inevitably controversial—but how fruitful is it?'
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 premises, 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 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 unequivocally 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 analysed 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 which would be the
linguistic faculty proper.

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
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. 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).

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, 168; 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 systeminess 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. 'What then is a naturalistic environment for an
aphasic patient?', asks J. M. Wepman appropriately, 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

3 Cf. this point made in the context of child language disability (Crystal 1980).
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’ behavioural. 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.

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References


