

THE NOTTINGHAM LINGUISTIC CIRCULAR

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by

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EDITORIAL

Contributions to this number of NLC follow a gradation or, to use a once-fashionable expression, a "cline" - moving from a controversial discussion on a matter of linguistic theory into a descriptive account of the distribution and performance of a lexico-stylistic feature in French and so to an article and a review on topics related to the function of language in society. (The tilting of the "cline" must of course depend on the reader's preferences!).

We leave it to these excellent articles to recommend themselves. Our intermediary function as editors has been restricted to setting the papers in what seemed to be the best order and to a timorous excision of one or two highly charged expressions from the vigorous exchange between Andrew Radford and Geoffrey Pullum. This reluctant censorship has in no way intruded upon the substance of their arguments.

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NOTICES AND PROSPECTS(1) Forthcoming meetings:

- Nottingham, Easter 1977,
Date to be arranged. Child Language Seminar, c/o M W Stubbs,
English Studies, University of Nottingham
NG7 2RD
- Southampton, 13-15 Dec
1976 (probable dates) Languages for Life, BIAL Seminar, c/o
John Mountford, La Sainte Union College,
Southampton SO9 5H3
- Walsall, 29-31 March 1977 Linguistics Association of Great Britain,
Spring Meeting, c/o Robin Fawcett, Dept
of Language Studies, West Midlands College,
Gorway, Walsall, Staffs.
- London, 6-8 Nov 1977 Linguistics Association of Great Britain,
Autumn Meeting, c/o R A Hudson, Dept of
Linguistics, University College, London WC1.
- Lancaster, Spring 1978 LAGB, Spring Meeting.
- Philadelphia Marriot, USA,
28-30 Dec 1976 Linguistic Society of America, 51st Annual
Meeting, c/o LSA, 428 E Preston St,
Baltimore, Maryland, 21202, USA.
- Honolulu, Hawaii, July-
Aug 1977 Linguistic Society of America, Summer
Institute, c/o LSA at address above.
- Hawaii, 18-20 Aug 1977 Symposium on Austronesian Linguistics, in
connection with 1977 Linguistics Institute
of the LSA, c/o L A Reid, Dept Linguistics,
University of Hawaii, 1890 East-West Rd,
Honolulu, Hawaii 96822.
- Salzburg, Austria, July 25-
Aug 26 1977 Linguistic Summer School, c/o Institut für
Sprachwissenschaft der Universität Salzburg;
Imbergerstrasse 2, A-5020, Salzburg.
- Vienna, Austria,
22-26 Aug 1977 3rd Symposium on Semiotics, c/o Institut
für Sprachwissenschaft der Universität
Wien, Liechtensteinstrasse 46a, A-1090,
Wien.
- Vienna, Austria,
29 Aug - 2 Sept 1977 XII Internationaler Linguisten-Kongress,
c/o Linguisten-Kongress, Postfach 35,
A-1095, Wien.
- Pisa, Italy,
Aug/Sept 1977 4th International Summer School,
Computational and Mathematical Linguistics,
c/o A Zampolli Director of the International
Summer School, CNUCE, Via S Maria 56100,
Pisa, Italy.

Chicago, USA,
28-30 Dec 1977

Linguistic Society of America, Annual Meeting, c/o LSA at address above.

Lucerne, Switzerland,
27 March - 1 April 1978

Congress on Language Learning, c/o 1978 Congress on Language Learning, Congress Office, Eurocentres, Seestrasse 247, CH-8038, Zürich, Switzerland.

Montreal, Canada,
21-26 Aug 1978

5th International Congress of Applied Linguistics, Organised by Canadian Association of Applied Linguistics under auspices of International Association of Applied Linguistics.

(2) Reports on Conferences

The Teaching of Linguistics in Higher Education, Middlesex Polytechnic, 14-16 July 1976.

This was the second conference on the theme of how to teach Linguistics, and a third one is planned for next year, so clearly as participants we thought it was worth it. The aim of this year's Seminar was practical: all the speakers put their necks on the block and actually said what they taught and how, and in most cases produced duplicated teaching materials, gave demonstration lessons or even courageously brought along a video-tape of real class sessions. No-one reached any startling conclusions about a theory of linguistics teaching. There was not much attempt to discuss things at this level. But what I came away with was several ideas for classes, some of which I have already tried out on students. Most general points about teaching methods remained implicit, but nevertheless came over very strongly in the contrast between different teachers' demonstrations. The conference members were generally in the role of supposedly, and often actually, naive students. We were lectured at in traditional fashion, with tree-diagrams on the blackboard and cartoons on the overhead projector. We were taken through prepared handouts. We were made to work out rules for Russian syntax and play with Japanese haiku. And we watched a video-tape of students learning TG. The Seminars are organised by Mike Riddle, Senior Lecturer in Linguistics, Middlesex Polytechnic, The Burroughs, Hendon, London, NW4 4BT, who could be contacted about next year's session.

(M W Stubbs)

Research Seminar on Sociolinguistic Variation, West Midlands College, Walsall, 9-11 September 1976.

A mixed collection of papers. The largest group were concerned with applying and developing early Labovian survey methods in Britain: in Reading (Trudgill), Belfast (Milroy and Milroy), a northern Ireland village (Dougias), Edinburgh (Reid) and Scouse (Knowles). Various reports on work in Africa: Swahili and English in Mombasa (Russell), Yoruba-English code-switching in Nigeria (Akere), register systems in Ghanaian English (Ure), post-creole in South Ghana (Ellis).

And on other countries: attitudes to Pakeha English and Maori in New Zealand (Holmes), West Indian English in Britain (Sutcliffe). Reports on major surveys: the Caribbean survey (Le Page), the Orleans corpus (Butterworth). And miscellaneous papers on nonverbal communication, articulatory settings in Edinburgh, and other topics. A stream of short papers were presented: 14 half-hour papers on the second day. So this didn't leave much time for thinking or discussion. My main impression is a blur of facts to be added to my store of sociolinguistic data: that New Zealand teachers are wont to explain "Maori underachievement" by reference to their "restricted language" - familiar statement?; that sociolinguistic variation is demonstrably present in rural as well as urban communities; that working class Belfast speakers are not good at distinguishing RP from Dublin accents; that "nous" is on its way out in French and is being replaced by "on"; that /t/ is pronounceable as a fricative in Scouse; and so on. Theoretical discussion was sporadic, although several speakers were clearly trying to get out of the grip of the "ghost of early Labov" as Le Page put it. Criticisms and problems with the Labovian (1966) methodology which emerged over several papers were: the difficulty even skilled phoneticians have in coding phonological variables; the quasirandom sampling of speakers; and the sociolinguistic variation in the researcher's speech as he interviews different informants.

(M W Stubbs)

Third Systemic Theory Workshop, University of Nottingham,
19-21 July 1976.

Following the successful meetings held at Walsall and Colchester in previous years, some linguists with particular interest in the systemic model met to hear and discuss nine papers on various aspects of systemic theory.

The meeting began with an attempt by Mr. C.S. Butler (Nottingham) to relate Halliday's accounts of mood and modulation to what was felt to be a more truly functional account of the sociosemantics and syntax of directives in English. This was followed by a paper by Mr. H.M. Davies (Stirling), in which he discussed his very interesting work on the relationship between intonation and information structure in loud-reading.

The second day began with two papers on the processes of 'realisation', by which choices from systems become represented in the surface structure of the language. Two contrasting schemes were presented: Miss H.M. Berry (Nottingham) showed, by means of an actual derivation, how structures and formal items could be derived stepwise from systemic features by the processes of inclusion, discontinuity, conflation, insertion, concatenation and particularisation; Mr. R.P. Fawcett (Walsall), on the other hand, favoured a schema in which realisation statements specified the filling of particular slots within a pre-existing 'starting framework'. Realisation was also the topic of a short paper circulated in advance by Dr. R.A. Hudson (London) who was unfortunately unable to be present.

The remainder of the second day was occupied by two further papers. Mr. J. Martin (Toronto) discussed the area of 'phonicity', and presented semantic networks dealing with the ways in which information located in the contextual configuration of an utterance contributes to the interpretation of that utterance. Mr. W.I. Downes (LSE, now East Anglia) outlined some ways in which the term 'function' is used by systemicists, and distinguished between semantic, thematic and grammatical functions, also pointing out ways in which transformational linguists treat the notion of grammatical function.

On the final day of the meeting we heard three papers. Miss E.C. Davies (London) discussed the place of semantic roles, such as 'teller', 'performer', 'knower', 'decider', 'speaker', 'addressee', in a linguistic description. Professor J.Mc.H. Sinclair (Birmingham) then told the meeting of his view that systemic linguistics failed, in a number of important respects, to meet the requirements of a model which could satisfactorily handle discourse phenomena. This eloquent paper was a salutary reminder that systemicists are far from having produced a fully comprehensive theory, able to cope with all that they would like to cope with. We can, however, take comfort in the fact that in many of the areas of particular interest to the systemicist (including discourse analysis) TG has even less to offer. The meeting ended with a paper by Dr. J.O. Ellis (Aston) on systemic theory in comparative descriptive linguistics.

Perhaps one of the most successful features of the meeting was the beneficial effect of allowing each speaker 1½ hours for his paper and discussion. So often we find that long papers suffer from an at times embarrassing lack of structural coherence: at the Systemic Workshop the deliberate inclusion of only a relatively small number of papers produced papers which treated their topics in some depth, and which prompted lively discussion.

(C S Butler)

Annual Meeting of the British Association for Applied Linguistics,
Exeter, 14-16 September 1976.

The keynote lecture by Dr. A Crystal was followed by 14 papers. In accordance with past practice some were devoted to a common theme, which this year was 'syllabus design', others ranged more widely. Descriptions of the Council of Europe unit credit scheme (A.J. Peck) and of the communicative syllabus for English being designed at Reading (K. Johnson and K. Morrow) fell into the first category, as did contributions by G. Seidmann, P. Riley and C. Candlin. Papers on 'The laryngograph in foreign language teaching' (Baird), on 'Language Planning' (T.Gorman), 'Distractors in Listening and Reading Comprehension' (Culhane) and on 'The school-child's expansion of the major auxiliaries' (Wilding) belonged to the second group. Two talks, also in this latter group, were of particular interest to me. In a paper entitled 'Psychological coding' D. Bruce gave a lucid account, supported by absorbing evidence, of the process of reduction and elaboration that seems to occur as people try to recall a passage, while H. Giles drew on some theories from social psychology - similarity-attraction, social exchange, causal attribution and group distinctiveness - to explain speech diversity.

At the AGM an increase in membership to 375 was reported and the decision taken to support the publication of the proceedings of the 1975 Seminar on 'Language problems of overseas students in higher education'. The new Chairman is Dr. A. Davies (Edinburgh).

(W Grauberg)

BAAL Seminar on Translation, Exeter, 13-14 September 1976.

This seminar on the theory and practice of translation attracted 63 participants from a number of countries. Translation theory, grammatical and stylistic comparison, translation in language teaching and lexicography were the main subjects discussed. Copies of abstracts and a booklist are available from the organisers, Mr. W.S. Dodd or Dr. R.R.K. Hartmann, The Language Centre, University of Exeter, EX4 4QH.

(W Grauberg)

ON THE NON-DISCRETE NATURE OF THE
VERB-AUXILIARY DISTINCTION IN ENGLISH

Recent work by (e.g.) Ross (1972) and Comrie (1975) has called into question the traditional assumption that syntactic categories in natural language are discretely characterisable. In this paper, we shall present further evidence in support of this claim, by arguing that the categorial distinction between verb and auxiliary in English is not a discrete one.

The general principle for category assignment in a rule-governed grammar is as follows:

- (1) A lexical item L is a member of a category C just in case L undergoes a specified set of rules, R.

The auxiliary-verb distinction in English has traditionally been based on the assumption that verbs and auxiliaries behave differently in respect of (at least) the following rules in English. Firstly, auxiliaries undergo NEGATIVE CLITICISATION (by which not cliticises onto the auxiliary), whereas verbs do not: cf. e.g.

- (2) (a) He might not (mightn't) understand
(b) He seems not (*seemsn't) to understand (cf. note 1)

Secondly, verbs require do in negatives, interrogatives, emphatics and tags: cf. e.g.

- (3) (a) He doesn't want to go there
(b) Does he want to go there?
(c) He does want to go there
(d) Harry wants to go there, Harry does
(e) Harry wants to go there, does he?
(f) Harry wants to go there, doesn't he?

whereas auxiliaries never take do in these constructions: cf.

- (4) (a) He will not (*doesn't will) go there
(b) Will he (*does he will) go there?
(c) He will (*does will) go there
(d) Harry will go there, Harry will (*does)
(e) Harry will go there, will he? (*does he?)
(f) Harry will go there, won't he? (*doesn't he?)

Thirdly, verbs permit NOMINALISATION, but auxiliaries never do: cf.

- (5) (a) His wanting to go there was predictable
(b) *His willing to go there was predictable

Fourthly, verbs undergo SUBJECT RAISING, EQUI, etc. - or more generally, occur in untensed clauses - whereas auxiliaries do not:

- (6) (a) I'd expect him to want to go home
(b) *I'd expect him to will go home

Fifthly, verbs undergo TO INSERTION (i.e. take to before an infinitive), whereas auxiliaries do not: (cf. note 2)

- (7) (a) He wants *(to) leave
(b) He will (*to) leave

And sixthly, verbs undergo CONCORD in the present tense, whereas auxiliaries do not:

- (8) (a) He wants (*want) to do it
(b) He can (*cans) do it

The traditional auxiliary-verb distinction is based on criteria such as those above (cf. note 3). Now, if syntactic categories in natural language are discrete, then we should expect predicates to pattern in all six respects either like a typical verb, or like a typical auxiliary (or, possibly, both). However, as we shall see, this is by no means the case: the 'ideal' verb and the 'ideal' auxiliary represent two extremes of a continuum, between which lie a perplexing variety of semi-auxiliaries - i.e. predicates which pattern in some respects like auxiliaries, in others like verbs.

At one extreme, we find a set of predicates which pattern in every respect like typical auxiliaries - i.e. they undergo NEGATIVE CLITICISATION:

- (9) I won't/wouldn't/can't/couldn't/shan't/shouldn't/??mayn't/mightn't/mustn't arrive early (cf. note 4)

they never take do: cf.

- (10) Will/would/can/could/shall/should/may/might/must I do it on my own?

they don't permit NOMINALISATION:

- (11) *My willing/woulding/canning/coulding/shalling/shoulding/maying/mighting/musting/ arrive early surprised them

they don't occur in untensed clauses:

- (12) *He'd expect me to will/would/can/could/shall/should/may/might/must go there

they don't allow to:

- (13) I will/would/can/could/shall/should/may/might/must (*to) arrive on time

and they don't undergo CONCORD

- (14) *He wills/woulds/cans/coulds/shalls/shoulds/mays/mights/musts do it

though in the case of forms such as would, could, should, and might this is clearly a consequence of the fact that they are morphologically past tense forms: hence we should not expect them to show concord markings, so that the CONCORD criterion in this instance is simply inconclusive.

A predicate which patterns like a typical auxiliary in all respects save one is the 'auxiliary' do (though, of course, occurrence with do here is not applicable as a heuristic for categorising do, for obvious reasons): for example, it behaves like a typical auxiliary in permitting NEGATIVE CLITICISATION

(15) I don't understand you

and in not permitting NOMINALISATION or TO INSERTION, and not occurring in untensed clauses: cf. e.g.

- (16) (a) *His doing not turn up annoyed me (cf. The fact that he did not turn up annoyed me)
 (b) *He didn't to turn up
 (c) *I want him to do not turn up

But on the other hand, do patterns like a verb in undergoing CONCORD:

(17) He really does (*do) like her

Another difficult case is ought. On the one hand, it patterns like a typical auxiliary in permitting NEGATIVE CLITICISATION: cf.

(18) He oughtn't to say anything

and in not occurring in nominals or untensed clauses (cf. note 5)

- (19) (a) *My oughting to go there irritated me
 (b) *It would be a nuisance for me to ought to go there

But on the other hand, it patterns like a typical verb in respect of requiring TO INSERTION:

(20) He ought *(to) apologise

(the CONCORD criterion is inapplicable since ought is morphologically past). An intermediate case is its behaviour in respect of do-support; most speakers permit ought to function as an auxiliary and (e.g.) form negatives without do:

(21) He oughtn't to misbehave in front of Aunt Agatha

But there are also a subset of speakers who treat ought as a verb, and use do:

- (22) %He didn't ought to say that (% indicates only some speakers accept this form)

Used is another awkward intermediate case: on the one hand it patterns like an auxiliary in not permitting NOMINALISATION: cf.

(23) *His using/used to go there surprised me

But on the other hand, it behaves like a typical verb in respect of requiring TO INSERTION

(24) He used *(to) go there on his own

In other respects, however, used is more difficult to categorise: for example, NEGATIVE CLITICISATION with used is marginal for me, though some speakers accept it:

(25) %I use(d)n't to see her as often

Likewise, there are some speakers who permit used in untensed clauses such as:

- (26) (a) %Every morning, he would use(d) to go and get a paper
 (b) %He hadn't used to be so awkward
 (c) %He tended not to used to go there in the daytime

A clearer case is the behaviour of used in do structures: for most speakers used can pattern either as a verb (with do) or as an auxiliary (without do) in this respect:

- (27) (a) Did he use(d) to get up early?
 (b) Used he to get up early?

- though (27)(b) is rather more formal than (27)(a). Naturally enough, the CONCORD criterion is inapplicable with used, given that used is morphologically a past tense form.

At the other end of the spectrum, we have predicates like get, which function in all respects (and in all uses) as a verb. For example, get never permits NEGATIVE CLITICISATION: cf.

(28) *He gotn't arrested/to be famous

always requires do-support:

(29) He didn't get arrested/to be famous

permits NOMINALISATION:

(30) His getting arrested/to be famous surprised me

occurs in untensed clauses

(31) He wants to get arrested/to be famous

takes to before an infinitive

(32) He got *(to) be famous

and undergoes CONCORD:

(33) He's the kind of person who gets (*get) arrested/to be famous

Thus, get patterns in all respects like a typical verb.

Hitherto, we have been looking at a class of predicates which either have a single function, or - if they have more than one function, as in the case of may used as both an epistemic and root modal - pattern in a unitary fashion irrespective of their function. Now, by contrast, we turn to look at a class of predicates with multiple functions, and multiple syntactic behaviour.

Have and be are a case in point: they are problematic, in that they have three distinct functions: i.e. as auxiliaries:

- (34) (a) He is waiting/respected
 (b) He has left

as modals:

(35) He is/has to leave tomorrow

and as verbs:

(36) (a) He is president
(b) He has a car

In all three functions, have and be pattern like typical auxiliaries in permitting NEGATIVE CLITICISATION: cf.

(37) (a) He isn't waiting/respected
(b) He hasn't left
(c) He isn't/hasn't to leave tomorrow
(d) He isn't president
(e) He hasn't any money

but conversely, pattern like typical verbs in undergoing CONCORD (cf. 37), and in taking to before an infinitive: cf.

(38) He is/has *(to) leave tomorrow

(though, of course, this criterion is inapplicable to have and be in their auxiliary and verbal functions, since trivially in such cases they do not take an infinitive). But in other respects, the behaviour of have and be is rather more fragmentary. For example, in respect of dc-support, be and auxiliary have function like typical auxiliaries in not permitting do:

(39) (a) Is he (*does he be) waiting/respected?
(b) Is he (*does he be) to leave tomorrow?
(c) Is he (*does he be) president?
(d) Has he (*does he have) left?

But modal and verbal have pattern either like auxiliaries, or like verbs:

(40) (a) Has he to leave/any money?
(b) Does he have to leave/any money?

By contrast, in respect of NOMINALISATION, only modal be patterns like a typical auxiliary in never occurring in nominals: cf.

(41) *His being to leave tomorrow has upset me

In all other uses, have and be pattern just like verbs in permitting NOMINALISATION: cf.

(42) (a) His being respected surprised me (cf. note 6)
(b) His being president annoyed me
(c) His having finished disconcerted me
(d) His having to leave astonished me
(e) His having a car came as no surprise

A similar pattern is found in respect of occurrence in untensed clauses: once again, only modal be patterns like an auxiliary in not occurring in untensed clauses:

(43) *I'd expect him to be to leave tomorrow

but in other uses, have and be occur freely in untensed clauses: cf.

- (44) (a) I never expected him to be waiting/respected/president
 (b) I never expected him to have finished/to leave/a car

No lesser problem is posed by the dual behaviour of need and dare. Once again, we can distinguish three different functions for need/dare - as auxiliaries:

- (45) (a) Need I have a shave?
 (b) Dare you use a transderivational constraint?

as modals:

- (46) (a) Do I need to do my homework?
 (b) Do you dare to challenge my authority?

and (in the case of need) as a verb:

- (47) Do I need a new car?

An idiosyncrasy of auxiliary need is that it is generally restricted to occurring in what Klima (1964) terms 'affective' contexts:
 contrast:

- (48) (a) *He need wash the car
 (b) I don't think he need wash the car
 (c) He always buys more than he need do
 (d) I doubt if he need give evidence in court
 (e) He only need make a statement

At first sight, need and dare wouldn't seem to pose much of a problem. On the one hand, in their modal or verbal functions, they exhibit the properties typical of 'pure' verbs: e.g. they block NEGATIVE CLITICISATION:

- (49) (a) *He needsn't (to buy) a new car
 (b) *He daresn't to criticise me

but they undergo DO SUPPORT:

- (50) (a) He doesn't need (to buy) a new car
 (b) He doesn't dare to criticise me

NOMINALISATION:

- (51) (a) His needing (to own) a car puzzled me
 (b) His daring to challenge my authority shocked me

TO INSERTION (cf. 48-51), and CONCORD:

- (52) (a) He needs (to have) a shave
 (b) He dares to challenge my authority!

as well as occurring in untensed clauses:

- (53) (a) I never expected him to need (to buy) a car
 (b) I never expected him to dare to challenge my authority

On the other hand, dare and need also pattern like typical auxiliaries in permitting NEGATIVE CLITICISATION:

(54) He daren't/needn't say anything

but in not taking DO-SUPPORT:

(55) Dare he/need he tell the truth?

not undergoing NOMINALISATION: cf.

(56) *His not daring/needng say anything surprised me

nor occurring in untensed clauses:

(57) *I never expected him to dare/need buy a car

and in occurring without to before an infinitive:

(58) I doubt if he dare/need say anything.

Thus, superficially, need and dare would seem to pose few problems, insofar as they pattern either like 'pure' verbs, or like 'pure' auxiliaries (with a consequent subtle difference of meaning).

However, the symmetry of the above picture is disturbed somewhat by the existence of an idiolect which permits dare and need when used as verbs to exhibit the auxiliary-like property of not undergoing TO INSERTION. For example, when need and dare are used with do (a characteristic of verbs), they can omit to (optionally) - a characteristic of auxiliaries: cf.

(59) (a) Do I need (to) go shopping?

(b) Do you dare (to) challenge my authority?

Likewise, when need and dare undergo CONCORD (and hence are used as verbs), they also permit to to be omitted (like auxiliaries):

(60) I don't think he dares/needs (to) come

What at first sight is puzzling, however, is the following contrast:

(61) (a) He dares (to) do almost anything

(b) He needs *(to) buy a new car

i.e. in the (b) example to cannot be omitted, but in the (a) example it can. This apparently surprising fact would seem to be not unrelated to the fact that in (61)(a) the verbal form dares can be replaced by the corresponding auxiliary form dare: cf.

(62) He dare do almost anything

whereas in (61)(b) needs cannot be replaced by the auxiliary form need (since the latter is restricted to occurring in 'affective' contexts): cf.

(63) *He need buy a new car

What I am suggesting is a transderivational condition on the omission of to with need and dare used as verbs to the effect that to can only be omitted just in case the verbal form of need/dare is replaceable by the corresponding auxiliary form, salva grammaticitate. More simply, I would suggest that omission of to when

need/dare are used as verbs is by analogy with the corresponding auxiliary form (which, of course, would not require to). Support for this transderivational condition appears to come from data such as the following: notice that to can generally be omitted with the verbal form in those cases where the auxiliary form is acceptable:

- (64) (a) He's the kind of person who dare(s) do anything
 (b) I don't think he need(s) come
 (c) He dare(s) not contradict me
 (d) He eats more than he need(s) do
 (e) I doubt if he need(s)/dar[?](s) say anything
 (f) He only need(s) clean the stairs
 (g) Do I need come/Need I come?

but to cannot be omitted where the auxiliary form is unacceptable:

- (65) (a) *I think he need(s) have his haircut
 (b) *I've no doubt that he need(s) buy a new car

Thus it would seem that need/dare pattern either like auxiliaries, or like verbs, with the exception that when used as verbs, for some speakers they permit to to be omitted just in case the corresponding auxiliary form is acceptable.

We can summarise the behaviour of the various predicates we have considered in the table on p. 16.

As the reader can verify for himself, (66) presents us with a near perfect squish, in which there are very few ill-behaved cells. Indeed, there would seem to be only two respects in which (66) departs from an ideal squish: firstly, the behaviour of may and used in tending to resist NEGATIVE CLITICISATION; and secondly, the behaviour of used and ought in allowing DO SUPPORT. But in all other respects, the behaviour of the predicates concerned with the rules concerned is entirely regular: bearing in mind that we are dealing with 23 predicates and 7 rules (i.e. a total of 161 cells), the aberrant behaviour of four cells is indeed a trifling complication.

At the beginning of our paper, we asked whether or not the categories verb and auxiliary are discretely characterisable in English: we have seen that although some predicates (e.g. will, would, can, could, shall, should, must, might, possibly may) pattern like typical auxiliaries in all respects, and others pattern like typical verbs (e.g. want, get, etc.), and yet a third class (need, dare) pattern either like verbs, or like auxiliaries (all facts which could be handled without any complication in a discrete grammar), there are also a class of semi-auxiliaries which pose a serious problem for a theory of discrete grammar. These include do, have and be in all their functions, used, and ought: they are problematic insofar as they neither undergo all the rules which typical verbs undergo, nor behave like typical auxiliaries in all respects. Of course, within a theoretical framework which permits unlimited use of arbitrary rule-features, the problem is solved simply by marking each predicate with a positive or negative value in respect of some set of features F_1, \dots, F_n corresponding to the number of rules R_1, \dots, R_n involved. But such a solution implies that the behaviour of semi-auxiliaries is entirely idiosyncratic and unpredictable, whereas on the contrary, we have seen that they

| (66) | NEGATIVE CLITICISATION | DO SUPPORT | NOMINALS | UNTENSED CLAUSES | TO WITH INFINITIVE | CONCORD |
|--|---------------------------|-----------------------------|---------------------------|----------------------------------|---------------------------|--|
| will, would can, could shall, should may, might must need (auxiliary) dare (auxiliary) | A (?? <u>may</u>) | A | A | A | A | A (not applicable to <u>past</u> forms) XXXXXXXXXXXXXXXXXXXX |
| do | A | n.a. | A | A | A XXXXXXXXXXXXXXXXXXXX | X V |
| be (modal) | A | A | A | A | X V X | V |
| ought | A | A/%V | A | A | X V X | n.a. |
| used | ??A | A/V | A XXXXXXXXXXXXXXXXXXXX | A A XXX XXX%V XXXXXXXXXXXX | V | n.a. |
| be (auxiliary) be (verb) have (auxiliary) | A | A | V XX XX XX | V | n.a. | V |
| have (verb) have (modal) | A XXXXXXXXXXXXXXXXXXXX | A A XXX XXX XXX XXX V | V | V | n.a. V | V |
| get need (verb) dare (verb) | V | V | V | V | V | V |

A represents 'patterns like an auxiliary in this respect'

V represents 'patterns like a verb in respect of...'

n.a. means 'not applicable'

(The XXXXX-line plots the lower limit of auxiliary-like behaviour)

behave in entirely systematic ways: some (e.g. do) are more AUX-like in their behaviour, while others (e.g. have) are more verb-like in their behaviour. Similarly, some rules (e.g. CONCORD) apply to predicates which manifest even weak verbal properties, whereas others (e.g. NOMINALISATION) are restricted to applying to predicates which have relatively strong verbal characteristics. In short, semi-auxiliaries behave systematically on the basis of the interaction of two parameters: (i) the degree of 'verbiness' of the trigger; and (ii) the degree of 'verb-neediness' of the rule concerned. Thus, a mass of apparently idiosyncratic, exceptional data reduces to the systematic interaction of two parameters which are not themselves discretely characterisable (i.e. which don't simply have the values 0 or 1, but which may have some value lying in between these extremes).

Footnotes

note 1 One peculiarity of NEGATIVE CLITICISATION should perhaps be mentioned: namely, that not cannot cliticise onto a clitic auxiliary: cf. e.g.

- (i) (a) He has not left
- (b) He's not left
- (c) He hasn't left
- (d) *He'sn't left

This appears to be a somewhat idiosyncratic restriction, given that there is no general constraint against multiple clitic forms in English:

- (ii) (a) He'd've gone there, if I'd asked him to
- (b) He'll've finished it by now

note 2 It might be objected that there are a number of verbs in English which take an infinitive without to-cf. e.g.

- (i) I heard him remark that syntax was relational
- (ii) I saw Harry use a transderivational constraint last week
- (iii) They made Max change his name to Melvin

Hence, we might claim that the to-criterion is irrelevant. However, notice that such verbs generally have a 'latent' to which shows up in the passive:

- (iv) He was heard to remark that syntax was relational
- (v) Justice must be seen to be done
- (vi) Max was made to change his name to Melvin

note 3 The observant reader will notice that I have not included CLITICISATION TO SUBJECT as a defining property of auxiliaries: i.e. the ability to cliticise onto a subject. There are a number of reasons for this decision. Firstly, there are only four predicates (will, would, have and be) which permit CLITICISATION TO SUBJECT: cf. e.g.

- (i) (a) He'll be late
- (b) He'd take a long time
- (c) He's working
- (d) They've finished

Hence, a criterion such as the above would exclude can, could, shall, should, may, might, must, need, dare, do and get from the category 'auxiliary' - in spite of the fact that they all pattern like auxiliaries in other ways. Secondly, even when have and be are used as verbs, they still permit CLITICISATION TO SUBJECT

- (ii) (a) He's president
(b) They've no money

whereas if CLITICISATION TO SUBJECT were a criterial property of auxiliaries, we should expect that have and be would only undergo the rule when used as auxiliaries. Thirdly, by no means all forms of the predicates concerned permit CLITICISATION TO SUBJECT: for example, although have does so in both present and past tenses:

- (iii)(a) He has (he's) left
(b) He had (he'd) left

be does so only in the present tense:

- (iv) (a) I am (I'm) working
(b) I was (*I's) working

It is also interesting to note that there are a number of other restrictions on CLITICISATION TO SUBJECT: for me, this is possible with have and be used in the negative:

- (v) (a) I'm not working
(b) They've not finished

except when have is used as a verb:

- (vi) *They've not any money

but not with will or would:

- (vii)*He'll not leave/*He'd not leave

(unless, marginally, the scope of not ranges uniquely over the subordinate clause). Notice also that have can cliticise onto another clitic auxiliary: cf.

- (viii)(a) He'll've been arrested
(b) He'd've been arrested

but (for me, at any rate) only when used as an auxiliary, not as a modal or verb: also, I'm not nappy about have cliticising to a clitic auxiliary in the negative:

- (ix) ??* He'd've not been arrested

note 4 We might hope to account for the unnaturalness of forms such as ??mayn't and usedn't by arguing that the scope of the negative in such cases ranges solely over the subordinate clause: hence if not belongs to the subordinate clause, we might expect cliticisation to be blocked. But this won't work, given forms such as:

- (i) He mightn't leave

where we have the same scope relation, and yet NEGATIVE CLITICISATION is permitted.

note 5 Notice that the failure of ought to occur in nominals or untensed clauses cannot be attributed to the fact that it is a past tense form, for past tense forms such as had occur in both: cf.

- (i) (a) After he had (after having) eaten his supper, he went to bed
 (b) The fact that he had (his having) gone made worried me

note 6 The ungrammaticality of forms such as:

- (i) *His being working surprised me

is of no consequence, in that it is attributable to violation of the DOUBL-ING CONSTRAINT discussed in Ross, 1972b, and Milsark (1972).

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ON THE NONEXISTENCE OF THE
VERB-AUXILIARY DISTINCTION IN ENGLISH

Andrew Radford (1976) has claimed that the facts of the English auxiliary system lend support to the proposals of Ross and others that nondiscrete categories, and along with them the notion 'degree of membership in a category', should be admitted into linguistic theory. His paper is interesting in that it amasses data which can be analyzed in terms of a matrix or "squish", his (66), which is the most well-behaved squish that has appeared in the literature to my knowledge. The peculiar thing about Ross's research on squishes so far has been that he has never been able to exhibit any phenomenon describable in terms of a truly well-behaved squish, one in which there really is a smooth gradient from A to B, with unblemished A-ness at the top left, unsullied B-ness at the bottom right, and no "ill-behaved" cells of B-like qualities smack in the middle of the A's or vice versa. On page 319 of Ross (1972), for instance, a tiny "subsquish" is given with just twenty-five cells, and two of them are ringed as counterexamples to the claim being made - crosses among the ticks, indicating inexplicable ungrammaticalities where the gradient properties that are claimed to exist would predict grammaticality. Ross has never done better than this, and has regularly done far worse, in providing validated instances of regular gradience in grammatical properties like verbiness or nouniness or clausiness. He has been forced to coin the term "squishoid" for an only partially squish-like squish. Squishoids resemble little more than random assemblages of ticks and crosses on a matrix labelled with constructions and lexical items, and have never to my knowledge contributed to the solution of any problem or the obtaining of any insight into grammatical phenomena.

Radford's verbiness/auxiliariness squish is one of the best instances of discovering what Ross claimed would be found. His very useful and thorough compilation of data provides the basis for something very close to a regular gradience between an item like must with totally auxiliary-like behaviour (permits n't, never permits do, has no nominalizations, cannot occur in a tenseless clause, does not govern a following to on an infinitive, and shows no number agreement) and an item like get with the converse properties (cf. *getn't, doesn't get, getting, to get, gets). Right in the middle, as predicted by the gradience claim, items can be found with only about 50 per cent of the auxiliary-like properties, otherwise being verby. An example is used. We find usedn't (for some speakers) but also didn't use to (for some speakers), there are no nominalizations, occurrence in untensed clauses is marginal or nonexistent, but a following infinitive takes to just as after a verb like want. More verby still is be, which allows n't (isn't) and never cooccurs with do in its auxiliary uses (e.g. *She doesn't be writing an historical novel) but occurs in nominalized and tenseless clauses and has a full number agreement paradigm.

However, I claim that none of this material makes the "squishy categories" notion even remotely plausible, and that here, as probably in all other areas of syntax, squishes are a complete red herring, without descriptive or theoretical value of any kind. The conclusion to which Radford's data really lend support is not that

there is some kind of "nondiscrete distinction" between verbs and auxiliaries in English but that there is no distinction, and no category of auxiliary in the grammar at all.

The correct view of auxiliaries is, ironically, one that is originally due to Ross himself. It is that all auxiliaries, including be, have, and all the modals, are simply main verbs of separate clauses in deep structure (henceforth, initial structure, a more neutral term). The alleged peculiarities of those verbs that have been known as auxiliaries are not in fact so peculiar when examined more closely, and can be (must be) accommodated quite appropriately as lexical irregularities or susceptibility to certain minor rules of the syntax.

It is impossible to present here in detail the arguments that support this view, but in Pullum and Wilson (forthcoming) this is done. It is argued there that the claim that all auxiliaries are main verbs in initial structure is one which is quite independent of the claims regarding "generative semantics" with which it has become associated. Even if syntax is to be strictly autonomous, and semantic interpretation is to be carried out at surface structure, the arguments for the main verbs analysis are compelling. Since generative semanticists already find it an eminently desirable view (cf. McCawley 1971 and many other works), the main verbs treatment of auxiliaries should now be recognized as fully established. In order to underline this point, and to flesh out the particular claims of the analysis, Pullum and Wilson present in detail the statements of the base rules, transformations, and lexical classifications that would be part of an autonomous syntax for English that incorporated the main verbs analysis. The main difference noticeable in the new formulations is that they represent a spectacular simplification over the familiar Syntactic Structures system of rules with its celebrated base rule Aux \rightarrow Tense (Modal) (have-en) (be-ing). The latter rule is not now needed in the grammar at all, and nor is any analogue of it.

The main stumbling block in the way of recognizing that all auxiliaries are verbs has generally been the modals. While it is, and always has been, almost embarrassing to have to claim that the have of He has gone, the be of He is coming, and the do of He doesn't like us are different from the verbs of identical shape that appear in He has a Jensen, He is a D.Litt., and He does linguistics, defenders of the Syntactic Structures analysis have always felt a little more secure and confident, it seems, about giving the modals a node label distinct from V (Verb). (Notice how in Emonds 1976 it is conceded that be, have, and do are labelled V in all their occurrences, but modals still are not.) In fact, however, it is Radford's own evidence that is the best indicator of how the syntactic analysis of English must deal with these items, as I shall now briefly show.

Let us take must as our example, for intuitively there is no less verby verb in English. (I agree entirely that this informal intuition exists: I am only claiming that it is like the feeling that the slow worm isn't a lizard or the whale isn't a mammal. Slow worms are lizards, albeit legless; whales are mammals; and modals are verbs.)

The point I want to make is simply that there is no criterion that might be used to identify must as a modal, because (and this is Radford's point as it should be reinterpreted) no grounds can be

found on which a class of modals might be identified. Must allows negative cliticization (mustn't), agreed. But that is neither necessary to establish modalhood (may is alleged to be a modal even for those dialects, e.g. mine, where *mayn't doesn't exist) nor sufficient (have and be take n't but are never called modals). Must never tolerates supportive do, but then nor do be or have. Must does not take to on a following infinitive, but again this is not necessary for modalhood (ought to, used to, and is to are fairly clearly modals as judged by their distribution but always take to, while the modals need and dare have main verb alloforms which take it), and neither is it sufficient (verbs like make, let, see, hear, etc. have the same property: You made me love you). Must fails to exhibit the -s of the third person singular present indicative, true; but is to (as in He is to leave immediately, synonymous with and distributionally similar to He must leave immediately) is a modal with a full agreement paradigm, so this cannot be a necessary property of modals. As to whether we should allow that it is possible for a verb to lack the -s affix in its paradigm, I cannot see how we could make a case against it. We know that -s when used as the regular plural affix in English takes zero shape with a subclass of nouns (sheep etc.), and we would never use this to deny the nounhood of sheep. And in both Old English and Modern German the paradigm irregularity that makes the 3sg and 1sg forms of the present indicative homonymous not only appears with a subclass of (undeniable) verbs, but appears on exactly that subclass of verbs that includes the cognates of must, shall, will, etc. The paradigm of must, then, can hardly be a total surprise typologically.

We are left with the one important property of the modals that really does pick them out as a special subclass, the one that keeps them out of untensed clauses and nominalizations, and I now suggest what I think is the correct account of that. Consider must, and note that if it is a verb, it is a peculiarly irregular one, for it lacks the past tense altogether (*He must/musted leave yesterday). It is therefore an irregular, in fact a defective, verb. The lexicon must contain a straightforward list of its inflectional forms, with blanks where the missing past tense forms would go. Now, the vital thing about the paradigms of the modals, all of which are to some degree irregular and thus listed anyway, is that they lack completely the infinitive, the past participle, and the -ing form. As is shown explicitly in Pullum and Wilson (forthcoming), this simple morphological fact is (as McCawley 1971 had surmised) all that is needed to explain the nonexistence of *He will must go, *He expects to must go, *His musting go disappointed me, and so on. The modals are those verbs which have blank spaces against the entries for infinitive, -en form, and -ing form in their lexically entered paradigm lists.

The broader class of so-called auxiliary verbs is also definable by reference to the structure of the lexicon. Hudson (forthcoming) and Zwicky (1976) have independently argued that contracted negative forms show such irregularity in syntax, morphology, and phonology that they must be lexically entered as units (won't, shan't, etc.). Those verbs which have such additional entries are the so-called auxiliary verbs, and will undergo Subject-Auxiliary Inversion in yes-no questions. (Even may will be on the list, for may has a past tense form might, and although the *mayn't slot is blank in the lexicons of many speakers, the parallel mightn't slot is present.)

When a detailed syntax for the auxiliary system of English is undertaken, therefore, no role for or evidence of squishes or "nondiscrete categories" is found, and no categories for modals or other auxiliary elements emerge, apart from V. The rich and complex data that Radford has rightly been reexamining provides a salutary lesson in the importance of making specific proposals for lexical and syntactic description rather than tabulating superficial diagnostic characteristics if theoretical headway is to be made.

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POSTSCRIPT ON SQUISHES

Geoff Pullum (1976) attacks my (1976) paper on auxiliaries in swashbuckling style, concluding that the squish is not a valid theoretical construct. While I agree that squishes do not in themselves lend support to nondiscrete categories in syntax (clearly a severe weakening of linguistic theory), I think that nonetheless the squish has a useful role to play in capturing fundamental generalisations. For basically a squish is simply a matrix of conditional dependency relations, and thus to deny the existence of squishes is to deny the existence of complex dependency relations of the type that I illustrated in my paper - a claim which would be tantamount to abandoning the search for regularity in syntax. More concretely, the squish I present in my paper is probably best interpreted as supporting the existence of the following dependencies:

(1) -CONCORD > -TO > -UNTENSED > -NOMINAL > -DO > +NEG CLIT

where (1) is interpreted informally as follows: ceteris paribus (i.e. allowing for exceptions) then if a verb doesn't undergo CONCORD, then it won't take to before an infinitive, won't occur in untensed clauses, won't take a nominal, won't take do-support, but will take NEGATIVE CLITICISATION. On the other hand, a verb which does take CONCORD, but doesn't take to, won't occur in untensed clauses, or nominals, and so forth. I leave it to the reader to verify for himself that the implicational statement (1) captures significant generalisations which would elude the arbitrary use of ad hoc lexical features or the equivalent ploy of positing a class of "defective verbs".

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SQUISH: A FINAL SQUASH

Radford's postscript asserts that "significant generalizations" are embodied in the formula "-CONCORD -TO -UNTENSED -NOMINAL -DO +NEG CLIT". This piece of pseudo-formalism is supposed to express a series of "conditional dependency relations" involving lack of verbal agreement affixes, lack of the to marker on infinitival complements, failure to appear in tenseless clauses, lack of nominalized forms, failure to co-occur with supportive do, and ability to take the cliticized negative n't.

It is thus claimed that if a verb does not take verbal agreement (-s in the 3sg. present indicative) it will not take to on a following infinitive. A counterexample to this is ought, to which Radford claims verbal agreement is "not applicable"; the latter claim is illegitimate, for ought is not synchronically a past tense form like used, but a present tense modal verb like must. It is also claimed that if a verb does not govern to on an infinitival complement it will not appear in tenseless contexts. Counterexamples to this are make, let, see, hear, etc. It is further claimed that if a verb cannot appear in tenseless contexts it will not have nominalized forms. This is a tautology, since nominalizations are tenseless. Next, it is claimed that if a verb lacks nominalizations it will not permit supportive do. Counterexamples to this are ought and used for those who say They didn't ought to and the like. Finally, it is claimed that if a verb fails to occur with supportive do it will permit negative cliticization. May, am, and used are all counterexamples to this for me, since I do not have *mayn't, *amn't, or *usedn't.

In sum, not only does Radford's statement of "conditional dependency relations" between unrelated surface features of English grammar continue to be an entirely pointless excrescence in theoretical terms, it is also a tissue of empirical falsehoods. Applied to my dialect it makes not a single exceptionlessly correct claim about anything. It can hardly contribute, therefore, to the quest for "regularity in syntax". If the alternative has to be that of accepting that defective verbs are to be analyzed as defective verbs (which Radford describes as a "ploy"), then that would seem to be the alternative we must embrace. It ill befits a scholar of Dr. Radford's standing to descend to vulgar abuse in the effort to avoid facing up to this patent fact, and one is saddened to see a member of his renowned institution of learning demeaning himself thus.

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THE 'LOCUTION PREPOSITIVE' AS A STYLISTIC MARKER
IN MODERN FRENCH

Computerised lexical studies have achieved considerable significance during the past decade, producing powerful techniques of analysis and often suggesting novel lines of approach to syntactic questions. We can for example explore the combinatorial factors which lie deep within the identity of a language, or construct sentence stereotypes as a basis for stylistic comparisons. Beyond this there are two trends which should perhaps be fully recognised. The first concerns the close attention which is being paid to the performance of hapax, various types of stylistic marker, and the operation of function words. Enkvist (1973) makes the point that, whilst the statistician may seek to discover linguistic universals, using very large textual samples - 'even to all the texts in languages', students of style have been more interested in establishing linguistic differentials. Consequently small in-depth studies may be profitable providing that we realise the complexity and range of feature combinations which can have stylistic importance. The second trend reflects an interest in word groups and the combinatorial rules which produce them. This is evident particularly in matters of terminology, and it is noteworthy that Phal, in introducing the VGOS (1971), pays some attention to the function of word groups in scientific texts, both as neologisms and as logical operators. The system of selection of items in the Vocabulaire général d'orientation scientifique is, as in many surveys, statistical and functional: some are included despite low frequency in the corpus, and others are omitted because morphosyntactically they are extensions of other items. Consequently, since Phal, and grammarians such as Le Bidois before him, consider locutions prépositives as merely extensions of adverbial expressions, a major area of lexico-syntactic importance has become overlooked. Its performance and distribution serve as stylistic markers, since its semantic component reflects a search for precision on the part of the author. In didactic texts it is a frequent feature, whilst in spoken French occurrence is restricted and the range of types much smaller.

In this study three aspects are considered:

Formal characteristics
Syntactic performance
Register distribution

Formal characteristics

Dubois (1969) emphasises a major division: locutions which contain a substantive element and those which do not. In addition we note a further division between locutions which contain a definite article and those which do not. Hence the following schema:

| TYPE A | TYPE B | |
|--------------------|-----------------------------|------------------|
| No substantive | (1) Substantive present (2) | |
| | Without article | With article |
| Jusqu'à | en face de | aux alentours de |
| D'entre | par rapport à | au lieu de |
| Lors de | grâce à | le long de |
| Quant à | | |
| En ce qui concerne | | |
| A même | | |

In general we find that there is a positive correlation between high frequency and type A, low frequency and type B2. The examples given above show too that Phal is right to make a general statement about the regularity with which the final preposition - préposition de sortie and the initial preposition - préposition d'entrée, are restricted to a small set: A, DE, EN, PAR, SOUS.

The format of locutions is strongly affected by ellipsis, and as Frei (1929) indicated omission of prepositions occurs commonly in modern French. In the Grammaire des fautes he cites

C'est en face la Sorbonne
Rapport à ces nuages-là, il va pleuvoir
Cause depart
Près le pont.

One notes that L.C. Harmer comments extensively on this process when doublets appear:

| | |
|---------------|-----------|
| Par manque de | MANQUE DE |
| Du côté de | COTE |
| A la fin de | FIN |
| A ras de | RAS DE |

The French Language Today, pp.138-9 (1954)

One continues to note further examples of this process in modern French both in spoken and written texts. The prepositional function of the elliptical form is by no means reduced:

En face de l'église EN FACE l'église FACE l'église.

The relationship between adverb and prepositional word group is not so simple as that described by Phal, Dubois and Pinchon. Several types of transformation exist:

| Type | Locution prepositive | Adverb |
|------|----------------------|-----------------------|
| 1 | Autour de | Autour |
| 2 | hors | Dehors |
| 3 | Jusque dans | Jusque-là |
| 4 | Jusqu'à | --- |
| 5 | Grâce à | --- |
| 6 | A l'intention de | a cette/mon intention |
| 7 | Dans le domaine de | dans ce domaine. |

Clearly there is semantic restriction imposed as substantive elements appear to retain nominal function.

Syntactic performance

Four categories are observable:

- (A) Locutions which introduce a substantive but not an infinite verb
e.g. Au milieu de, en forme de.
- (B) Locutions which introduce infinitives
e.g. Afin de, en train de
- (C) Locutions which introduce substantives without the article.
e.g. A titre de, en cas de
- (D) Locutions which can introduce any part of speech.
e.g. Quant à, près de

One notes there are three syntagmatic functions performed by the locutions:

Connection of two substantives or substantive phrases:

Un écart par rapport à la norme
Mon père en présence de ma mère

Connection of verb phrase and substantive or substantive phrase:

Je les ai réalisés à l'aide de bandes adhésives

Formation of adverbial expressions, particularly heading a sentence:

De par le Roi
En fin de volume.

These last illustrate a process of lexicalisation which Phal tries to investigate both in the VGOS and elsewhere. Tests of mobility and separability reveal the full cohesion of such expressions. It can be seen again in

En fin de compte
Hors de combat etc.

Register distribution

In modern French there is strong tendency for expository and documentary texts to contain large numbers of locutions instead of simple prepositions, possibly because the majority carry greater precision. In a small survey of 460,000 words of material appearing since 1950 results were obtained which indicate that the locutions are register sensitive.

The corpus was composed of four ranges of texts:

- R1 Spoken language transcripts.
- R2 Creative writing.
- R3 Documentary material, journals.
- R4 Administrative memoranda.

Only 93 types of locution appeared in R1, whilst in R2 there were 177, in R3 221, and in R4 119. In R4 though the types are fewer, there is high frequency of occurrence and the result is a style in which certain phrases are repeated ad nauseam. Types found only in R3 and R4 are:

| | |
|---------------|------------------|
| D'ICI | PAR SUITE DE |
| A COMPTER DE | A L'EXCEPTION DE |
| RELATIF A | AU MOYEN DE |
| EN ETAT DE | DE L'ORDRE DE |
| EN QUALITE DE | EN L'ABSENCE DE. |
| EN VERTU DE | |
| EN VOIE DE | |

By considering the frequencies of occurrence it is possible to suggest there are significant differences between the rate of incidence in spoken language and that in written texts.

| Range | Texts | Running words | Locution average |
|-------|-------|---------------|------------------|
| 1 | 10 | 98,000 | 1 : 450 |
| 2 | 14 | 130,000 | 1 : 220 |
| 3 | 27 | 177,000 | 1 : 180 |
| 4 | 10 | 48,000 | 1 : 130 |

It is accepted that inadequate statistics can lead us into major errors but the following examples were typical of the results of many of the high frequency items. Taking three locutions which were synonymous the register distribution appeared as follows:

| | R1 | R2 | R3 | R4 |
|--------------------|----|----|----|----|
| Pres de | 4 | 40 | 17 | 3 |
| A proximité de | - | 2 | 5 | 2 |
| Aux abords de | - | - | 1 | - |
| or Quant à | 1 | 17 | 23 | 4 |
| Par rapport à | 1 | 3 | 29 | 6 |
| En ce qui concerne | 1 | 2 | 16 | 11 |

Locutions prépositives are a well established category in French grammar. Some have tried to treat them as a closed set having the same function as a preposition. In fact, it is better to investigate them as an open set of lexico-syntactic interest in which the processes of lexicalisation can be seen at several levels. Perhaps this is where the really big computer corpus can help us, in revealing the diachronic pattern in the development of these word groups.

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CONCEPTS OF VERBAL DEFICIT IN BERNSTEIN'S WRITINGS
ON LANGUAGE AND SOCIAL CLASS

NB.

- (i) Except where otherwise indicated, all page references are to Basil Bernstein, Class, Codes and Control, Volume 1: Theoretical Studies towards a Sociology of Language, 2nd., revised ed., Routledge and Kegan Paul: London 1974.
- (ii) All references to papers by year only or by year and letter only are to papers by Bernstein. For full details see References.

1. Introduction

In (1969), (1971a) and (1973) Bernstein seeks to dissociate his work from the concept of linguistic deprivation or verbal deficit. However, when discussing the concepts of linguistic deprivation, cultural deprivation and compensatory education in (1969) he also says:

'I have taken so much space discussing the new educational concepts and categories because, in a small way, the work I have been doing has inadvertently contributed towards their formulation.' (p.194)

In other words, some people have interpreted Bernstein's theory as a theory of linguistic deprivation or verbal deficit. Much the same point is made in (1971b) where Bernstein notes that

'Both left wing and right wing were convinced that the basic model was that of deficit.' (p.19)

Assertion and counter-assertion can do nothing to answer the question at issue - namely, whether Bernstein's writings entail a concept of verbal deficit: the question can only be answered by examining his writings themselves. As I have argued in an earlier paper,¹ Bernstein does not provide a definitive statement of his theory and does not make it clear which of his papers represent the current version of his theory, and it will therefore be necessary to examine all the more important papers. It will be argued that the evidence suggests that his theory does, in fact, entail a concept of verbal deficit.

2. Definition of verbal deficit

The concept of verbal deficit is far from easy to define, partly because people who use it generally take it for granted and, more importantly, because the concept is utterly at variance with modern thinking in linguistics. However, for the purposes of this paper it is essential to provide a working definition; otherwise, it might be objected that the paper did indeed trace something in the work of Bernstein, but that

that something was not a concept of verbal deficit. Put at its simplest, the concept of verbal deficit assumes that some speech in some sense 'falls short of' or fails to 'match up to' a norm. This statement conceals a number of other assumptions which may be listed as follows:

- (1) The speech of a speaker A or a group of speakers X is compared either explicitly, or implicitly with a norm N. The comparison may relate to the speech of A or X either in a given context, or in contexts deemed comparable (e.g. the replies of five-year-old children to a teacher's open-ended question), or in a range of contexts, or generally. N may be either the speech of another speaker or group of speakers, or it may be a model in the mind of the investigator.
- (2) The comparison reveals differences between the speech of A and/or X on the one hand and N on the other. The differences may be in any one or more of the following areas: phonetics, phonology, morphology, syntax, lexis, semantics.
- (3) At least some of the differences are interpreted in terms of the alleged inadequacy of speech of A and/or X.
- (4) The alleged inadequacy is then interpreted. The interpretation may be anywhere on a continuum from a trivial observation on the performance of A or X in one specific context (e.g. 'John put it rather tactlessly') to sweeping statements with far-reaching implications about the linguistic competence of A or X (e.g. 'The lower working classes rely heavily on gestures and facial expression for communication' or 'much of lower class language consists of a kind of incidental "emotional" accompaniment to action here and now'²).

Since the interpretation may fall anywhere on the continuum there is no point in distinguishing between concepts of verbal deficit in performance and verbal deficit in competence, especially as the status of the competence/performance dichotomy is the subject of much controversy. However, since on the above definition, any negative value-judgement on the speech of an individual or group implies a concept of verbal deficit, it is important to distinguish relatively trivial notions of verbal deficit, near the weak end of the continuum, from more substantial concepts of verbal deficit. It is also important to draw a distinction between concepts of verbal deficit that are peripheral to a theory from those which are central.

3. The papers published in and before 1960

In these early papers Bernstein establishes two modes of language-use, which he calls public language and formal language. The middle classes and 'associative levels' have at their disposal both modes, while the working classes are restricted to public language. The contention that the working classes are limited linguistically in this way is central to Bernstein's hypothesis on the relationship between language and social class as expounded in his early papers. Bernstein (1959) states that:

2

The second of these statements is taken from A. Jensen, 'Social Class and Verbal Learning'. Cited in Labov (1969), p.4.

'... an individual may have at his disposal two linguistic usages, a public language and a formal language, or he may be limited to one, a public language, depending upon his social group.' (p.43)

A footnote immediately following this statement leaves the reader in no doubt as to which social group(s) is limited to public language:

'The largest of such groups is composed of individuals who come from 'unskilled and semi-skilled social strata.' (p.56)

Thus, by comparison with other sections of the population, the unskilled and semi-skilled strata - generally referred to by Bernstein as the working class - show a verbal deficit. The significance of this deficit is all the greater in view of the value-judgements expressed by Bernstein on public and formal language. The latter is explicit, grammatically 'accurate', a suitable vehicle for the expression of 'individual qualifications', its very form 'implies sets of advanced logical operations' (p.28), and it 'points to the possibilities inherent in a complex conceptual hierarchy for the organizing of experience' (p.55). Public language, on the other hand, is characterized by:

'short, grammatically simple, syntactically poor sentence construction; inappropriate verbal forms; simple and repetitive use of conjunctions; rigid and limited use of adjectives and adverbs; selection from a group of traditional phrases' (pp.46-47)

Moreover, in public language 'the very means of communication do not permit, and even discourage, individually differentiated cognitive and affective responses' (p.47) and:

'Because of a simple sentence construction, and the fact that a public language does not permit the use of conjunctions which serve as important logical distributors of meaning and sequence, a public language will be one in which logical modification and stress can be only crudely rendered linguistically ... Of equal importance, the reliance on a small group of conjunctions (and, so, then, because) often means that a wrong conjunction is used or an approximate term is constantly substituted for a more exact logical distinction. The approximate term will then become the equivalent of the appropriate logical distinction.' (p.44)

Further evidence of a concept of verbal deficit in Bernstein's early papers is provided by the experiments reported in Bernstein (1958) and (1960). In the first experiment a test of verbal intelligence (the Mill Hill Vocabulary Scale Form I Senior) and a test of non-verbal intelligence (Raven's Progressive Matrices, 1938) were administered to 309 GPO messenger boys. As Bernstein predicts, the results show a significant overall discrepancy between the results on the two tests. Whereas the means for the verbal test all fall within the range 94-104 IQ those for the non-verbal test cover a much wider range, namely 76-124 IQ. Thus for those subjects with

very low or very high scores on the non-verbal test (the 71-80 and 121-126 IQ groups) the discrepancy between the mean verbal and the mean non-verbal IQ scores was found to be substantial (18-20 points). In (1958) Bernstein predicted that if the same tests were administered to a group of middle-class boys matched for age the relationship between verbal and non-verbal IQ scores would be found to be significantly different.³ Bernstein (1960) reports the results of an experiment designed to explore this difference. In this experiment Bernstein uses a fresh sample of working-class subjects, consisting of 61 messenger boys, and a middle-class sample consisting of 45 public-school boys, both samples being matched for age. The tests used were the same as in the previous experiment. The results for the working-class sample are substantially in accord with those found in the earlier experiment, while those for the public-school sample show no comparable overall discrepancy between verbal and non-verbal IQ.

The results of these experiments are, of course, open to various interpretations. One interpretation might be that the verbal intelligence test failed to discriminate between the working-class boys and may therefore have been inappropriate; another might be that the verbal intelligence scores of working-class boys are much of a muchness. One might also ask: What is this thing glorying in the name of verbal intelligence, a thing that is apparently tested by the ability to define the meanings of isolated words? What is important, however, is Bernstein's own interpretation of the results. In his discussion of both experiments Bernstein is particularly interested in the data showing a big discrepancy between high non-verbal IQ and near-average verbal IQ among working-class boys. Before conducting the experiment reported in (1958) Bernstein predicted that:

'The higher the score on the matrices the greater the difference between the matrices and the Mill Hill scores.' (p.30)

And the experiment confirmed this prediction. The second experiment was preceded by the prediction that:

'for the working-class group ... the language scores would be severely depressed in relation to the scores in the higher ranges of a non-verbal measure of intelligence.' (p.62)

Again, the experiment confirmed the prediction. Bernstein does not question the validity of the tests used; nor does he discuss the relationship between the verbal intelligence tested by the Mill Hill test and actual speech. In view of the fact that he treats the scores as meaningful, valid and reliable it would be hard to find a clearer statement of a theory of verbal deficit. Indeed, in his discussion of the second experiment in (1961) he states that:

'The depressed scores on the verbal test for those working-class boys who have very high non-verbal scores, could be expected in terms of the linguistic deprivation experienced in their social background.'⁴

3 See p.32.

4 Bernstein (1961), p.90.

Without the concept of unequal access to public and formal language there would be little left of Bernstein's hypothesis on the relationship between language and social class. This concept of verbal deficit is compounded by his view of the inadequacies of public language itself and is substantiated by his own experiments.

4. The papers published in and after 1961

In these papers Bernstein seeks to refine and develop the hypothesis on class-related modes of language-use put forward in his earlier papers. The most important innovation is the abandonment of the concepts of public and formal language in favour of those of restricted code and elaborated code. Unlike public and formal language, which were presented as observable varieties of speech, the codes are regulative principles governing speech and are at one and the same time linguistic, psychological and sociological in character. But the concept of two substantially different modes of language-use is preserved in the form of speech regulated by restricted and elaborated code respectively. As with public and formal language, social access to the codes - and thus to the two modes of language-use - is unequal: in general, the lower working-class (defined in (1962) as 29% of the population⁵) does not have access to elaborated code. This point is as fundamental to the post-1960 papers as it is to the early papers, and is restated as recently as (1971a):

'One of the effects of the class system is to limit access to elaborated codes.' (p.176)

Thus the lower working-class still displays a verbal deficit in relation to the rest of the population. In (1969), (1971a) and (1973) Bernstein is anxious to stress that he is not saying that the lower working-class is incapable of producing speech regulated by elaborated code, merely that there are restrictions on their use of such speech:

'At no time did I ever consider that I was concerned with differences between social groups at the level of competency; that is, differences between social groups which had their origin in their basic tacit understanding of the linguistic rule system. I was fundamentally concerned with performance, that is; I was interested in the sociological controls on the use to which this common understanding was put.' (pp.242-243)

A similar point was already made in (1969):

'Because a code is restricted it does not mean that a child is non-verbal, nor is he in the technical sense linguistically deprived, for he possesses the same tacit understanding of the linguistic rule system as any child. It simply means that there is a restriction on the contexts and on the conditions which will orient the child to universalistic orders of meaning, and to making those linguistic choices through which such meanings are realized and so made public.'
(p.197)

5

See p.81.

Two observations should be made. First, restrictions of this kind on performance constitute a verbal deficit. If, in a given context, speaker A is able to produce a range of varieties of speech but speaker B, as the result of some 'sociological control', is able to produce only one of the varieties open to speaker A, then speaker B shows a verbal deficit in relation to speaker A, whatever the reason for the deficit. Second, Bernstein's hypothesis is muddled here. If elaborated speech variants are controlled by elaborated code and if some sections of the population do not have access to elaborated code, then it is hard to see how these sections of the population can produce elaborated speech variants, except by mimicry. The contradiction could be resolved by saying that the lower working-class has only partial access to elaborated code; but Bernstein does not say this. But even a concept of partial access to elaborated code would presumably imply partial access to elaborated speech variants and thus a concept of verbal deficit.

What is Bernstein's view of speech regulated by restricted code? His definition of the linguistic dimension of the codes in terms of syntactic predictability in (1962), (1965) and (1970) could be interpreted as implying a comparison that is unfavourable to speech regulated by restricted code. However, Bernstein seems to have abandoned this definition in favour of one in terms of relative implicitness and explicitness. In general, the post-1960 papers do not contain extreme, negative value-judgements of the kind made on public language in the early papers but there are at least some echoes of earlier value-judgements:

'The rigid range of syntactic possibilities leads to difficulty in conveying linguistically logical sequence and stress. The verbal planning function is shortened, and this often creates in sustained speech sequences a large measure of dislocation or disjunction. The thoughts are often strung together like beads on a frame rather than following a planned sequence.' (p.134)

Similarly, the discussion of the imaginary conversation between a husband and wife and the Millers in (1971a) echoes earlier positive value-judgements on formal language. For example, the reader is told that the speech in this imaginary conversation 'shows careful editing, at both the grammatical and lexical levels.' (p.177)

More important is Bernstein's contention that 'elaborated codes orient their users towards universalistic meanings, whereas restricted codes orient, sensitise, their users to particularistic meanings' (p.176). Universalistic meanings are relatively explicit and context-free, while particularistic meanings are relatively implicit, context-bound and local. Although Bernstein does not say so, anyone who is restricted to particularistic meanings or whose access to universalistic meanings is limited will show a verbal deficit, at least at the semantic level, in relation to someone with full access to both kinds of meaning.

5. Conclusion

Bernstein's basic sociolinguistic model of two dichotomous modes of language-use, with unequal social access to both, inevitably entails the hypothesis that those speakers who do not have access, or full access, to both modes show a verbal deficit in relation to other speakers. No amount of praise for the

aesthetic qualities, metaphor and pithiness in public language or for the potential of meanings in speech regulated by restricted code can alter this.

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REVIEW OF

M.A.K. Halliday (1975) Learning How to Mean: Explorations in the Development of Language.
London: Edward Arnold (Explorations in Language Study)

In Learning How to Mean Michael Halliday presents "a tentative framework for a functional, or sociolinguistic, account of the early development of the mother tongue" (p.6), carefully illustrated with a detailed case study of how his own son, Nigel, learnt language in the early stages, with the prime focus on the period 9 months to 2 years. As the term 'sociolinguistic' suggests, the emphasis is on linguistic development as a social process; a biological foundation to language is not denied, but the emphasis is on how the child learns language in the social environment in which he finds himself. Like Brown (1973, p.63), Halliday favours a 'rich interpretation' of child language, i.e. an approach based on the investigation of meaning, rather than structure. And like Brown, Halliday adopts a non-autonomous approach to language, in which linguistic development is seen as "an aspect and a concomitant of ongoing developmental processes of a more general kind" (p.139). But there is an important difference. Whereas Brown has tentatively linked language development to cognitive development, to aspects of the Piagetian cognitive sequence, with the child's first sentences being seen as expressing "the construction of reality which is the terminal achievement of sensori-motor intelligence" (Brown, 1973, p.200), Halliday offers an alternative (but he would argue complementary) perspective in which the child's learning of language is linked to the learning of culture. In Halliday's view, "a child's construction of a semantic system and his construction of a social system take place side by side, as two aspects of a single unitary process" (p.121). As the child learns the meanings of his culture in his social interactions with others, he simultaneously learns the primary means through which these meanings are encoded, the linguistic system.

There are important points of contact between the present study and Halliday's previous work, and of particular interest is the paper 'Relevant models of language' (Halliday, 1969, 1973). In this relatively informal paper Halliday offered teachers a perspective on the child's uses of language, arguing that the normal child by the time he starts school at five uses language for seven functions, has seven 'models' of what language is used for. These functions are now well known and may be listed briefly:

1. Instrumental: language for the satisfaction of material needs ('I want');
2. Regulatory: language in the control of behaviour ('do as I tell you');
3. Interactional: language as a means of getting along with others ('me and you');
4. Personal: language for the expression of self identity ('here I come');
5. Heuristic: language as a means of exploring the environment ('tell me why');

6. Imaginative: language to create a world of the child's own making ('lets pretend');
7. Representational (later informative): language for the communication of content, the expression of propositions ('I've got something to tell you').

Halliday's view is that by five children have developed a range of uses of language for each function, though which uses depends on their socialization experiences.

In Learning How to Mean Halliday puts these seven functions in a different focus, assigning them a fundamental role in his functional account of early language development. He sees early language development as a process of learning to mean, of learning a set of options for expressing each of the functions, a 'meaning potential' for each function. He puts forward two particularly interesting hypotheses:

1. There is a developmental sequence in the child's learning to express these functions, with the functions appearing "approximately in the order listed, and in any case with the 'informative' significantly last" (Halliday, 1975, p.37).
2. In the early stage the functions are separate from one another, with each utterance having just one function.

The significance of the second hypothesis is that Halliday conceives of language development as a process of moving from the child's system in which each utterance serves a single function to the adult system in which each utterance is pluri-functional. In essence, Learning How to Mean is an attempt to trace the development of the 'macro-functions' of the adult system, the 'ideational', 'interpersonal' and 'textual' components, from their foundations in the child's earliest utterances where initially the child lacks the grammar to integrate functions and to express more than one meaning at a time.

What are the findings? The first hypothesis concerning the order in which the functions appear did not receive very much support. There was no sign of a developmental progression within the first four functions. Furthermore, the imaginative seemed to emerge before the heuristic, but the origin of the heuristic function was a problem area. Finally, the informative function did not appear in the early stage (Phase I)*, in fact not until the end of Phase II; however this was not entirely unexpected as it was thought that the informative would appear significantly later than the others. The second hypothesis, by contrast, received a large amount of support:

'The functions themselves ... emerge with remarkable clarity ... it was possible, throughout NL1-5, to assign utterances to expressions, expressions to meanings and meanings to functions with relatively little doubt or ambiguity. There

* Halliday provides descriptions of Nigel's language (NL) at 1½ monthly intervals, beginning at 9-10½ months (NL1). Phase I covers NL1-5, Phase II periods NL6-9, and Phase III NL10 onwards.

was one significant exception to this (the heuristic function) ... otherwise, although the functions clearly overlap in principle, or at least shade into one another, the value of an element at all levels in the system was not difficult to establish' (Halliday, 1975, pp.40-41).

The question of clarity of the child's utterances for other researchers is a matter to which we shall return shortly.

It will be helpful to summarize Nigel's progress diagrammatically, drawing on two charts given by Halliday (Table 1, p.147; Figure 7, p.158).

Phase I (NL1-5)

NL1 9-10½ mo.
NL3 12-13½ mo.
NL4 13½-15 mo.

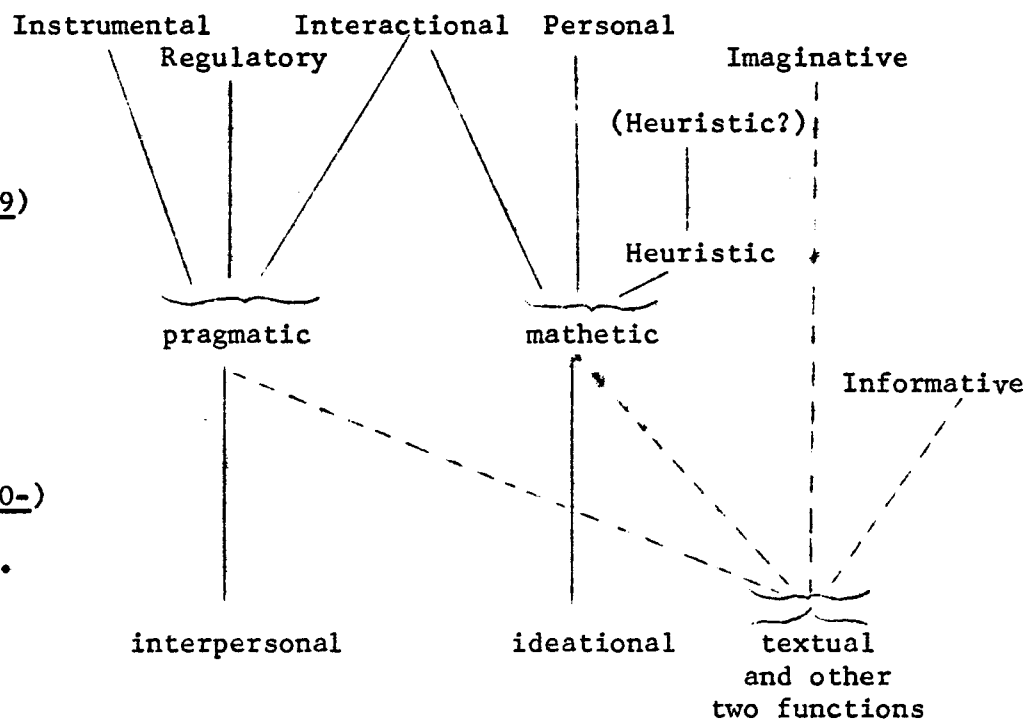
Phase II (NL6-9)

NL6 16½-18 mo.

NL9 21-22½ mo.

Phase III (NL10-)

NL10 22½-24 mo.



It can be seen that Phase II is the critical transitional period. There are two aspects to this. It is the time when the child combines early functions to yield a 'mathetic' function, that is, language for learning about the social and material environment, the language of observation, recall and prediction, and a 'pragmatic' function, that is, language to manipulate others, language that requires others to respond. The child's utterances are for a time either mathetic or pragmatic, and it is of interest that for a while Nigel made the distinction remarkably explicit, using a falling intonation for the mathetic and a rising intonation for the pragmatic. Later the child's utterances are both mathetic and pragmatic, foreshadowing the ideational and interpersonal functions of the adult system. The second aspect of Phase II is that it is the time when the child learns grammar. At first the child's grammar is functionally-specific; he has structures that are tied exclusively to just one function or the other. Halliday describes his interpretation of the structures as "child-oriented semantic".

Examples are:

- (pragmatic) more meat structure 'Request & Object of desire'
 (mathetic) green car structure 'Visual property & Object
 observed'

Later the child develops a more abstract and general grammar which makes it possible for him to mean more than one thing at a time.

In assessing the significance of Halliday's functional approach, there are two things to be taken into account, its relevance for teachers and its implications for linguists. I believe that Halliday's approach to language is of enormous significance for teachers. If there is a sense in which "educational failure is primarily linguistic failure" (Doughty and Thornton's General Introduction), it is in functional terms that the problem is to be understood, in terms of how the child learns to use language as a part of his culture. And Halliday has had a large influence on the development of Bernstein's code theory, in particular the definition of the codes in terms of 'critical socializing contexts'. But that said, I do not think that Learning How to Mean is strictly a book for teachers, although it appears in a series that aims to bridge the gap between the professional linguist and the teacher. Much of the material has appeared elsewhere in academic linguistic publications; and Halliday does not attempt to spell out the educational implications of his work (as he does in Language and Social Man) nor does he attempt to relate his functional scheme to other educationally-orientated functional schemes, in particular those of James Britton and Joan Tough - something that would be helpful for teachers. It is probably primarily in linguistic terms that Learning How to Mean is to be evaluated.

Halliday's approach to meaning, as exemplified in this book, is likely to be seen as controversial by many linguists. It is essentially the same approach as the one he has suggested for the study of certain adult uses of language, for example, the options open to a mother for controlling her child (Halliday, 1973; Turner, 1973). As readers of NLC will be aware, the sociosemantic approach has recently been extensively criticized by Fawcett (1975). The approach is essentially situational, going beyond what some would call the 'purely' linguistic, and it is this fact that draws the criticism. Many would agree with Fawcett (1975, p.36) that it is "fully justifiable for a linguist to limit his area of study to the code itself". For my part, I would agree with Palmer (1976, p.46) that "there is ... no such thing in semantics as linguistic ability that is unrelated to knowledge of the world", and argue that the analysis of certain kinds of discourse - particularly if one is interested in the question of the extent to which meanings are made explicit through language - involves grappling with the situation as a semiotic structure. In Learning How to Mean Halliday confronts the problem of situational meaning in perhaps its most extreme form, beginning his account of Nigel's meanings before Nigel is 'linguistic', when he has no structures or words and his sounds owe almost nothing to the English language. Halliday makes it very clear that the first six functions are "all extralinguistic": "they arise, and can be realized, independently of language, though language immeasurably extends the meaning potential that is associated with them" (p.64). Only the informative function is intrinsically linguistic. A function such as the

instrumental can easily be realized by non-verbal means, for example, by the child grasping an object firmly ('I want that'). (Fawcett points to this characteristic of socio-semantic options, namely that non-verbal realizations are sometimes possible, as an indication that the options do not belong to a level of language. I do not find this argument fully convincing as there are many grammatical categories, notably definite pronouns and demonstratives, which no one would doubt are part of language, but which may on occasion be replaced by an appropriate gesture such as pointing.) It is clear that Halliday is assigning Nigel's early utterances to meanings and functions had to make inferences about the child's intentions, assessments as to the extent to which he was satisfied by particular responses, interpretations concerning his expressions, gestures and so on. And, as we have seen, Halliday was able to do this with "relatively little doubt or ambiguity". But unfortunately, in Learning How to Mean Halliday does not really treat as problematic the fact that the reader is not in a position to make such interpretations. The emphasis is on presenting the results of the analysis, not on showing how it was done. Such information would be helpful, and indeed necessary for some if they are to accept the view that the child's utterances are strictly monofunctional in the early phase. I know that Halliday has prepared the data in a form that would be helpful, and hope that the publishers will publish it for the benefit of other researchers.

There is a further aspect of the approach I would like to consider, namely the relationship between function and structure. For Halliday language is as it is because of what it has to do; linguistic form reflects social function. In this perspective the child's early linguistic development is of critical interest since it is at this period that the child's linguistic structures are most directly related to social function; they are functionally-specific. A crucial question for anyone interested in relating function to form is: what do you have to make linguistic form look like in order to show a relationship? It is of interest that Halliday in dealing with this critical early period chooses a semantically-oriented interpretation of linguistic structure which he terms "child-oriented semantic": "semantic in order to relate it to function, child-oriented to show the part it plays developmentally" (p.47). An obvious difficulty with this decision is that it blurs the distinction between function and form, but maybe this accurately represents the facts at this stage. Moving from the child's system to the adult language, the question arises: how semantic should the lexico-grammar be if one is following the sociosemantic approach? The thrust in Halliday's (to appear) recent work is to semanticize the lexico-grammar. This is an emphasis with which Fawcett (1975, p.35) agrees but he sees the sociosemantic options as in part duplicating options in the lexico-grammar, "resulting in a disturbing degree of redundancy in the overall model". For Fawcett this is an argument against the sociosemantic approach. Naturally, like Fawcett, I am against introducing redundancy into the model if it can be avoided, but I do not think that the answer is to throw out the sociosemantic options; rather, I believe that for certain purposes a conception of the lexico-grammar in more formal, less semantic, terms is appropriate. And it is not just a matter of redundancy. If one takes as one's starting point the fact that the same formal items may realize different meanings in different types of social situation, and sees the aim of a sociosemantic approach as that of

describing these situationally-specific meanings, then the last thing one wants is a semantics that assigns fixed meanings to formal items, a point that has been made forcibly by John Sinclair in the context of his work on discourse analysis.

In Learning How to Mean Halliday is dealing with an area in which linguists themselves have much to learn. In his lively and highly original treatment, Halliday provides a clear statement of the issues which must be central to any future debate.

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Contributions to NLC and correspondence about contributions should be addressed to:

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