

FURTHER REMARKS ON HALLIDAY'S FUNCTIONAL GRAMMAR: A REPLY TO MATTHIESSEN & MARTIN

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1. Introduction

I am grateful to the editors for giving me the opportunity to reply to Matthiessen & Martin's paper (this volume) responding to my 1988a review article on Michael Halliday's *Introduction to Functional Grammar* (1985), henceforth *IFG*. The patronising and overtly hostile tone of their paper makes this a distasteful task, but it contains so many misrepresentations of what I said,¹ and their claim (§III) 'to have shown that all [my] critical remarks can be answered' is so far from true, that it should not be allowed to go unchallenged. It should also be borne in mind that although Matthiessen & Martin purport to be presenting the Systemic-Functional point of view on various issues, what they say is in certain cases, as we shall see, in conflict with what Halliday himself says in *IFG*.

I shall turn straightaway to the substantive issues raised in my review, discussing them under the headings of rank, theme, subject and grammaticalization.

2. Rank

2.1 A terminological preliminary

From the early scale-and-category days (cf. Halliday 1961) a distinctive feature of Halliday's theory of grammar has been that it incorporates a scale of rank which imposes significant constraints on the kinds of constituent structure analyses assigned to sentences. In *IFG* (§2.2) Halliday characterizes the distinction between rank-based and other constituency models by saying that they involve respectively 'minimal bracketing' and 'maximal bracketing'. The name he uses for the non-rank approach is 'immediate constituent (IC) analysis', and Matthiessen & Martin follow this terminology in a way which seems to me somewhat tendentious. In the strict sense of the term, 'immediate constituent' can be applied in any approach which assigns a hierarchical constituent structure to sentences, and indeed Halliday himself uses it in this way of his own rank-based approach in his paper 'Types of Structure', henceforth 'TS' (1981a:29,34): the ICs of some item are simply the constituents at the immediately next layer down in the hierarchy. (I shall follow this convenient terminology in a number of places below.) In order for 'IC analysis' to be properly contrasted with 'ranked constituent analysis', therefore, it must be given some more specific interpretation. Matthiessen & Martin (§2.3) gloss IC analysis as 'the approach developed within neo-Bloomfieldian linguistics'. Although neo-Bloomfieldian linguistics was undoubtedly a major influence on generative grammar, it would not be valid to suggest that the constituent structures assigned in, for example, Chomskyan grammar, are arrived at by methodological procedures like those put forward in the classic neo-Bloomfieldian paper on the topic, Wells 1947. To avoid subscribing to any such implication, I will draw the contrast simply in terms of 'rank grammars' and 'non-rank grammars'. This brings out the point that Halliday's theory of constituent structure is distinct from others by virtue of certain special features involving the concept of rank: the issue is whether this concept is well-motivated and helpful.

2.2 Hypotactic univariate structures

In my review I questioned Halliday's treatment of what he calls hypotactic univariate structures. My argument, in summary, was as follows:

- (1)
- i Given three elements related hypotactically, the theory makes available three possible structures: $\alpha \beta \gamma$, $\alpha\alpha \alpha\beta \beta$, $\alpha \beta\alpha \beta\beta$.²
 - ii However, while there is a genuine contrast between $\alpha\alpha \alpha\beta \beta$ and $\alpha \beta \gamma$ (or $\alpha \beta\alpha \beta\beta$), there is no such genuine contrast between $\alpha \beta\alpha \beta\beta$ and $\alpha \beta \gamma$, and we consequently have no satisfactory, principled basis for deciding which of these latter two structures to assign in particular instances. The theory is therefore not restrictive enough, in that it allows for contrasts which don't in fact occur. (In parataxis, the three comparable structures, 1 2 3, 11 12 2, 1 21 22 are needed to handle three genuinely contrasting structures.)
 - iii The natural way of remedying this excessive richness is to restrict hypotactic structures to two elements at any one layer, thus allowing the two layered structures $\alpha\alpha \alpha\beta \beta$, $\alpha \beta\alpha \beta\beta$, but excluding the linearly recursive structure $\alpha \beta \gamma$.
 - iv The resultant structures involve maximal bracketing, whereas for multivariate structures the theory adopts a minimal bracketing approach, and no proper justification is provided for setting up a distinction between two types of structure along these lines.

An initial point to be made about Matthiessen & Martin's reply on these matters is that they quote (SII.2.3) my observation 'it seems to me that hypotactic univariate structures do not lend themselves satisfactorily to the minimal bracketing principle' outside the context of this argument, where its justification depends on (1iii). They thus feel able to remark: 'This claim seems rather odd in view of the contrast between an IC analysis of *those two splendid old electric trains* as contrasted with the analysis of it in terms of hypotactic univariate structure', namely $\epsilon^{\wedge} \delta^{\wedge} \gamma^{\wedge} \beta^{\wedge} \alpha$. 'Rather odd' is an (obvious) understatement: my observation would be patently absurd if there were such a contrast. But argument (1iii) excludes the $\epsilon^{\wedge} \delta^{\wedge} \gamma^{\wedge} \beta^{\wedge} \alpha$ structure, replacing it with one involving a layering of binary structures identical with the so-called 'IC analysis'! [Misrepresentation₁] (For more specific reasons for rejecting the $\epsilon^{\wedge} \delta^{\wedge} \gamma^{\wedge} \beta^{\wedge} \alpha$ analysis of this example, see below.)

Before explicitly addressing my argument (1ii), Matthiessen & Martin implicitly reject it while commenting on my discussion of

- (2) *I'll tell him you'll be there if you can*

which I presented as one where the IFG analysis would be $\alpha^{\wedge} \beta^{\wedge} \gamma$. Matthiessen & Martin write: 'it is necessary to stop to correct [my] claim that Halliday's analysis of [(2)] is $\alpha \beta \gamma$; it is not — it is $\alpha \beta\alpha \beta\beta$: cf. IFG pp. 200-1. The reason is very simple: the domain of the projection of *I'll tell him is you'll be there if you can* and not just *you'll be there*; the extent of the domain can be seen clearly with the paratactic alternative: *I'll tell him "Henry'll be there if he can"*' (SII.2.3). To say that 'the reason is very simple' is to implicate that I have not just made a mistake, but made a particularly obvious and elementary one. But there is in fact no basis for saying that I have made a mistake at all here: to present my analysis as incorrect relative to IFG is a serious misrepresentation [Misrepresentation₂]. The examples on pp. 200-201 of IFG differ in various respects from (2), and none is assigned the structure $\alpha^{\wedge} \beta\alpha^{\wedge} \beta\beta$; arguably the one resembling it most closely is

- (3) *She took her umbrella in case it rained when she was leaving*

(in the interpretation where *when she was leaving* is dependent on *in case it rained*, rather than on *she took her umbrella*, i.e. where it gives the time of the potential raining, not of her taking the umbrella) to which Halliday gives the same analysis as I do to (2), namely $\alpha^{\wedge} \beta^{\wedge} \gamma$. The difference between (3) and (2) is that the second clause in (2) is a projection, while that in (3) is

an expansion (more specifically, an enhancement) — but there is nothing in *IFG* to suggest that this requires a difference in structure at primary delicacy: quite the contrary, for expansion and projection are treated in parallel fashion at this degree of delicacy. The sort of reasoning Matthiessen & Martin use for (2) can be adapted to yield the conclusion that — contrary to Halliday — the analysis of (3) too should be $\alpha^{\wedge}\beta\alpha^{\wedge}\beta\beta$, not $\alpha^{\wedge}\beta^{\wedge}\gamma$: the reason she took her umbrella was not that it might rain (at some unspecified time), but that it might rain when she was leaving. An *IFG* example where the second clause is of the projection type appears a few pages earlier (p. 196):

(4) *John reported that Mary had told him that Fred had said the day would be fine*

Matthiessen & Martin's 'very simple' reasoning would clearly lead to an analysis of this as $\alpha^{\wedge}\beta\alpha^{\wedge}\beta\beta\alpha^{\wedge}\beta\beta\beta$ — compare *John reported "Mary told me that Fred had said the day would be fine"* and *Mary told me "Fred said the day would be fine."* But in fact Halliday himself analyses (4) as $\alpha^{\wedge}\beta^{\wedge}\gamma^{\wedge}\delta$.

There are two conclusions to be drawn from Matthiessen & Martin's comments on (2). The first is that, ironically, the kind of reasoning they use leads to nested binary structures for hypotaxis, not linearly recursive ones, so that in fact it lends support to the sort of analyses I am advocating, against Halliday, in (1iii). The second conclusion is that they are clearly applying a different kind of reasoning from Halliday: hence my warning at the outset that they cannot be accepted as authoritative spokespersons for the *IFG* theory and description of English.

To say that there is a very simple reason for selecting an $\alpha\beta\alpha\beta\beta$ analysis over $\alpha\beta\gamma$ is to betray a serious misunderstanding of the issue raised in (1ii). The fullest theoretical discussion of hypotactic univariate structures is in 'TS'. Halliday acknowledges (p. 34) that there is an 'important difference' between parataxis and hypotaxis, namely that in parataxis 'all possible bracketings can occur with a clear distinction in meaning' (cf. the parenthesized sentence at the end of my (1ii) above), whereas this is not so with hypotaxis. Here ' $\alpha\beta\gamma$ will contrast clearly with $\alpha\alpha\alpha\beta\beta\beta\dots$ but there will be no such clear distinction between $\alpha\beta\gamma$ and $\alpha\beta\alpha\beta\beta$ '. He goes on to say that 'in a hypotactic structure the final pair of elements may be represented indifferently, in most cases at least, either as on the same layer (as the lowest element) or as forming a new inner layer: $\alpha\beta\gamma$ is the same as $\alpha\beta\alpha\beta\beta$, $\alpha\beta\gamma\delta$ as $\alpha\beta\gamma\alpha\gamma\beta$, and so on. On grounds of simplicity, since the introduction of a new layer is an additional complexity in the structure, the single layer representation $\alpha\beta\gamma$, $\alpha\beta\gamma\delta$, etc. is preferred' (p. 35).

In *IFG* Halliday draws attention to the distinction between $\alpha\beta\gamma$ and $\alpha\alpha\alpha\beta\beta$ (by reference to the ambiguity of (3) above), but this time he does not discuss the problem of differentiating between $\alpha\beta\gamma$ and $\alpha\beta\alpha\beta\beta$. I was wrong, as Matthiessen & Martin point out, in saying that *IFG* always uses $\alpha\beta\gamma$ rather than $\alpha\beta\alpha\beta\beta$ for the clause complex: I induced on the basis of too few examples that he was following the simplicity argument just quoted from 'TS'. But this has no bearing on my argument (as is evident from the fact that it does not appear in (1)). The crucial point is that there is still no principled basis for choosing between the two structures $\alpha\beta\gamma$ and $\alpha\beta\alpha\beta\beta$ that the theory provides. No clearer demonstration of this can be given than by noting that the very same (unambiguous) example is analysed now in one way, now in the other:

(5) *She set to work || nibbling first at one and then at the other || and growing sometimes taller and sometimes shorter, || until she had brought herself down to her usual height*

This is given the structure $\alpha\beta_1\beta_2\gamma$ on p. 199, but $\alpha\beta\alpha_1\beta\alpha_2\beta\beta$ on p. 270. But this is not the only evidence for my claim that the choice between $\alpha\beta\gamma$ and $\alpha\beta\alpha\beta\beta$ is not made on any principled basis. Consider also the example on pp. 193-4, only the second in the chapter on clause complexes:

(6) *I don't mind || if you leave || as soon as you've finished || as long as you're back || when I need you.*

This is given the structure $\alpha\alpha \alpha\beta \alpha\gamma \beta\alpha \beta\beta$. Why is *as long as you're back when I need you* bracketed together as a separate constituent, whereas the very similar *if you leave as soon as you've finished* is not? Halliday mentions that the order of the two topmost blocks could be reversed to give *As long as you're back when I need you, I don't mind if you leave as soon as you've finished*. This may be a factor leading him to bracket the fourth and fifth clauses together (I have not noted any representations of the form $\beta^{\wedge}\gamma^{\wedge}\alpha$)³; the second and third clauses can together move around the first, however, to give *If you leave as soon as you've finished I don't mind, as long as you're back when I need you, so we are still left with the question of why the fourth and fifth are bracketed together while the second and third are not*. And note that such reordering is also possible in (3) (in the interpretation focussed on above, i.e. that where the *when*-clause gives the time of the possible raining), giving *In case it rained when she was leaving, she took her umbrella* — and yet no nesting is proposed here. One aim of IFG is to enable readers to apply the theory and description it presents to the analysis of English texts; but how we are to choose between $\alpha \beta \gamma$ and $\alpha \beta\alpha \beta\beta$ when doing that remains a mystery.

My argument (1) was first presented in essence in Huddleston (1977); it is reported, but without substantive discussion, in Butler (1985:35). It is worth noting, therefore, that when Matthiessen & Martin do come to address (1ii) directly they have to rely on a personal communication from Halliday: this suggests that (at least as far as Matthiessen & Martin are aware) the issue has not been considered worth worrying about in the Systemic-Functional literature — and the way in which they report Halliday's personal communication suggests that they share that view. They say we need all three of the structures given in (1i) in order to distinguish between the examples in (7) [their (72)-(74)]:

(7)	i	<i>infant birth rate</i>	$\gamma^{\wedge}\beta^{\wedge}\alpha$	"rate of birth of infants"
	ii	<i>record birth rate</i>	$\beta^{\wedge}\alpha\beta^{\wedge}\alpha\alpha$	"birth-rate of record proportions"
	iii	<i>still birth rate</i>	$\beta\alpha^{\wedge}\beta\beta^{\wedge}\alpha$	"rate of still-births"

No commentary or explication is given beyond what I have cited in (7). There is no discussion of the classical distinction between a compound noun and a phrasal modifier-head construction (e.g. *glasshouse* vs *glass house*); *birth rate* would commonly be written as a single orthographic word, or at least hyphenated, in (ii) and pronounced with the prosody of *glasshouse*, not *glass house*, but in (iii) it is *still birth* that is compounded, with potentially the same kind of orthographic and prosodic marking. What is the Systemic-Functional position on compounds: are they single units at word rank or do they occupy two elements in structure at group rank? A further problem with (7) is that there is no recognition of the pragmatic oddness of (i): what does *infant* add to *birth rate* on its own? A more natural expression would be *infant death rate*, but I see no reason to accept that this (with *death rate* pronounced 'death rate, as in Wells 1990, for example) is distinct in bracketing from *record death rate*. The kind of gloss Matthiessen & Martin give for (ii) could apply equally well to either: "death-rate of infants" and "death-rate of record proportions". *Death* could be replaced by *mortality*: *infant mortality rate* and *record mortality rate*; and since there is a sense of *mortality* which incorporates the meaning "rate", both could be reduced to two-word structures: *infant mortality* and *record mortality*. It is true that it would also be possible (though quite marked) to pronounce or write *infant death rate* with *infant* and *death* grouped together, i.e. as structure (iii). And there would be no effective difference in meaning. But this is not a reason for giving it a single structure, $\gamma^{\wedge}\beta^{\wedge}\alpha$. We must simply recognize that in this particular instance there is neutralization of meaning between the structures $\beta^{\wedge}\alpha\beta^{\wedge}\alpha\alpha$ and $\beta\alpha^{\wedge}\beta\beta^{\wedge}\alpha$. This is not a theoretical problem because it matches the semantic neutralization that we also find between *death-rate of infants* and *rate of infant deaths*. Under my proposal (1iii), whereby hypotactic structures are binary, we have to allow for a small amount of neutralization of meaning between $\alpha\alpha \alpha\beta \beta$ and $\alpha \beta\alpha \beta\beta$ — neutralization with an explanatory parallel in multivariate structures involving embedding/rankshift; under Halliday's position (1i), where both binary and multiple structures are allowed, with or without layering/nesting, there is — as the quotation from 'TS' makes clear — massive and systematic neutralization of meaning between the structures $\alpha \beta \gamma$ and $\alpha \beta\alpha \beta\beta$, neutralization that has no justification or explanation elsewhere. In my view it is quite unsatisfactory to present (7) as though it could provide motivation for this. One set of three examples, given without explication, cannot provide a satisfactory basis for a systematic three-way distinction

throughout the grammar. Are Matthiessen & Martin maintaining, contrary to 'TS', that a three-way contrast does apply throughout? If it applies within the clause complex, what are illustrative examples? If it doesn't apply there, why should that be so? It is incumbent upon Matthiessen & Martin to address such questions.

A second point Matthiessen & Martin report from Halliday's personal communication is that the $\alpha \beta \gamma$ structure 'allows us not to have to choose between $\alpha \beta \alpha \beta \beta$ and $\alpha \alpha \alpha \beta \beta$ ' in such examples as (8) [their (75)]:

- (8) *It appeared that Teddy, | | who owned the most magnificent dachshund, | | which was Lurid Liberace, | | ...*

Again there is no further commentary — and in particular they fail to take up the problems associated with Halliday's treatment of non-restrictive relative clauses that I raised in my review (p. 145, n.7). It has not been properly argued that a non-restrictive relative with a nominal group as antecedent has the function of modifier to the clause containing that nominal group. Such clauses differ from the dependent clauses in constructions like (2)-(4); one important difference is that there can be more than one at the same 'depth', as in

- (9) *Both Kim, who hadn't been before, and Pat, who had been many times, enjoyed the performance immensely*

Here both relative clauses will — under a Systemic-Functional analysis — be dependent on *both Kim and Pat enjoyed the performance immensely*, but they are not themselves related in parataxis (or in any other way), so that the immediate structure cannot be $\alpha \beta$, with nested β . Indeed the example cannot be analysed as a realization of any of the three structures listed in (1i). Within the 'TS' framework it would be $\alpha \beta i \beta ii$, though there is no mention of this kind of structure in *IFG*; it seems to me very questionable whether it is consistent with the characterization that is given of a univariate structure. But in any case the highly problematic nature of the non-restrictive relative construction makes it unsafe to base any general conclusions about hypotactic clause complex structure on it. And again it is not a valid argument to plead that having an $\alpha \beta \gamma$ structure in addition to $\alpha \alpha \alpha \beta \alpha$ and $\alpha \beta \alpha \beta \beta$ enables one to avoid having to choose between the latter two in the very special and uncommon case illustrated in (8), when in fact admitting this third structure means that one now has to make an unprincipled choice between it and $\alpha \beta \alpha \beta \beta$ in innumerable common or garden examples like (2)-(3).

I pointed out in my review (p. 151) that the $\alpha \beta \gamma$ structure is interpreted differently for the nominal group than for the clause complex. For the latter it contrasts clearly with $\alpha \alpha \alpha \beta \beta$, as illustrated in the ambiguity of (3), whereas there is no contrast with $\alpha \beta \alpha \beta \beta$, as seen in the possibility of analysing (5) in either way. In this *IFG* follows 'TS' except that (as noted above) it does not consistently follow the 'simplicity' policy of using $\alpha \beta \gamma$ in preference to $\alpha \beta \alpha \beta \beta$. For the nominal group, however, the structure that $\alpha \beta \gamma$ contrasts with is $\alpha \beta \alpha \beta \beta$:

- | | | | |
|------|----|----------------------------|-----------------------------------|
| (10) | i | <i>very small animals</i> | $\beta \beta \beta \alpha \alpha$ |
| | ii | <i>old electric trains</i> | $\gamma \beta \alpha$ |

$\alpha \alpha \alpha \beta \beta$ does not appear for the nominal group in *IFG*, though (as we have seen) Matthiessen & Martin are now offering *record birth rate* as an instance of that structure. This raises the question of why and how *old electric trains* differs from *record birth rate* (other than in the matter of compounding referred to above). The meaning they give for the latter, "birth-rate of record proportions" can be paralleled by that applying to the former, "electric trains that are old". Crucially, (10ii) is not parallel in interpretation to (3). In (3), according to Halliday's analysis, *when she was leaving* modifies *in case it rained*, and this in turn modifies *she took her umbrella*. But in (10ii) *old* clearly does not modify *electric*: it is the (electric) trains that are old, not the electricity. This is brought out in Halliday's own explanation (*IFG*:170): '[We start] with the most general term, *trains*. Moving to the left, we get: (which trains? —) *electric trains*; (which electric trains? —) *old electric trains ...*'. Yet two pages later Halliday says that in a hypotactic univariate structure ' α is modified by β , which is modified by γ , ...', a statement

which matches his interpretation of (3), but is sharply at variance with what he has just said about (10ii). The same objection is made by Hudson, who expresses it in more forthright terms: 'at times total confusion seems to reign ..., because the dependency relation is given a completely different interpretation when applied to the internal structure of the [nominal group]' (1986:808). In their response, Matthiessen & Martin deny that the two interpretations of the $\alpha\beta\gamma$ structure are inconsistent: they are merely DIFFERENT. They say first that the relation of modification is MARKED differently in different contexts, as illustrated in their Table 7. But this has nothing whatever to do with the objection I had made, which was concerned with inconsistency in the meaning ascribed to the $\alpha\beta\gamma$ structure. On this issue of meaning Matthiessen & Martin merely assert that the difference is a matter of variation from one grammatical context to another, not of inconsistency. They do not attempt to explain why the move from a clause complex context to a nominal group context should effect a change in the interpretation of γ from modifier of β to modifier of $\alpha + \beta$.

Instead they refer the reader to 'Martin (1988) [which] explores these different interpretations in detail'. However, when one follows up that reference one finds that Martin's treatment is significantly different from that of IFG, from the one which Matthiessen & Martin are seeking to defend in their paper. For expansion in the clause complex, for example, he draws a contrast between just two bracketings, which he shows in tree structure form (Figures 17 & 18, p. 257):



(i) is the structure for Martin's example (29), given here as (12i); (ii) is said to represent the more common pattern and is not illustrated, but presumably (12ii), on the understanding that *because she was tired* gives the reason just for Mary's leaving, would be an example:

- (12) i *John left when Mary left because they were tired as often happens*
 ii *John left when Mary left because she was tired as she often is*

Representations of (11) in linear form, using the IFG notation, would be $\alpha\alpha\alpha^{\wedge}\alpha\alpha\beta^{\wedge}\alpha\beta^{\wedge}\beta$ and $\alpha^{\wedge}\beta\alpha^{\wedge}\beta\beta\alpha^{\wedge}\beta\beta\beta$ respectively, but Martin does not use that form of notation in this 1988 paper; instead he uses $\alpha^{\wedge}\beta^{\wedge}\gamma^{\wedge}\delta$. But this is used for both: 'No attempt has been [made] in this section to distinguish between Figure 17 and Figure 18 as far as the $\alpha, \beta, \gamma, \delta, \epsilon...$ notation is concerned'. The interpretation of $\alpha\beta\gamma\dots$ representations thus has to be STIPULATED for particular constructions. It will be clear that, with respect to the issues involved in the current debate, this is quite different from IFG — and it seems to me seriously misleading not to acknowledge that this is so. $\alpha\beta\gamma\delta$ is not used to represent a structure that contrasts with $\alpha\alpha\alpha^{\wedge}\alpha\alpha\beta^{\wedge}\alpha\beta^{\wedge}\beta$ and $\alpha^{\wedge}\beta\alpha^{\wedge}\beta\beta\alpha^{\wedge}\beta\beta\beta$, but as a substitute for either of these, a notation which does not show the bracketing; in representations that do show the bracketing, we have purely binary structures. This position is not distinct from my (1iii), and quite at variance with the Hallidayan position that Matthiessen & Martin claim to justify in (7).

Let us return now to the Matthiessen & Martin paper. They go on to '[emphasize] that whether we choose to follow Halliday or to follow [my] suggestion to restrict hypotaxis to binary structures [i.e. to accept (1iii)] is a DESCRIPTIVE decision. It does not affect the theory of interdependency structure nor the theory of rank.' Again, this is simply asserted: there is no explanation or justification of what they take to be theory, and what mere description, in the IFG framework. Is the distinction between univariate and multivariate structures theoretical or descriptive? Presumably theoretical, for it is crucially linked to the theory of rank — the requirement of total accountability applies to the ranks proper, but not to complexes (at least in general: see below). And presumably the distinction within the univariate category between parataxis and hypotaxis is also theoretical: if there is a 'theory of interdependency structure',

it could hardly fail to deal with such a general distinction. I cannot then see how accepting that hypotactic structures are always binary, not linearly recursive, could fail to have significant implications for this theory of interdependency. I would contend that it makes the distinction between univariate and multivariate structures quite problematic.

In 'TS' Halliday introduces the distinction as follows (p. 31): 'A multivariate structure is one involving more than one variable; a univariate structure is one involving only one variable. The elements of a multivariate structure are thus different variables each occurring once only; let us represent this for the moment as X·Y·Z. The elements of a univariate structure are repetitions of the same variable, which we can represent provisionally as X·X·X.' (When the distinction between parataxis and hypotaxis is made, this provisional representation is replaced by $X_1 \cdot X_2 \cdot X_3$ and $X_\alpha \cdot X_\beta \cdot X_\gamma$ respectively, and then the invariant X is deleted to yield the familiar representations 1·2·3 and $\alpha \cdot \beta \cdot \gamma$.) Elaborating a little later (p. 33), Halliday goes on: 'Multivariate structures ... are non-recursive: each structure comprises a specific set of variables each occurring once only. Univariate structures, on the other hand, both paratactic and hypotactic, are recursive; as structures they are indefinitely extendable, although in many cases 'stop rules' operate which set limits to the actual strings that may occur. ... Thus while a multivariate structure such as S·P·C is 'closed', a univariate structure such as 1·2·3 or $\alpha \cdot \beta \cdot \gamma$ is open-ended, implying the possibility of adding new elements in the same recursive relation.' Limiting hypotaxis in principle (rather than by particular 'stop rules') to two elements clearly requires theoretical revision of the distinction between univariate and multivariate structures. As far as hypotaxis and multivariate structures are concerned, the distinction will now have to rely wholly on the much less clear-cut distinction between layering/nesting and rankshift.

One problem with the formulation quoted from 'TS' is that it has univariate structures consisting of repetitions of the same variable/element. But it is very difficult to accept that α and β are instances of the same element of structure; in IFG Halliday gives them distinct names, Head and Modifier, and characteristically they are realized by different classes (e.g. main/independent vs dependent clauses in the clause complex). In this case, how does an $\alpha \beta$ structure differ from a binary multivariate one, which will likewise have distinct elements characteristically realized by different classes? Halliday remarks (IFG:198) 'that, although it is the functions that are labelled, the structure actually consists of the relationships between them. A univariate structure is an iteration of the same functional relationship'. But if there can be no more than two elements, we cannot have iteration of the same functional relationship within a single layer.

Moreover, if hypotactic structures are invariably binary, the analyses assigned will be of the maximal bracketing kind, whereas Halliday makes much of the fact that his approach to constituency involves minimal bracketing in contrast to the maximal bracketing of so-called IC analysis — Systemic-Functional Grammar needs to rely less on constituency because more of the descriptive work is done by functional labelling. A binary treatment of hypotaxis would hardly be consistent with this general approach. I made this point in my review (p. 149), contrasting the multivariate clause construction (13) and the univariate clause complex (14):

(13) I 'll tell him the truth under such circumstances
 S Fin Pred Comp Comp Adjunct

(14) I'll tell him | | you called | | if you like
 $\alpha\alpha$ $\alpha\beta$ β

(see Matthiessen & Martin's Figure 6). I argued that it is simply an artefact of the model and description that *the truth* and *under such circumstances* appear at the same hierarchical level, while *you called* and *if you like* do not. In response, Matthiessen & Martin write 'if the difference were simply an artefact, we should find a SYSTEMATIC difference within the clause that matches the distinction in the clause complex represented by the different structures for *I'll tell him you'll be there if you can* [i.e. (2) above] ($\alpha \wedge \alpha\beta \wedge \beta\beta$) and for [(14)] ($\alpha\alpha \wedge \alpha\beta \wedge \beta$). That is, we should be able to find the two alternative bracketings shown in Table 5.' (See their paper for Table 5.) That formulation itself implies that we can't find a comparable distinction in the

clause, and Matthiessen & Martin continue: 'as long as there is no evidence for a SYSTEMATIC alternation of this kind, the minimal bracketing of the multivariate analysis achieves precisely what we want to say: potentially different scopes are not reflected in the structure of the clause, whereas clause complexes can combine to give different bracketings, as shown in Table 6.' But the systematic alternation that Matthiessen & Martin imply to be lacking is in fact the source of one of the most common ambiguities in English. I used it in my 1984 textbook to illustrate the phenomenon of constituent structure ambiguity. The example I gave (p. 4) was *Liz attacked the man with a knife*, where *with a knife* can have as its scope either *attacked the man* (indicating a circumstance of the attack, namely what weapon was used) or just *man* (contributing to the defining description of the attackee): the structures correspond essentially to the upper and lower left-hand ones respectively in Matthiessen & Martin's Table 5. An example closer to those involved in the present debate is *I'll tell him the effect under such circumstances*. With the structure shown in (13), the Adjunct *under what circumstances* specifies the conditions under which I'll tell him the effect; in the structure where *under such circumstances* is a rankshifted Qualifier of *effect* the meaning can be glossed as "I'll tell him what the effect under such circumstances is". The fact that the potential structural ambiguity in (13) is pragmatically resolved in favour of the reading where *under such circumstances* has *tell him the truth* (rather than just *truth*) as scope parallels the fact that in the pragmatically natural reading of (14) *if you like* has *(I'll) tell him you called* (rather than just *(you) called*) as its scope. The alternation is as systematic in the clause case as in the clause complex case. Thus far from having countered my objection, Matthiessen & Martin have pointed the way to further evidence in support of it.

A major problem in evaluating the Systemic-Functional distinction between univariate and multivariate structures arises from the uncertainty concerning the applicability of constituent structure to univariate structures. At the outset ('TS':36), Halliday expressed the view 'that constituent structure is not a very adequate way of representing hypotaxis'. In *IFG* (201-202) he contrasts spoken and written language in the following terms: 'in spoken language [the clause complex] represents the dynamic potential of the system — the ability to "choreograph" very long and intricate patterns of semantic movement while maintaining a continuous flow of discourse that is coherent without being constructional. This kind of flow is very uncharacteristic of written language. Since grammatical theory evolved as the study of written language, it is good at synoptic-type "product" representations, with constituency as the organizing principle, but bad at dynamic-type "process" representations, which is what are needed for the interpretation of speech. A ball-and-chain picture of this kind [as illustrated in his Figure 7-7] is a small experiment in choreographic notation — something which unfortunately cannot be pursued here.' The idea of a non-constituency analysis is thus not developed: as far as (the rest of) *IFG* is concerned, then, the analysis proposed for univariate structures involves constituent structures just as much as multivariate ones do. The text is quite explicit on this point. For example, Halliday explains on p. 201 that the notation used for univariate structures 'expresses both constituency and dependency at the same time', and Figure 7-6 on that page illustrates how the notation can be translated into a labelled constituent structure tree diagram. The branches indicate the constituents and the symbols — paratactic 1, 2, etc., hypotactic α , β , etc. (together with secondary delicacy diacritics) label the structural function of the constituents. Matthiessen & Martin, however, appear to take a contrary view. They contrast constituency and interdependency as different modes of organization, associating constituency with multivariate structure, interdependency with univariate structure — see their Table 3. They go on to say (§II.2.1(i)) that 'complexes are non-constructional — that is, their elements are not to be seen as parts of wholes; the interpretation of clause complexes is through interdependency rather than constituency'. But no explanation is given as to what this means. When is an element in a structure a part/constituent, and when is it not? Take the univariate structure (3), for example, where the three elements, α , β , γ are (realized by) clauses: is it possible for something to be a clause without being a constituent? What are the terms in an interdependency relation if they are not constituents?⁴ I would contend that as it stands the idea is not sufficiently developed or explicit for it to be able to figure in any coherent argument.

2.3 Clause complex, sentence and total accountability

A distinctive feature of Halliday's version of rank grammar — in contrast, say, to that incorporated (with a different terminology) in tagmemics — is that it is constrained by the principle of 'total accountability'. This means that 'each unit should be fully identifiable in description', so that 'every item of the text is fully accounted for at all ranks' (1961:253) — a sentence will thus be analysable exhaustively into an integral number of clauses, and of groups, and of words, and of morphemes. This was a controversial feature of the early theory (cf. Matthews 1966) but, strangely, it is not mentioned in *IFG* — and for this reason Matthiessen & Martin imply that it is improper for me to devote lengthy discussion to it. Anyone wanting to use *IFG* as a basis for textual analysis, however, needs to know what the current position is on this issue, for it places clear limits on the class of permitted analyses; if Matthiessen & Martin object to my citing only relatively early works from the systemic literature, they should show in what way more recent ones shed light on this issue. They speak of my 'critique of Halliday's alleged position on total accountability in the theory' (§I.2), where 'alleged' implicates that they think I have got it wrong, but they conspicuously fail to say what they take his current position on this issue to be.

I noted in my review (pp. 142-143) that complex units (which have purely univariate structures) fall outside the scope of the total accountability requirement, with the unexplained exception of the clause complex. Before discussing the substantive issues involved in this exception, I must draw attention to another gross piece of misrepresentation on Matthiessen & Martin's part. They quote (§II.2.1(i)) a passage from my review in which I in turn quote from *IFG*: 'Halliday then says that a written "sentence can be treated as one clause complex, with the 'simple' (one clause) sentence as the limiting case". This implies that a clause complex may consist of a single clause'. Matthiessen & Martin then rephrase 'can be treated as one clause complex' as 'can be a clause complex' and on the basis of this substitution go on to say: 'it is hard to know what to respond to the implication Huddleston finds that a clause complex may be a single clause: Halliday writes "sentence" and Huddleston reads "clause complex" [Misrepresentation3]. It should not be necessary to point out that 'a written sentence can be a clause complex' has a quite different meaning from 'a written sentence can be treated as one clause complex'. The first involves *can* in a use implying "some/sometimes" (cf. Palmer 1979:153): "some sentences are clause complexes — but others are not (they are single clauses)". The second involves something closer to deontic *can*: approximately "it is legitimate to treat a written sentence — any written sentence — as a clause complex". Notice, moreover, that earlier on the same page Halliday writes 'A [grammatical] sentence will be defined, in fact, as a clause complex'.

It may be that I have mistaken Halliday's intentions, but if that is so I believe it is because of the lack of clarity and explicitness in *IFG* concerning the concepts of grammatical and written sentence and their relation to the clause and clause complex. On p. 25 he accepts the term 'sentence' as a unit in the grammatical constituent hierarchy, noting that this is a different sense from that where it is a unit of writing, and that the two senses may not always exactly correspond (though he does not give any examples where they do not). As a grammatical unit, the sentence belongs to a 'strict hierarchy of constituents, each one being related by constituency to the next. A sentence consists of clauses, which consist of groups (or phrases), which consist of words, which consist of morphemes.' This of course is very like the original 1961 account of rank — which allows a sentence to consist of a single clause. He indicates an intention to 'reconsider the significance of the term "sentence" in this hierarchy' in Ch. 7. The first (short) section of Ch. 7 is called "clause complex" and "sentence"; it is the source of the quotation above. Halliday adopts the position that 'the notion of "clause complex" enables us to account in full for the functional organization of the sentence'. The [grammatical] sentence is, as we have seen, defined as a clause complex, and he goes on: 'The clause complex will be the only grammatical unit which we shall recognize above the clause. Hence there will be no need to bring in the term "sentence" as a distinct grammatical category.' Now surely all this would be extremely misleading if the intention were not to simply make a (principled) change of terminology from 'sentence' to 'clause complex', but rather to change the term and at the same time narrow the extension of the newly named category so as to exclude from it what in the original terminology were sentences consisting of just one clause. And recall that the forward reference to Ch. 7 spoke

use, clause complex, group, or group complex on the other is that a sentence cannot be embedded/rankshifted: in *The point that impressed me most and that I'd like to look into* *that is this* ... the underlined expression is a rankshifted clause complex, not a sentence. (I have not found comparable examples in *IFG*, but I would be surprised if Halliday would use the embedded counterpart of '| | |' for them.) I very much agree with Halliday that it is the clause that provides the best way into the grammar. The sentence is then approached derivatively: in the simplest cases it is identical with a clause, and in others it consists of a combination of clauses, i.e. is identical with a clause complex. And since there is some indeterminacy as to whether two successive clauses in a text are grammatically united in a larger construction, there will be some indeterminacy over the boundaries between one sentence and the next.

4 Parataxis and rank.

A major problem for the *IFG* theory of rank constituency is raised by such examples as (16) [= (17) in my review, and (80) in Matthiessen & Martin]:

- (16) *You can't join a debating society and not speak*

This cannot satisfactorily be regarded as derived by ellipsis from *You can't join a debating society and you can't not speak*, whose meaning is quite different. What we need for (16) is a structure where *join a debating society* is a constituent coordinated with *not speak*, but this is not consistent with a rank analysis. Matthiessen & Martin's response is first to claim it as an advantage for *IFG* theory that the problem/difficulty arises in that theory, whereas in an analysis with VP conjunction — *you can't (join a debating society and not speak)* — the issue disappears and the example becomes no different from the prediction *you will join a debating society and not speak* (where the elided Mood element can be reinstated without a change in meaning similar to the first example: *you will join a debating society and you will not speak*)' (§II.2.4). But the difference between the two pairs is not lost in the analysis I am advocating: rather it appears in its proper place, in the semantics of coordination, where we need to describe the conditions under which subclausal coordination is or is not semantically equivalent to clausal coordination, as in such classic examples as (17) and (18) respectively:

- (17) i *Kim and Pat know Greek*
 ii *Kim knows Greek and Pat knows Greek*
 (18) i *Kim and Pat are sisters*
 ii *Kim is a sister and Pat is a sister*

The difference between Matthiessen & Martin's pairs — that the *can't* pair are non-equivalent while the *will* pair are equivalent — is due to the fact that the coordination is within the scope of a negative in (16), but not in *You will join a debating society and not speak*. The interaction between negation and coordination can be handled elegantly in a grammar where *join a debating society* is a constituent of *You can't join a debating society*, one which can be coordinated with such other like constituents (VPs) as *not speak*. A rank-based grammar does not allow this: here *join* enters immediately into construction with *can't*, not with *a debating society*. This counts as a significant argument in favour of non-rank grammars over rank grammars; to suggest instead, as Matthiessen & Martin do, that it is an argument in favour of rank grammars is to stand the standard type of linguistic argumentation on its head.

Matthiessen & Martin go on to claim that the problem for *IFG* that is raised by (16) is not in fact related to ellipsis: they argue that the allegedly elided *you can't* can be reinstated without loss of grammaticality, and hence the ellipsis analysis can be accepted after all. The issue, they say, is 'SYSTEMIC, not STRUCTURAL'. This solution, it seems to me, is completely lacking in credibility. Matthiessen & Martin themselves observe (§I.3(i)) that '[a] functional grammar is a "semanticky" one; the interpretation of the grammar presented in *IFG* is designed to bring out the SEMANTIC NATURALNESS of grammar', and indeed a major source of my disagreement with *IFG* stems from my belief that it allows the semantics to distort and greatly complicate the grammar (see §5 below). An analysis of (16) as involving ellipsis implies acceptance of a theory of the 'autonomy of syntactic structure' (to borrow terminology from the debates within

generative grammar) which is totally at variance with the spirit of *IFG* — and likewise with that of the very detailed study of ellipsis presented in the major Systemic-Functional work on the subject, Halliday & Hasan (1976).

Finally, Matthiessen & Martin contend that (16) is a grammatical metaphor for the conditional construction *If you join a debating society you can't not speak*. The same relation between 'metaphorical' and 'congruent' forms is said to appear in such pairs as *Make another move and I'll shoot* and *If you make another move I'll shoot* [their (81)-(82)], and the fact that the metaphorical version here involves no ellipsis is seen as support for the view that the problem in (16) is not related to ellipsis. Here, however, Matthiessen & Martin underestimate the extent of the problem illustrated by (16) — compare, for example:

- (19) i *Who went out and left the gate open?*
- ii *No one had received an invitation and not accepted it*
- iii *Few people go to bed at nine and read until twelve*
- iv *Only one of them had both seen the problem and solved it*

(Example (i) is taken from my review, p. 146, n. 8.) These and innumerable others like them cannot plausibly be regarded as metaphors for conditionals, but again their meaning is quite different from that of examples where the alleged ellipsis is filled out: *Who went out and who left the gate open?*, *No one had received an invitation and no one had not accepted it*, and so on. Note, moreover, that even the autonomous syntax approach fails for examples like the last: one cannot reinstate the allegedly elided subject because of the *both*: **Only one of them had both seen the problem and only one of them had solved it* or **Both only one of them had seen the problem and only one of them had solved it*. *Both-and* is not used to mark coordination between clauses (except, irrelevantly to this case, certain kinds of subordinate clause). There is thus both syntactic and semantic evidence that the coordination here is not between the immediate constituents of the sentence. Examples like (16) present a major problem for rank theory, and Matthiessen & Martin's proposed solutions do not work.

2.5 Hypotaxis and rankshift

IFG makes a distinction between two kinds of clause subordination illustrated in, for example, (20i) vs (ii) (*IFG*:247):

- (20) i *He insisted that they had to wait in line*
- ii *He resented that they had to wait in line*

In (i) the underlined clause functions as β in a hypotactic clause complex: *he insisted* constitutes a clause functioning as α in that complex. In (ii) the underlined clause is embedded/rankshifted to function as Complement (more precisely — see below — as Head of a nominal group which functions as Complement) of the main clause: *he insisted* does not constitute a clause by itself. With hypotaxis, the subordinate clause is not part of, not a constituent of the superordinate clause, whereas with embedding it is. §2.2 of my review questioned the way Halliday explains and applies the distinction between two kinds of subordination.

In their response, Matthiessen & Martin again misrepresent what I said. In §1.1 they have me rejecting the 'differentiation of hypotaxis vs embedding instead of [having] only "subordination"'. But it is absolutely clear from my discussion that I am not proposing a single undifferentiated relation of subordination: the last paragraph of my §2.2 discusses how a 'comparable distinction' can be made in non-rank grammars — a distinction 'that works much more satisfactorily because it is not based on rank' [Misrepresentation₄]. Notice, moreover, that in my 1984 textbook (which Matthiessen & Martin refer to several times) the introductory section of the chapter on coordination and subordination (pp. 378-381) explicitly makes the point that not all clause subordination is a matter of embedding: I distinguish the embedding subordination of (21i) from the non-embedding subordination of (ii):

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- (21) i *Liz prepared the food that they had ordered*
ii *Ed liked it, whereas Max thought it appalling*

I remarked that of the two, embedded subordination is the more sharply distinct from coordination, the existence of the non-embedded subordination reflecting 'the familiar phenomenon whereby the distinction [in this case, between coordination and subordination] is partially gradient rather than sharply polarised'. In my review, the distinction is illustrated by

- (22) i *She has gone home early because she had an appointment with her solicitor*
ii *She has gone home early because her light is off*

I suggested that in (i) the underlined clause enters into construction with *gone home early* (i.e. is embedded), whereas in (ii) it enters into construction with *she has gone home early* (i.e. it is an IC of the whole sentence, thus not embedded). It is therefore quite wrong for Matthiessen & Martin to say that I 'seem to espouse' the position that 'all of Halliday's hypotactic clauses' are embedded (§II.2.2) [Misrepresentation₅].

I also reject Matthiessen & Martin's claim at the beginning of §II.2.2: 'This topic is very important but the issues Huddleston brings up under this heading are DESCRIPTIVE ones and have nothing to do with Halliday's THEORY of rank: Huddleston's preferred analysis is a descriptive analysis and could be accommodated within Halliday's theory without any problems. It is thus a source of confusion to include the discussion as part of the general discussion of the theory of rank'. (Note, in passing, the derogatory implicature, triggered by the initial 'very important but', that I have missed the main issues.) Contrary to what Matthiessen & Martin claim, rank THEORY is crucially involved because, although it allows 'downward rankshift', it does not allow 'upward rankshift' (which is inconsistent with the total accountability requirement). The possibility of downward rankshift enables us to have the *because*-clause in (22) occurring at different places in the rank-based constituent hierarchy: in (ii) there is no rankshift and hence the subordinate clause is an IC of the whole sentence (which here has the form of a clause complex), but in (i) it is rankshifted and hence appears lower in the constituent structure. But the ban on upward rankshift means that we cannot handle (23) in the same way:

- (23) i *They had already read the report very carefully*
ii *They had already read the report, rather surprisingly*

Rank theory does not allow us to assign to (23ii) a bracketing like that of (22ii), i.e. one where the ICs are *they had already read the report* (a clause) and *rather surprisingly* (a group): it would violate the principles of the theory for a sentence to consist immediately of a (non-embedded) clause and a group. Rank theory requires that both sentences in (23) have the form of a clause and that the underlined groups be ICs of the clause, i.e. be at the same place in the constituent hierarchy. This point is made on pp. 147-148 of my review. In response, Matthiessen & Martin say (patronisingly): 'This claim is presumably an oversight on Huddleston's part; the two clearly do not appear at the same place in the constituent structure: *very carefully* is a circumstance of Manner in the experiential constituency structure of the clause (and consequently a circumstantial Adjunct), whereas *rather surprisingly* is a Comment Adjunct in the interpersonal constituency structure of the clause'. There was no oversight on my part. I was not suggesting that Halliday does not differentiate at all between (23i) and (ii): my concern was with the rank constituent structure (bracketing), not the assignment of functions (labelling) — and what I said was quite correct [Misrepresentation₆].

Thus when Matthiessen & Martin say 'the issue is not whether there are dependent clauses hypotactically related to dominant ones in clause complexes (contrasting with rankshifted ones) but where to draw the boundary between dependent clauses and rankshifted ones', they are begging the question. A less tendentious way of putting it would be to say 'the issue is not whether there are two kinds of subordination, but where to draw the boundary between them'. In Systemic-Functional theory a major factor in determining where to draw the boundary is rank.

The fact that downward rankshift is allowed means that rank is not THE determining factor — but it is nevertheless a very important one. This is especially so in Halliday's grammar of English — more, for example, than in that of Hudson 1968, inasmuch as Halliday makes less use of rankshift.⁵ Since the rank of an item is very much a matter of its internal structure, it is strange, as I remarked on p. 145 of the review, that a theory emphasizing its functional orientation should give so much weight to rank in drawing the boundary between the two kinds of subordination. A good deal of my discussion of the hypotaxis-rankshift distinction was therefore devoted to showing functional similarities between groups and some of Halliday's hypotactic clauses.

There is not space to take up all these similarities in detail again. I will instead focus primarily on coordination, as illustrated in

- (24) i *He left before the debate*
- ii *He left before the vote was taken*
- iii *He left before the debate or (at least) before the vote was taken*

The coordination in (iii) is indicative of a functional similarity between *before the debate* in (i) and *before the vote was taken* in (ii), but IFG misses this by treating the former as an Adjunct in clause structure, the latter as a β element in clause complex structure. Even if we could accept Matthiessen & Martin's claim to be able to handle (iii) in a way which does not necessitate any amendment to Halliday's analysis of (i) and (ii), it would still count against that analysis that it fails to bring out this functional similarity.

Let us now examine how Matthiessen & Martin propose to handle (iii). They in fact have no firm position and make no reference to any Systemic-Functional literature, so that one wonders whether the problem has been carefully worked on by systemic grammarians — certainly the two proposals they do put forward do not appear to have been thought through very thoroughly. They are presented in a single sentence: 'Either we can treat the clause *before the vote was taken* as rankshifted in this environment, serving as the extending element in a group/phrase complex or we can analyze the example as an elliptical clause complex: *He left before the debate or [he left] (at least) before the vote was taken*'. Although both analyses are said to be 'theoretically possible', they both in fact raise important theoretical problems. The first is inconsistent with the theoretical explanation of the distinction between multivariate and univariate structures given in 'TS' (39-40): "Rankshift" is the device by which multivariate structures, which are not themselves recursive, are enabled to operate recursively. ... Rankshift ... is ... a property of certain constituents entering into certain structures that are themselves of the multivariate type'. This explicitly excludes the rankshifting of a constituent into a univariate structure, which is what Matthiessen & Martin are proposing. To abandon that constraint would undermine the theoretical distinction between rankshift and layering/nesting: rankshift applies in multivariate structures, layering/nesting in univariate ones. Contrary to what they say, therefore, Matthiessen & Martin's first proposal is not 'theoretically possible': it requires a relaxation of constraints that Halliday has built into the theory. There are also descriptive issues related to it that Matthiessen & Martin would need to address before claiming to have solved the problem raised by (24iii). One concerns the constraints on possible coordination. Given that, for IFG, *before the debate* in (i) and *before the vote was taken* in (ii) have different functions, why is it that they can be coordinated? The account will need to differentiate between (24) and, say, (25):

- (25) i *They were vindictive, in her opinion*
- ii *They were vindictive, I think*
- iii **They were vindictive, in her opinion and I think*

Under what conditions can constituents of unlike function be coordinated? The second descriptive issue concerns the alleged arguments for saying that *before the vote was taken* is hypotactic, not rankshifted, in (24ii): it would need to be shown that the arguments against treating *before the vote was taken* as rankshifted in (ii) do not apply in (iii). We shall see below that this cannot be done.

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Consider then Matthiessen & Martin's alternative proposal, ellipsis. This too cannot be sustained. We have already seen the problems that arise when non-elliptical subclausal coordination is treated as elliptical clausal coordination, and they apply just as much in the present context as in that in which they were introduced in §2.4 above:

- (26) i *No-one left before the debate*
- ii *No-one left before the vote was taken*
- iii *No-one left before the debate or before the vote was taken*
- (27) i *One member was making jokes during the debate*
- ii *One member was making jokes while the vote was taken*
- iii *One member was making jokes both during the debate and while the vote was taken*

No-one left before the debate or no-one left before the vote was taken doesn't have the same meaning as (26iii), and **One member was making jokes both during the meeting and one member was making jokes while the vote was taken* not only differs in meaning from (27iii) but is syntactically ill-formed. Notice, moreover, that such coordination is not restricted to what Matthiessen & Martin call 'reified processes like *debate*'. The same problems arise in *No-one saw her before nine or after she went off on her bicycle*, where their metaphor analysis would be inapplicable. Contrary to what Matthiessen & Martin allege, therefore, the phenomenon illustrated in (24iii) does provide 'damaging' counter-evidence to the IFG analysis of (i) and (ii).

Let us turn now to the argument Matthiessen & Martin advance for treating *before the vote was taken* in (24ii) as hypotactic rather than rankshifted. This analysis is said to bring out the relation between (24ii) and the paratactic

- (28) *He left and then the vote was taken*

More specifically, it 'allows us to explain why *before the vote was taken* is immediately accessible to argumentation in discourse just as a paratactically related clause would be'. Thus if speaker A said (24ii) or (28), B could in either case respond with *Was it?* (see Matthiessen & Martin's examples (43)-(44)). Rankshifted clauses, by contrast, 'are not discourse accessible in this way'. Again the discussion is far too brief to count as a serious argument. What is the 'explanation' that the hypotactic analysis enables us to give? The implication is that hypotactic clauses are 'discourse accessible' just as paratactically related ones are, whereas rankshifted ones are not. But Matthiessen & Martin must know that this generalisation is not even approximately true.

Even with these particular sentences, (24ii) and (28), *was it?* is a much more natural response to (28) than to (24ii), contrary to what Matthiessen & Martin claim by saying 'just as in'. *Before* is commonly included among the set of presupposition triggers for English (cf. Keenan 1971, Levinson 1983, etc.): it is not necessary to go into the issue of what precisely is meant by presupposition to see that the communicative status of *the vote was taken* is different in the two examples. In a natural utterance of (28) the speaker asserts that the vote was taken, but this is not so with (24ii) — hence the possibility of the speaker adding *was it?* as a tag in (28) but not in (24ii). One factor involved in presupposition is precisely that a presupposed proposition is less accessible to challenge than an asserted one. We need make only a small change to the examples for the difference to emerge quite dramatically:

- (29) i A *The bell was rung before the vote was taken*
- ii B *Was it?*
- (30) i A *The bell was rung and then the vote was taken*
- ii B *Was it?*

In (29) *was it?* can only question the superordinate clause: it asks whether the bell was rung (before the vote); to question the content of the subordinate clause is quite marked and hence

would require a much more explicit form, such as *Was the vote in fact taken?* In (30), by contrast, the natural interpretation of *Was it?* is that it questions the second clause: "Was the vote taken?"

Let us change the examples a little further, moving into the future time sphere:

- (31) i *I'll ring the bell before the vote is taken*
 ii *I'll ring the bell and then the vote will be taken*

In (ii) *will it?* questions the second clause, but in (i) *is it?* is not an acceptable response: there is a very sharp contrast in discourse acceptability between questions challenging the second clause in these two examples. The fact that in (i) we have *is* but in (ii) *will* is highly relevant to this difference, but I will defer development of that point until §5.3, when we come to look at the relation between tense and modality.

(31i) is indeed very similar to one used in Halliday & Hasan (1976:196) to illustrate the LACK of discourse accessibility in rankshifted clauses. There they expressed it not as 'discourse accessibility', but as the ability to serve 'as the target of presupposition from another sentence' (in a different sense of 'presupposition' from that in which I used it above): they presented it, as Matthiessen & Martin appear to be doing now, as 'the basis of the distinction between hypotaxis and rankshift', with the following as illustrative examples:

- (32) i *I shall stay in the city when I retire this year*
 ii *I shall stay in the city, even though I retire this year*

Do you? (= "Do you retire this year?") is an acceptable response to (ii) but not to (i), which is therefore in the 1976 work treated as rankshifted. The discussion of the contrast between hypotaxis and rankshift in *IFG* does not give this as the basis, and there is no reason to think that *IFG* would treat (32i) as rankshift — it does not fall into any of the types of rankshift given in Table 7(8) (p. 220), which is said to be exhaustive. And certainly the long list of hypotactic examples on p. 215, for example, contains several that do not pass the test of discourse accessibility.

Note, moreover, that contrary to what Matthiessen & Martin assert so uncompromisingly, it is possible to challenge the content of clauses that *IFG* treats as rankshifted:

- (33) A *He much regrets that he offended your mother*
 B *But he didn't: she wasn't put out at all*
 (34) A *They are going to charge everyone who uses the photocopier for private purposes*
 B *But no one does*

It would be irrelevant to point out that B's response here is of a slightly different form from that in the examples we have been considering: it has a broadly similar effect of challenging what A has said. Matthiessen & Martin are claiming to offer an explanation, not a mere classification. (And in any case, *But did he?* can hardly be excluded as a response in (33).) As things stand, they have provided neither a criterion for distinguishing hypotaxis from rankshift, nor an argument for treating (24ii) as hypotaxis rather than rankshift. Notice, moreover, that there is no difference in discourse accessibility between (24ii) and (iii) — so if there were an argument here against rankshift in the former it should also count against it in the latter, contrary to one of their proposals for handling this coordination (see above).

2.6 Open interrogatives and hypotaxis

I pointed out in my review that Halliday does not mention the construction illustrated in

- (35) *What does she think he used?*

This is an open interrogative ('wh' or content interrogative in *IFG* terminology) where the interrogative element has a function within a hypotactic clause. I mentioned this in my introduction, in the context of pointing out the 'massive difference in the aspects of English that interest Halliday and those that occupy those working within, say, a Chomskyan framework'. (I should add — since Matthiessen & Martin fail to do so — that my discussion of this difference noted equally that Halliday covers topics that are unjustifiably neglected in the Chomskyan literature.) Matthiessen & Martin begin their response (§II.1) by saying: 'it is important to note what lies behind the difference [sc. in the attention given to the permitted roles of the interrogative element by Halliday and Chomsky] (Huddleston doesn't, except to suggest that it is a matter of personal interest).' The parenthesis is not true: I mention the very same point as they do, namely that it is related to Halliday's focus on textual analysis — 'Admittedly the problem of preventing the generation of deviant strings does not arise when the attention is on textual analysis' (this actually occurs in the passage they quote!) [Misrepresentation7].

Matthiessen & Martin question whether the omission is significant — 'examples such as [(35)] are actually not very common in natural text (such as the London-Lund corpus)'. Does the reference to the London-Lund corpus mean that they have examined it to ascertain the frequency? If not, it is misleading to mention it; if so, why do they not give us the statistical information? The count should of course not be restricted to open interrogatives: exactly the same issues arise with exclamatives, relatives and the marked theme construction (*The others she thinks we can forget about*).

The reason I suggested it was a significant omission is that the construction 'present[s] problems for the hypotaxis analysis' (p. 139), the issue we have been discussing in the last section. If (35) is a hypotactic clause complex, then what is the domain of the system Halliday calls 'mood' — the system contrasting declarative, interrogative and so on? *IFG*, like earlier work, presents this as a system of the clause, but in (35) it would seem to be the whole clause complex that is interrogative. In paratactic complexes, by contrast, it is the individual clauses that select for mood, as is evident from examples like:

(36) *I'd like to buy them a present, but what can I buy that they haven't got already?*

Here the clause at 1 is declarative, while that at 2 is an open interrogative, and no mood can be assigned to the complex as a whole. (We have here further evidence against Matthiessen & Martin's claim that similarity to parataxis supports the analysis of examples like (24ii) as involving hypotaxis rather than rankshift.)

Matthiessen & Martin do not address this issue, and though they devote several pages to the discussion of (35), they conspicuously fail to provide a (congruent) analysis of it. Instead they approach it via Halliday's idea of a metaphor of modality: they reword it as (37) [their (27)]:

(37) *What in her view did he use?*

and then declare that here the wh-element is 'one or another of the three functions Subject, Complement or Adjunct'. I had quoted this phrase from *IFG* (p. 83), observing that all Halliday had to say on the question of the possible role of the wh-element was that it was always conflated with one of these three elements. In repeating the quotation in the present context they imply that Halliday's account can stand: that contrary to what I had suggested it does cover (35) and the like.

There are two flaws in this argument. The first is that the applicability of Halliday's idea of a metaphor of modality to examples like (35) is somewhat suspect. Halliday himself writes of this kind of metaphor (*IFG*:332-333): 'the speaker's opinion regarding the probability that his observation is valid is coded not as a modal element within the clause, which would be its congruent realization, but as a separate projecting clause in a hypotactic clause complex. To the congruent form *it probably is* so corresponds the metaphorical variant *I think it is so*, with *I think* as the primary or "alpha" clause.' He goes on to distinguish between subjective modality

(e.g. *I think*) and objective modality (e.g. *it is likely*) — but the latter does not cover *she thinks*. Indeed Halliday explicitly contrasts

- (38) i *I think it's going to rain*
 ii *John thinks it's going to rain*

such that (i) is a 'metaphorical variant' of *It's probably going to rain* and 'not a first person equivalent of *John thinks it's going to rain*, which does present the proposition "John thinks". The distinction is reflected in the tags — *isn't it?* for (i) and *doesn't he?* for (ii). Matthiessen & Martin's analysis ignores this distinction — though it is significant that when they do offer a metaphorical analysis they change the example from (35) to *This I think Oscar feels also* (see their Figure 2), which does fit in with Halliday's concept of modal metaphor. The applicability of this kind of analysis is even more questionable with examples like

- (39) *What did she allege that I had tried to persuade you we should do?*

Matthiessen & Martin have clearly not addressed the question of whether the solution they propose is sufficiently general to handle the phenomenon at issue.

The second flaw is that Halliday makes it quite clear that he does not regard the metaphorical analysis as sufficient by itself. In the section entitled 'the representation of metaphorical forms' (IFG:324-325) he argues that neither the face-value analysis nor the metaphorical analysis is 'satisfactory by itself'. The former is unsatisfactory because it does not properly bring out the meaning, the latter because it ignores the fact that the reworded version is not what the speaker actually said. To analyse (35) by means of a labelled box diagram of (37) ignores the fact that the speaker did not say (37) but (35). Halliday's metaphorical analyses are intended to supplement the face-value ones, not to supplant them. Matthiessen & Martin's discussion of (35) simply avoids this issue. Even if the above doubts concerning the applicability of the modal metaphor analysis of (35), (39) and the like can be resolved, we still need a face-value analysis — and for this purpose the statement that the wh-element is always conflated with Subject, Complement or Adjunct is not adequate. And of course structural representations of individual examples presuppose a system network that will generate them. Hence my question concerning the domain of the mood system.

2.7 Nominalization

The IFG analysis of

- (40) *Why she did it remains a mystery*

has *why she did it* functioning as Head of a nominal group which in turn functions as Subject of the main clause. I questioned this analysis in my review (p. 143), suggesting that it would be better to treat it (immediately) as Subject in clause structure. This IS a purely descriptive matter, and I relegated it to a footnote. Matthiessen & Martin take it up and accuse me of misrepresenting Halliday (end of §II.2.2) — because I do not mention that Halliday says (in a footnote!) 'Where [as in (40)] the embedded element functions as Head, we may leave out the intermediate (nominal group) step in the analysis and represent the embedded clause or phrase as functioning directly in the structure of the outer clause, as Subject or whatever; it does not affect the status of the embedded element as as nominalization.' There is not the slightest misrepresentation of Halliday in what I said. The second sentence of the quotation makes clear that his proposal is purely a matter of notational simplification, with no bearing on the analysis. But it was the analysis I was arguing about, not the notational representation: why should I bring in a point about notation when Halliday himself says it doesn't affect the analysis? For Matthiessen & Martin to say that I am here misrepresenting Halliday is, rather, to misrepresent me [Misrepresentations].

Matthiessen & Martin then go on to defend Halliday's analysis. And as so often they express themselves in such a way as to implicate a failure on my part to see the obvious: 'The justification Huddleston cannot see is, of course, the agnation between [(40)] and [(41)]:

(41) *The reason why she did it remains a mystery*

This is not a valid response to my argument that the embedded clause in (40) 'cannot enter into construction with any of the Pre- or Postmodifiers that are found in genuine nominal groups'. There are two reasons why this is so. The first is that we are not dealing with the same *why she did it* in the two examples: in (40) it is an open interrogative clause, but in (41) it is a relative clause. The second reason is that *why she did it* does not have the same function in (41) as it is alleged to have in (40), namely Head: *the reason why she did it* is not a genuine expansion of *why she did it*. This is why the distribution of the expressions is so different, as is evident from the contrast between countless pairs such as *The reason why she did it wasn't what she said it was* vs **Why she did it wasn't what she said it was*. One such difference mentioned in my review but not taken up by Matthiessen & Martin concerns extraposition and interrogative formation:

- (42) i *Why she did it is now known*
 ii **Is why she did it now known?*
 iii *Is it known why she did it?*
 (43) i *The reason why she did it is now known*
 ii *Is the reason why she did it now known?*
 iii **Is it now known the reason why she did it?*

(The asterisk in (43iii) applies to the extraposition reading, not the one involving what Halliday (1981b:23-24) calls the substitute theme construction.) If *the reason why she did it* were simply an expansion of *why she did it* we would have no explanation as to why the substitution of the former for the latter should have these effects on the distribution; under my analysis they are attributable to the distinction between a subject realized by a clause and one realized by a nominal group (for a more detailed discussion, covering 'gerundive' constructions too, see Huddleston 1984:312-317). For their argument to carry weight, moreover, Matthiessen & Martin would need to demonstrate that more significance attaches to the relation between (40) and (41) than to that between (i) and (ii) in

- (44) i *I don't know why she did it*
 ii *I don't know the reason why she did it*

where *why she did it* is not rankshifted at all in (i).

2.8 Adverbial and adjectival groups

IFG has three classes of the unit group: nominal, verbal and adverbial. Nominal and verbal groups have both experiential and logical structures, whereas the adverbial group has only a logical structure. Logical structures are represented as $\alpha \beta \gamma$, etc., as in the clause complexes discussed above, and indeed the logical component is said to define complex units (p. 159). The question then arises as to when something with the structure $\alpha \beta (\gamma \dots)$ is a complex unit, and when it is a ranking unit. A sufficient condition for it to be a ranking unit is that it simultaneously have a multivariate structure, as with nominal *those two splendid old electric trains* or verbal *has been eating*. The discussion of sentence and clause complex that we have already examined (§2.3) says that there is no multivariate structure above the clause and so 'the notion of "clause complex" enables us to account in full for the functional organization of sentences' (p. 193): this suggests that the condition that there be simultaneously a multivariate structure might be a necessary as well as sufficient condition for us to recognize a ranking unit. It is not treated as a necessary condition, however, for this would mean that there would be no adverbial group. I suggested in my review that the recognition of the adverbial group was motivated solely by the total accountability requirement, and that there might therefore be

something to be said for relaxing total accountability in such a way that it didn't apply in places where there was no multivariate construction. This would in effect be making the requirement of a simultaneous multivariate structure into a necessary as well as sufficient condition for assigning an $\alpha\beta$ construction to a ranking unit. Matthiessen & Martin reject this suggestion, but they do not address the question of providing some principled basis for deciding when an $\alpha\beta$ structure should be assigned to a ranking unit and when to a complex unit. Why, to take the example from my review (p. 143) is *very gently* merely a word complex in *the very gently simmering stew* but a group in *The stew was simmering very gently*?

One of the reasons they give for treating *very gently* as a group in the second example, where it is functioning as Adjunct in clause structure, is that in this position there is greater scope for expansion in Adjunct position than in the *very gently simmering* construction: for example, one can have *The stew was simmering more gently than the soup* (§II.2.1 (ii)). This, however, raises the question of why *more gently than the soup* is a purely univariate construction. And indeed Matthiessen & Martin's wording of their point is suggestive here. They say that the adverbial group 'can have a rankshifted "qualifier" just like the nominal group', but 'qualifier' is the name of an element in the experiential, multivariate structure of the nominal group: if the adverbial group can contain a qualifier, why doesn't it too have a multivariate structure? Matthiessen & Martin might say that it was a minor terminological slip, and that they really meant 'Postmodifier'. That would not have any bearing on the important question of how we in fact distinguish between multivariate and univariate structures. Let us take a somewhat simpler example with just two elements of structure, such as *sooner than we expected*, as in *It finished sooner than we expected*. What grounds are there for saying that this is a purely univariate structure? In discussing the issue of how to handle structure above the clause, Halliday contrasts it with what we find in the nominal group (IFG:192): 'there is [in the sentence] nothing like the structure of the nominal group ..., where the elements are (i) distinct in function, (ii) realized by distinct classes, and (iii) more or less fixed in sequence. A configuration of such a kind HAS to be represented as a multivariate structure.' Does not *sooner than we expected* have all three of these properties? As for (i), *sooner* is Head and *than we expected* is Modifier; as for (ii), *sooner* is an adverb, *than we expected* a (rankshifted) clause; as for (iii), the sequence is absolutely fixed. Why then is it not a multivariate construction? Again, it seems to me that the distinction between multivariate and univariate constructions, which is of great significance to the theory of rank, is in need of much more rigorous explication.

I suggested in my review (p. 144) that it was puzzling that Halliday recognizes an adverbial group, but not an adjectival group, given that 'adjective-headed expressions display a richer structure than adverb-headed ones'. Matthiessen & Martin's response to this is short and merits quotation: 'The simple answer is that of course there is an adjectival group; it is a kind of nominal group, just as a "substantival" group is a kind of nominal group, with a "substantive" as Head: Huddleston's puzzle is just a matter of delicacy.' The reader will note that 'simple' and 'of course' again trigger an implicature of dim-wittedness, of failing to see the obvious. The 'of course', however, is totally out of place: nowhere in IFG does Halliday mention a distinction of delicacy between adjectival and substantival groups, and he consistently refers to what Matthiessen & Martin are telling us are adjectival groups as 'nominal groups' (e.g. p. 252, where *plain or with cream* is given as a complex consisting of a nominal group and a prepositional phrase). As for 'simple', that the answer may be, but it achieves its simplicity by completely ignoring the point I was making about the difference in treatment of adjectival and adverbial groups. In *The stew was simmering very gently* the *very* and *gently* are ICs of the adverbial group, but in *They were very gentle* the *very* and *gentle* are not ICs of the (adjectival) nominal group. Because it is a nominal group it has a nominal group structure, and *very* and *gentle* together function as Epithet in the multivariate structure. That is, the nominal group consists immediately of a single element of structure, which is realized by a word complex. Unlike *very gently*, *very gentle* is treated alike when it occurs in prenominal function (*very gentle people*) and when it occurs without a following Head (*They were very gentle*): this is the difference I find puzzling, and delicacy has nothing to do with it.

2.9 Minimal bracketing and typology

Matthiessen & Martin's concluding remarks on the contrast between minimal and maximal bracketing approaches to constituency (end of §II.2.3) raise the issue of the typological distinction between SVO and what have been called 'non-configurational' languages such as Warlpiri. They write: 'Even if the maximal bracketing of the IC tradition turned out to be the most useful model for constituency in English, it has proved quite inappropriate for many other languages, whereas Halliday's minimal bracketing model is generally applicable precisely because it does not impose constituency where there is none'. But there is no compulsion in non-rank grammars to impose constituency where there is none — no compulsion, in particular, to impose a VP category on all languages. Matthiessen & Martin have too narrow a conception of non-rank grammars, reflected — as I suggested in §2.1 — in their interpretation of the term 'IC analysis'. Contrary to what Matthiessen & Martin claim, the typological contrast between languages with and without a VP category provides evidence against rank grammar, not for it. At the beginning of their §II.2.3 they themselves say that 'perhaps the most immediately striking difference' between rank grammar and what they call IC-analysis is that in rank grammar 'there is no VP'. But there is no more reason to reduce all languages to a flattened out structure, as rank grammar does, than there is to impose a binary NP-VP structure on all languages (as Matthiessen & Martin are implying non-rank grammars necessarily do). If — to return to the above quotation — 'the maximal bracketing of the IC tradition turned out to be the most useful model for constituency in English', then our theory should allow that analysis, but rank grammar doesn't (by virtue of having no VP): the fact that the VP category is not appropriate for all languages is no reason for excluding it from our analysis of those languages where it is appropriate. To do so, as rank grammar does, obscures the difference between the two types of language.

3. Theme

3.1 Questions concerning Halliday's concept of Theme

The function Theme is used in talking both of linguistic expressions and of non-linguistic entities. For example, in

(45) *Velan was perfectly contented and happy*

(from Matthiessen & Martin's (113)) Systemic-Functional Grammar might say either that the Theme was *Velan*, a nominal group, or that the Theme was "Velan", a person. To avoid possible confusion between these different facets of Theme, I shall refer to them as 'Theme_E' (Theme considered as expression) and 'Theme_C' (Theme considered as content), and in talking about the Themes of particular examples I shall, as I have just done for (45), give Theme_E in italics and Theme_C within double quotation marks. Theme_E may be either simple or multiple: a simple Theme_E corresponds to a single element in the rank-based constituent structure of the clause, a multiple Theme_E to more than one. We can now formulate the following questions that arise with Halliday's thematic analysis of clauses:

- (46) i How is Theme_E determined?
 ii What does it mean to say of some "x" that "x" is Theme_C?
 iii How is Theme_C determined, and what is the relation between Theme_C and Theme_E?
 iv What is the constituent structure assigned to a clause with multiple Theme_E?
 v What is the nature of the empirical evidence supporting the analysis?

3.2 Determination of Theme_E

For English, Theme_E is identified, as a first approximation, as the first element of the clause. There are other languages, however, where it is marked in quite a different way, e.g. by some kind of particle. Halliday (*IFG*:38) gives Japanese -wa as an example of such a marker, while Matthiessen & Martin (§II.3.1) cite Tagalog as a language 'where Theme is marked by a particle rather than position and tends to appear last in the clause, unless marked'. This is an important difference, though it receives no more than a passing mention in *IFG* and Matthiessen & Martin: it raises the question of how far the element marked by a particle in Japanese or Tagalog is in fact identifiable with the Theme of *IFG*'s analysis of English — I will return to this question briefly below.

To say that in English Theme_E is identified as the first element in the clause is only an approximation because there are two complicating factors: (a) A multiple Theme_E covers more than what is the first element in the rank constituent structure; (b) Not all clauses have a Theme_E.

The elaboration Halliday gives to cater for multiple Theme_E is along the following lines (*IFG*:53, 56):

- (47) i If the initial element in the clause does not function as Subject, Complement or CIRCUMSTANTIAL Adjunct ([which] embraces all Adjuncts other than conjunctive and modal ones), then the Subject, Complement or Adjunct next following is still part of the Theme
- ii The Theme of any clause ... extends up to (and includes) the topical Theme. The topical Theme is the first element in the clause that has some function in the ideational structure

Although this aspect of (46i) was not a major focus of my review, I did mention the problem for the account just given created by such a pair as

- (48) i *Wouldn't the best idea be to join the group?*
 ii *Isn't the best idea to join the group?*

In (i) Halliday takes the topical Theme_E to be *the best idea* (cf. *IFG*:55 [I have simplified his example in irrelevant ways]), which is consistent with the definition since *wouldn't* is not, by itself, an element with an ideational function. But in (ii) *isn't* does have an ideational function (it is simultaneously interpersonal Finite and ideational Process), and hence satisfies the definition of topical Theme_E. Matthiessen & Martin respond by saying that *isn't* in (ii) is selected as Theme_E qua Finite, not qua Process, and hence is not topical Theme_E: *the best idea* is thus topical Theme_E in both cases, as shown in their Figure 13. I have no doubt here that this does represent Halliday's intention, but the fact remains that *isn't* satisfies the definition of topical Theme_E given in (47ii) — it is significant in this respect that their analysis in Figure 13 does not show the ideational functions. Why, then, do Matthiessen & Martin present it as a matter of my seeming 'to have missed this point entirely', instead of acknowledging that I have drawn attention to the need for (47ii) to be corrected, refined? [Misrepresentation₉]. Note that it is not as though (47ii) were an initial statement made in the context of a discussion of declarative clauses: it comes AFTER the discussion of interrogatives.

A second problem for (47ii) concerns the existential *there*-construction, as in *There is nothing more he could do about it*, where Halliday takes *there* itself as topical Theme_E. In my review I questioned this analysis with respect to (46ii-iii), and will take it up in that context below, but it is worth noting here that it too conflicts with (47ii). Halliday himself says quite explicitly (*IFG*:130) that this *there* has no representational [ideational] function — see also the ideational analyses in Figure 5-22, where no function is assigned to it. It does not therefore satisfy the definition of topical Theme_E given in (47ii), even though it is analysed as such: I don't think there is any doubt that its lack of ideational function is closely associated with the problem of

interpreting it as the realisation of a topical Theme_C. (47ii) also needs refining to cater for clauses with no Theme_E.

3.3 Theme_C and its interpretation

The main focus of my criticism of Halliday's concept of Theme in my review related to (46ii-iii)—more specifically to his claim that Theme_C is what the clause is about. Matthiessen & Martin regard it as one of the 'fundamental problems of [my] critique' that I '[narrow] the notion of theme to the notion of topic — "aboutness"; "aboutness" is most closely associated with the topicality subtype of thematicity, i.e. with ideational Themes'. But there are three very good reasons for focussing on the relation between 'aboutness' and Theme. In the first place, the concept of what a clause, sentence or utterance is about is one commonly invoked in the non-systemic literature — there is here the potential for some point of contact between Systemic-Functional scholars and others. Secondly, it is reasonable to regard the topical Theme as the prototypical or most readily graspable kind of Theme: if we can't get clear on what Halliday means by topical Theme, we are not likely to get far in understanding textual and interpersonal Themes, or Theme in the more general sense that subsumes all three kinds. In this connection I would ask what kind of Theme is marked by the Japanese and Tagalog particles. Do these mark Theme in this general sense or, as I suspect, just topical Theme? Thirdly, the concept of what the clause is about or concerned with plays a highly prominent role in Halliday's explanation of Theme.

Let me substantiate this point. Theme is first introduced (under the provisional label 'psychological Subject') on p. 33 as 'that which is the concern of the message'. *This teapot my aunt was given by the duke* 'is a message concerning the teapot' (p. 33). 'Psychological Subject meant "that which is the concern of the message"' (p.33). 'In *this teapot my aunt was given by the duke*, the psychological subject is *this teapot*. That is to say, it is "this teapot" that is the concern of the message' (p. 34). 'The Theme ... is what the message is concerned with: the point of departure for what the speaker is going to say' (p. 36). 'The Theme is the element which serves as the point of departure of the message; it is that with which the clause is concerned' (p. 38). 'The Theme is the starting-point for the message: it is what the clause is going to be about' (p. 39). 'There is a difference in meaning between *a halfpenny is the smallest English coin*, where *a halfpenny* is Theme ("I'll tell you about a halfpenny"), and *the smallest English coin is a halfpenny*, where *the smallest English coin* is Theme ("I'll tell you about the smallest English coin")' (p. 39). 'So the meaning of *what the duke gave my aunt was that teapot* is something like "I am going to tell you about the duke's gift to my aunt ...". Contrast this with *the duke gave my aunt that teapot*, where the meaning is "I am going to tell you something about the duke"' (p. 43). And so on.

The concept of 'point of departure' or 'starting-point' also figures prominently; I focussed on the idea of what the clause is about, concerned with, on the grounds that it is not clear that these other concepts 'can sustain an interpretation that is independent of syntactic sequence' (compare Hudson's remark (1986:798) that if subordinating *that* is the point of departure it can only be in the sense of being the first element). It is in the context of quoting my remark concerning point of departure that Matthiessen & Martin mention Tagalog, suggesting that it is 'the iconicity of the realization of Theme in English that creates problems for the linguist at this point (a problem which would not arise in a language like Tagalog, where Theme is marked by a particle rather than position, and tends to appear last in the clause, unless marked)'. I question, however, whether this concept of point of departure is one that would naturally be used in describing Tagalog; if it were used, would it not itself be explained by reference to the concept of what the message is (primarily) about? Note that in talking of this concept of 'point of departure' Matthiessen & Martin write: 'There is good reason to think that if we can characterize the notion of "information flow", currently popular with a number of linguists, we will be able to relate the notion "point of departure" to it'. But this is a matter of future research, not current understanding; as things presently stand we have not been given a clear account of what 'point of departure' means that is independent of syntactic sequence, and — in the light of the above three

points — it is perfectly reasonable to examine the relation between Halliday's Theme and the concept of what the message is about or concerned with.

Let us begin with a clarification of terminology. Outside Systemic-Functional Grammar "what the message is about" is more often called 'topic' than 'theme' — cf. Reinhart (1981:55) 'The term "sentence-topic" is the Anglo-Saxon equivalent of the term "theme", coined by the Prague School of functional linguistics'. I made this point in a parenthesis in my review:

- (49) The interpretation of theme (or topic, as it is more often called) as what the clause is about is of course a familiar one — but it is surely not an interpretation that can be consistently associated in English with the initial element.

Matthiessen & Martin cite this passage in juxtaposition to the following quotation from *IFG* (p. 39):

- (50) Some grammarians have used the terms Topic and Comment instead of Theme and Rheme. But the Topic-Comment terminology carries rather different connotations. The label 'Topic' usually refers to only one particular kind of Theme [ideational or topical theme, CM & JRM]; and it tends to be used as a cover term for two concepts that are functionally distinct, one being that of Theme and the other that of Given.

They then comment: 'As the quotation from *IFG* shows, Halliday explicitly warns us against equating theme with topic, but this does not prevent Huddleston from doing just that'. This is another gross misrepresentation, implicating as it does that I have confused two distinct concepts and failed to heed Halliday's warning not to do so. The parenthesis is a terminological aside with absolutely no bearing on my argument; there is not the least confusion because my argument is concerned not with two concepts, but with a single concept going under two names, namely the concept of what the clause is about; and the definition of theme as what the clause is about is not surreptitiously brought in from the literature on topic but is given by Halliday himself — repeatedly, as the quotations above testify [Misrepresentation₁₀]. For the same reason I reject Matthiessen & Martin's earlier accusation (§1.5) that I engage 'in an unfortunate practice of glossing what Halliday says and then arguing about [my] own glosses rather than Halliday's original interpretation. ... this happens with Theme, which Huddleston glosses as "topic". 'Topic' is a standard term for what a message is about or concerned with, and as the quotations given above demonstrate, this is Halliday's original interpretation of Theme [Misrepresentation₁₁].

There are two factors involved in the terminological distinction Halliday draws in (50). One is that Topic is less general than Theme, corresponding only to the ideational subtype — hence his term 'topical Theme' for that subtype. Notice, here, that if there is any confusion of concepts in the debate it is to be found not in (49) but in Matthiessen & Martin's substitution of 'Theme' for the 'Topic' that appears in the Tagalog grammar they refer to, Schachter & Otanes 1972: I have found nothing in this work to suggest that their topic corresponds to Theme in general rather than just topical Theme. It should also be observed that when Halliday explains Theme in terms of what the clause/message is about he gives no indication that this is intended to cover only topical Themes. Nevertheless, I am quite willing to limit discussion of 'aboutness' to topical Theme, though this leaves us without any general account of Theme itself.

The second factor in (50) is that although there is some significant correlation between Theme and Given, they are distinct concepts and do not always match. This is undoubtedly an important point. It is echoed, for example, by Reinhart (1980:173): 'most of the studies mentioned above ... confuse the topic with the old information or the presuppositions of the sentence'; compare also Reinhart (1981:57): 'one [major approach to the definition of the relation topic-of] defines the topic as the expression whose referent the sentence is about. The other defines it as the expression representing old information'. But Halliday's point does not mean that topical Theme is inherently distinct from topic — defined, as in Reinhart's own approach, as what the sentence is about (cf. also Gundel (1985:84): 'what the speaker intends to communicate something about (the topic)', and so on) — for there is nothing inherent in the

concept of aboutness that limits it to what is given: presumably this is why Halliday feels able to use the term topical Theme for the ideational Theme, saying (p. 54) that 'it corresponds fairly well to the element identified as "topic" in topic-comment analysis'. Since I argue that Halliday's Theme_C, even when limited to topical Theme_C, is not always what the message is about, I shall use the term 'topic' in preference to Theme when I want to refer to the concept of what the message is about. I am not using it for 'two concepts that are functionally distinct', namely 'what the message is about' and 'given', but just for the first of these.

Granted that topic and given do not always match, we allow for topics that are not given, and given elements that are not topic. Plausible examples of these are respectively (51), an attested example cited in Ward & Prince (1991:170), and (52ii):

- (51) *Brains you're born with. A great body you have to work at*
 (52) i A. *What about Kim? What did she think of the idea?*
 ii B. *She didn't like it*

In (51) "brains" and "a great body" are not given, but are naturally regarded as topics, while in (52ii) "it" is given but not the topic — the topic is "she" (which is of course also given). But it is important to note that the mere fact that a given element is non-initial is not sufficient to establish that it is not topic. Consider the following possible responses, presented as (20) in my review (p. 159), to the question *What about the battery?*

- (53) i *It was O.K.*
 ii *There was nothing wrong with it*
 iii *I had to replace it*

"It" (= "the battery") is given in all three, but initial only in (i); nevertheless, I see no reason for denying that it is topic in all three. In particular, it would not be valid to dismiss the claim that it is topic in (ii) and (iii) as a confusion between topic and given: it needs to be demonstrated, not merely asserted, that the topic-expression must occupy initial position.

This brings us to my claim (49). The distinction between topical Theme and Theme in general in no way invalidates my objection: the weaker claim that the topical Theme_C is what the clause is about is still quite untenable. A fair number of the examples on the basis of which I disputed the claim that Theme_C is what the clause is about, including most of those in the early part of the discussion, did in fact involve topical Themes. Consider the following examples from p. 158:

- (54) i *Nothing will satisfy you*
 ii *You could buy a bar of chocolate like this for 6d before the War [spoken to someone who was not born before the War]*
 iii *There's a fallacy in your argument*

Matthiessen & Martin quote my observation that 'I can't make any sense of the idea that [these examples] are respectively about "nothing", "you" and "there"', yet the very long section on Theme in their response simply does not address this objection. They criticize me for using decontextualized examples. It is undoubtedly true that one could not give a satisfactory explanation of topic without using examples in context, but one doesn't need contextualized examples to demonstrate that the first element in a clause does not always identify what the clause is about, its topic. The examples in (54) are uncontentiously acceptable and natural, and simply don't need contextualizing to clarify their interpretation. At the end of their discussion of examples with initial negatives, Matthiessen & Martin write: 'The general point is arguably this: if Huddleston had gone through say twenty examples of negative Themes in running natural text, the issue would never have arisen'. Again there is the derogatory implicature — that my argument is superficial and that I could have saved everybody a lot of trouble if only I'd done some more work — but this implicature is quite without foundation. The 'issue' is whether (54i) is about "nothing", as Halliday's account predicts, and going through all the examples Matthiessen & Martin present does nothing to support the idea that it is about "nothing". One of the examples they give (a constructed one) is (55) [their (120)]:

(55) *Yesterday's market was disappointing. Not a single thing did I find to buy*

They say 'the thematic status of *not a single thing* ... would presumably not be questioned', but I certainly would question, and reject, the claim that the second clause has *not a single thing* as its topic expression, that it is about "not a single thing". An utterance of (45) by Max could be reported as *Max spoke about/of Velan, saying that he was perfectly contented and happy*, but there is no comparable way of reporting the second sentence of (55).

They begin their discussion of existentials with another derogatory implicature: 'it would seem that Huddleston's objection should have been forestalled by the work outside systemic linguistics on the presentative use of clauses of this type'. My objection was that (54iii) is not about "there", does not have *there* as its topic expression, and I do not believe there is any non-systemic work on existentials that argues that it is about "there". Why do Matthiessen & Martin not respond to my contention that (topical) Theme does not consistently identify what the clause is about? Do they maintain that it does? If so, they need to reconcile that claim with problematic examples like (54) (as I have said, their discussion of such constructions is not in terms of aboutness); if not, then we have a much more fundamental difference between topical Theme and topic than has been acknowledged, one which requires a major overhaul of the account of Theme given in *IFG*, as illustrated in the numerous quotations given above.

There is surely a vast difference between (54) and the elementary example with which we began, (45) (*Velan was perfectly contented and happy*). In the latter, we have said, the topical Theme_E is Velan and the topical Theme_C is "Velan", the person. The relation between Theme_C and Theme_E is that Theme_C is the referent of Theme_E, and the interpretation of the statement that the Theme_C is "Velan" is that the clause is about "Velan" (i.e. that in normal use it would be used to say something about "Velan"). The corresponding account cannot hold for (54), where the alleged Theme_E is not even referential. One of the things that makes the *IFG* account of Theme so unsatisfactory is that it introduces the concept via examples like (45) where the above account is readily comprehensible (e.g. *The duke gave my aunt this teapot*)⁶ and then extends it to radically different examples without acknowledgement or explanation of the differences.

I remarked of (54) that none of the alleged Themes could occur in the *as for* construction: **As for nothing, it will satisfy you*, etc. Matthiessen & Martin write: 'Presumably Huddleston's account shows why the examples don't work so we are entitled to ask what the account is. Unfortunately he doesn't present it'. My account is that topic is not systematically encoded in English (except in certain marked constructions, such as those with *as for*), so that an example like *My wife couldn't stand the dog* could be used with either "my wife" or "the dog" as topic, depending on the context — see pp. 158-159 of my review. This is not an idiosyncratic position; I referred there to Comrie (1981), but could have added other references, e.g. Reinhart (1981:58): 'which of the referring expressions of a given sentence counts as topic is determined, in most cases (i.e. except for sentences with a structurally marked topic), by its context of utterance'; or again Lyons (1977:505): 'we cannot say that *John* is the thematic subject [= topic expression, RDH] in the system-sentence *John ran away*: this system-sentence is in correspondence with several prosodically (and paralinguistically) distinct utterances, in some of which *John* would not be the thematic subject'.⁷ In (54), however, the alleged topical Theme_E is not even a potential topic-expression — which is why contextualization is unnecessary. And they cannot occur after *as for* because they are not referring expressions.

Matthiessen & Martin say that although *nothing* cannot occur with *as for*, it is 'perfectly fine' with *speaking of* or *about*, 'as long as it is not picked up referentially', as in (56) [their (123)]:

(56) *Speaking of nothing, have you paid the phone bill yet?*

There are three points to be made about this example. The first is that it can hardly be said to be uncontentionally natural: why do Matthiessen & Martin feel able to criticize me for using decontextualized examples and yet base an argument on one of their own like this? The second is

that, contrary to what they say, it is not 'perfectly fine' if we substitute *about* for *speaking of*. The third point is, most importantly, that (56) is not interpreted in the same kind of way as

(57) *Speaking of your father, will he be coming round next week-end?*

This IS about "your father"; a natural context is one where he has just or recently been mentioned. The participial clause provides a cohesive link with what has gone before, but although there is in this sense a continuity of topic the *speaking of* formula indicates that the comment is not connected with what has been being talked about. (56) is quite different: its topic is not "nothing", and *speaking of nothing* is not cohesive, but indicates that there is no topic-continuity. An important difference between *speaking of x* and *as for x* is that the former is clausal while the latter is not. This means that the former selects for polarity, and in (56) the clause is negative, just like *not speaking of anything (in particular)* — the fact that *nothing* incorporates a feature of clause negation is a major reason why it is not comparable to (57). *As* cannot fall within the scope of a negative: we can't say **not as for anything, ...*, and this is why there is no *as for nothing* comparable to (56): *as for* can only be used to specify the topic, and the fact that *nothing* cannot appear with *as for* is evidence that it is not a potential topic.⁸ Matthiessen & Martin suggest that examples like

(58) *As for the thirty rupees he bundled them into a knot at the end of his turban*

are not found very often — that the Complement of *as for* is more often antecedent for a pronoun in Subject function than in Complement function. This may be true, but it has no bearing on the issue, for Matthiessen & Martin accept that (58) is not ungrammatical or implausible: its status is not comparable to that of **As for nothing, it will satisfy you*, etc.

3.4 Theme in interrogative clauses

Let us turn now to the issue of Theme in interrogative clauses, beginning with the open ('wh' or 'content') subclass, as in

(59) i A *What's the new boss like?*
 ii B *She seems O.K.*

(where (i) is a question asked by speaker A, and (ii) is B's response). According to IFG the Theme_E of (i) is *what*. I described this analysis as 'very counter-intuitive' (p. 159), in that it requires us to say that the answer has a different Theme than the question: I would want to say that they have the same topic, namely "the new boss". Matthiessen & Martin criticize my appeal to intuition here, but it is worth noting that it can be supported by two observations. One is that a natural way of reporting A's utterance would be to say: *A asked B about the new boss*. The other is that in Japanese — which on my account encodes topic more systematically than English, and on Halliday's marks Theme by the particle *-wa* rather than by linear position — *-wa* would indeed be associated with "the new boss":⁹

(60) i A *atarashii bosu -wa dou desu ka*
 new boss TOP how be whether
 ii B *(kanojo -wa) mondai nai desu*
 she TOP problem no be

What explanation do Matthiessen & Martin have for the fact that my intuitions about English turn out to match what is encoded in Japanese? The reader will also have noted that Matthiessen & Martin claim that 'Huddleston at this point seems to be confusing thematicity with newsworthiness (Halliday's distinction of Theme-Rheme and Given-New)'. There is, however, no basis at all for this accusation. It is in (59ii) that the topic is given, and there is no disagreement between me and Halliday on this clause: he too would say that *she* is Theme. In (59i) I want to say that the topic expression is *the new boss*, which is new (Halliday's New

being marked prosodically): if I were confusing topic with given, I would not be saying this. [Misrepresentation₁₂]

Consider now Matthiessen & Martin's own account of (59). They say that the Theme of (i), obviously Theme_C, is 'the piece of information the speaker assumes the listener can supply, i.e. information that is recoverable for the listener although it isn't for the speaker'. There are three points to be made about this. Firstly, although they have criticized my appeal to intuition, they present not one shred of empirical evidence (or indeed any other kind of evidence) in support of this analysis: it is simply asserted. Secondly, it is quite unclear what they mean. What they treat as Theme of (59i) would also be Theme of

(61) *The piece of information that I assume you can supply, i.e. information that is recoverable for you although it isn't for me, is of vital importance to me*

I cannot believe that they intend to say that (59i) and (61) do in fact have the same Theme. One difference between them on the IFG account is that the Theme of (59i) is simultaneously interpersonal and topical (see, for example, Halliday's analysis of *why* in Figure 3-14, p. 55), whereas that in (61) is purely topical; nevertheless, the Themes in the general sense would still be the same. Thirdly, Matthiessen & Martin's analysis differs significantly from those that Halliday himself proposes for open interrogatives: on his account the Theme_C is "I want you to tell me the person, thing, time, manner, etc." (IFG:47).¹⁰ Is the difference intentional? If not, it is seriously misleading; if so, does this reflect a change in Halliday's analysis (the IFG account is not substantially different from that presented in the 1967 paper) or a difference of opinion between Matthiessen & Martin and Halliday?

In the absence of any explicit suggestion or evidence that Halliday has modified his analysis, it would seem better to begin with the IFG account rather than that quoted above from Matthiessen & Martin. The most obvious problem with the IFG analysis concerns (46iii), the relation between Theme_E and Theme_C: given that Theme_E = *what*, how do we arrive at Theme_C = "I want you to tell me the thing"? In our prototypical example (45), Theme_C ("Velan") is the referent of Theme_E, but clearly there is no such relation in the present example. Nor would it be plausible to say that Theme_C here is the meaning of Theme_E: *what* does not mean "I want you to tell me the thing". Evidence for this comes from a consideration of such pairs as

- (62) i *What has she bought?*
 ii *I know what she has bought*
- (63) i *Who broke it?*
 ii *I know who broke it*
- (64) i *When will she arrive?*
 ii *I know when she will arrive*

Assuming temporally, for the sake of argument, that "I want you to tell me the thing/person/time" is part of the meaning of the independent clauses, i.e. (i) in each pair, it would be inappropriate to attribute that meaning to the 'wh' words themselves, for this would require us to say that each of the 'wh' words has a different meaning in (i) than in (ii). A more general and economical account would be to attribute the "I want you to tell me" meaning to the CONSTRUCTION rather than to the individual 'wh' words: it would attach to the independent open interrogative construction, but not to the dependent/subordinate one. My example (59i) was in fact chosen because it poses a particular problem for any claim that Theme_C is attributable to the nominal group *what*, namely that *what* is here part of an idiom *what ... like*; thus (59i) would not be used (at least not in its natural, salient interpretation) to specify the value of x, such that the new boss is like x: this is reflected in the fact that in the answer, (ii), O.K. substitutes for the whole idiom *what ... like*, not just for *what*.

But if the Theme_C derives from the meaning of the construction, not from Theme_E, then this represents a massive shift in the concept of Theme relative to its application to prototypical examples like (45): in my view, this shift has never been properly acknowledged, let alone

A REPLY

justified
(1967:21)

(65)

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justified or explained. Note here that the original (and fuller) account given in Halliday (1967:212-213) is again in terms of what the clause is about:

- (65) Given that *what did John see?* means 'John saw something and I want to know the identity of that something', the theme of the message is that there is something the speaker does not know and that he wants to know; the rest of the message is explanatory comment about this demand: '(as for) what I want to know (it) is the interpretation of the "something" that John saw'. ...
We can therefore generalize the concept of unmarked theme as the element which the speech function would determine as the point of departure for the clause. In non-polar interrogative this is the WH-item; ... [it is] the subject in declarative and the finite verbal element in polar interrogative. These represent, in the unmarked case, 'what the clause is about'

This clearly implies that we are here dealing with a topic in just the same way as in a prototypical declarative like (45).

Let me now turn to the assumption that I have been accepting for the sake of argument: that "I want you to tell me the person, thing, time, manner, etc." is part of the meaning of independent open interrogative clauses — or more generally that "I want you to tell me ..." is part of the meaning of independent interrogative clauses, whether open or closed (for the latter the more specific meaning is said to be "I want you to tell me whether or not"). In *IFG:47* Halliday writes as follows:

- (66) The typical function of an interrogative clause is to ask a question; and from the speaker's point of view asking a question is an indication that he wants to be told something. The fact that, in real life, people ask questions for all kinds of reasons does not call into dispute the observation that the basic meaning of a question is a request for an answer. The natural theme for a question, therefore, is 'what I want to know'

I would take the view, by contrast, that precisely because independent interrogatives can be used in a wide range of ways, it is invalid to claim that "I want to know" or "I want you to tell me" is part of the meaning of this syntactic construction. Consider such everyday examples as

- (67) i A *She's just had another baby*
ii B *Has she? I didn't even know she was pregnant*
(68) *What is the best way of handling this problem?*

In a natural use of (67ii) B isn't wanting A to tell whether or not. A has already done that: B's *has she?* rather acknowledges what A has said as new and surprising information. (68) might be used in the prototypical way to ask the addressee to say what the best way is — but it could equally well be used by a lecturer intending to answer the question himself: what is wanted from the audience is not an answer, but attention to the lecturer's answer. The default assumptions here are: (a) "I want you to tell me ..." is not part of the meaning of the UTTERANCES (67ii) and (68) (as it occurs in the lecturing context); (b) The SENTENCES involved have the same meaning here as in utterances of them where they are used with the aim of obtaining information from the addressee. The *IFG* analysis is inconsistent with these assumptions, and surely needs justification and explanation in the light of such examples as (67) and (68).

In this connection, it is worth taking up the difference noted above between Halliday's thematic analysis of open interrogatives and Matthiessen & Martin's. They do not incorporate into the meaning Halliday's "I want you to tell me ..." component, but rather a condition that the speaker doesn't know the answer but presumes the addressee does. Again this applies to the most central use of interrogatives, but not, for example, to their use in examination-type questions. Consider, for example:

- (69) *When did Queen Victoria die?*

I might ask this because I don't know/can't remember the answer, and want to find out; but equally I might know the answer and ask the question in order to find out whether you do. Again, it seems to me that there is no reason to say that the SENTENCE (69) has different meanings in these two uses. Yet Matthiessen & Martin's thesis — following IFG, but contrary to that I have espoused above — is that Theme is systematically encoded in the syntactic structure of sentences in English, which leads to the highly implausible claim that the Theme_C of (69) is "The piece of information the speaker assumes the listener can supply, i.e. ..." even when it is used in a quiz.

Closed interrogatives raise a number of further problems. One was mentioned by Bazell in a review covering Halliday 1970a (1973:201) in connection with

- (70) i *Didn't Sir Christopher Wren build this gazebo?*
 ii *Did Sir Christopher Wren not build this gazebo?*
 iii *Did Sir Christopher Wren build this gazebo?*

According to Halliday's rules for determining Theme_E, it will be *didn't Sir Christopher Wren* in (i), *did Sir Christopher Wren* in (ii) and (iii). The Theme_C is broadly "I want you to tell me whether or not"; it is not clear how he would differentiate between (i) and (iii), but since they differ in meaning and in their Theme_E it is reasonable to assume that he would want to recognize a difference in Theme_C. The problem noted by Bazell is that (ii) has the same Theme_E as (iii) but the same meaning as (i). I referred to this point in my review (p. 160), but Matthiessen & Martin do not respond. In view of the personal criticisms that Matthiessen & Martin level against me in their paper, notably concerning my alleged inability to accept new ideas, it is appropriate to quote Bazell's comment on Halliday's thematic analysis of interrogatives: 'One cannot DEFINE the theme in English in terms of initial position and then EXPLAIN how it comes to be in this position by a peculiar English awareness! Any suggestion so implausible at first sight as the suggestion that *didn't* could be the theme of an English sentence — it is, on the face of it, not even a surface-constituent — would have to be supported by very solid arguments indeed. It is difficult to see here any trace of a genuine argument.'¹¹ Halliday's IFG account is essentially the same as in the earlier works (1967, 1970a), but there is no advance in terms of explaining or justifying this 'at first sight implausible' analysis — and Matthiessen & Martin have not attempted to remedy this situation. Note, moreover, that there is in fact one difference between the IFG analysis and the original one: in the earlier version the Theme_E was just the finite verb, whereas now it covers the Subject too. It would be interesting to know what EMPIRICAL evidence would count for or against this modification (cf. (46v)).

The second problem arising with this construction stems from the fact that whereas there are two main kinds of interrogative clause, open and closed, there are three main kinds of question, variable ('wh'), polar ('yes/no') and alternative. Both polar and alternative questions are characteristically expressed by closed interrogatives:

- (71) i *Is it a girl?*
 ii *Is it a boy or a girl?*

As observed in my review (p. 146, n. 8), Halliday does not mention alternative questions (and the same applies to the fuller discussion in the 1967 paper), but clearly the Theme_E of both examples in (71) will be *is it*. The Theme_C for (i) is "I want you to tell me whether or not"; but what is the Theme_C for (ii)? If we say it is the same as for (i), this conflicts with the fact that "I want you to tell me whether or not" is not part of the meaning of (ii). If we say it is different, this increases the difficulty of finding any consistent, uniform relation between Theme_C and Theme_E (cf. also Hudson 1986:797).

In this connection Halliday's remarks on the imperative only add to the difficulty of understanding his theory of Theme. He says (p. 49) that 'the basic meaning of an imperative clause is "I want you to do something"' (I ignore the *let's* subtype). In

(72) *You keep quiet*

the Theme_E is *you*, and the implication is that the Theme_C is "I want you to do something". This implication derives from the parallel with the slightly more explicit discussion of interrogatives from which I have quoted above, and from what he says about subjectless imperatives such as *Keep quiet*: 'Strictly speaking, these have no explicit Theme; the meaning "I want you to", which might have been thematized, by analogy with those above [i.e. (72), etc.] or with the interrogative, is realized simply by the form of the clause'. This is surely saying that "I want you to [do something]" is thematized in (72), i.e. is its Theme_C. But that is obviously not the meaning of its Theme_E, *you*, so again we must ask how Theme_C relates to Theme_E (compare, again, Hudson 1986:799). (72) has a pragmatically less salient interpretation where it is a declarative clause. Since the Theme_E is the same in either case, what is the basis for saying that in the imperative interpretation the "I want you to" meaning is thematized?

In summary, I would argue that no justification has been given for extending the concept of Theme from its application to prototypical cases like (45), where it is a matter of topic, of what the clause/message is (primarily) about, to cases like those discussed in this section, where it is a matter of presumed illocutionary meaning. The extension is clearly connected with the controversial and in my view untenable thesis that Theme in the prototypical topic sense is marked in English by first position. And it involves the loss of any uniform, consistent account of the relation between Theme_C and Theme_E — something which is of course facilitated by the failure to make any explicit and systematic distinction between Theme_C and Theme_E.

3.4 Thematic constituent structure

In clauses with a multiple Theme, the Theme_E position is filled by a string of elements that do not form a single constituent in the rank-based constituent structure. For example, in

(73) *Well but then Ann surely wouldn't the best idea be to join the group?*

the Theme_E is *well but then Ann surely wouldn't the best idea*, which is not not a group or group complex, or any other ranking unit or complex. A major feature of Halliday's model, however, is that it is multifunctional: it assigns multiple, complementary analyses so as to bring out the textual, interpersonal and ideational properties of clauses (though not just clauses). On the textual dimension, then, *well but then Ann surely wouldn't the best idea* IS a constituent — one functioning as Theme (a sister constituent of the Rheme, *be to join the group*). Where there is a multiple Theme, 'the part of the clause functioning as Theme has a further, internal structure of its own' (IFG:53). Halliday talks of this internal structure in terms of textual, interpersonal and ideational 'elements', which normally occur in that order. 'There is no further thematic structure within the topical Theme. There is, on the other hand, the possibility of further structure within the textual and interpersonal components of the Theme' (p. 54). This is illustrated in (73), where the textual component contains continuative, structural and conjunctive Themes, and the interpersonal element contains vocative, modal, and finite Themes. The textual structure for (73) Halliday gives in his Figure 3-13, which Matthiessen & Martin reproduce in their Figure 1. They claim that I misinterpret this by taking it to assign a hierarchical constituent structure in which the Theme has three ICs *well but then, Ann surely wouldn't*, and *the best idea*, with the first two of these each then further consisting of three ICs, the separate words. Matthiessen & Martin assert that there is only one layer of structure here, Theme^Rheme. Their evidence for this is simply that the labels 'continuative', 'textual', etc., do not have initial capitals. But a perfectly natural explanation for this is that they are modifiers not heads within the functional label: the functional element is not 'Textual', but 'textual Theme', and so on. The evidence for my interpretation is much stronger: we have seen that Halliday talks about the 'structure' of the Theme, about the order of 'elements' within it, about the textual and interpersonal 'components' within the Theme; in the same section he also speaks of the vocative as typically marking the beginning of the 'interpersonal Theme'. Surely all this implies constituency: what are ordered elements and components if they are not

constituents? Moreover, the box diagram notation is the same, save for the matter of capitals, as in cases where it indisputably represents layered constituent structure: compare the interpersonal analysis in Figure 4-21 (p. 93), where Subject and Finite are constituents of Mood; Figure 5-22 (p. 131), where in the last example the upper row of boxes are constituents of the clause, the lower row constituents of the clause complex; Figure A-3 (p. 350), which simultaneously has layered structure within the Theme, the Mood, and the Process, the latter being realised by a hypotactic verbal group complex (where the labels 'modulation' and 'process' are also written without initial capitals). Given that we have already noted several places where what Matthiessen & Martin say is at variance with what Halliday says, I do not accept that there is any misrepresentation of IFG in my discussion of (73).

In my review I questioned the validity of these textual constituent structure analyses, saying that 'Halliday gives no argument that the constituent structure proposed ... makes any contribution to the task of describing the semantic and syntactic structure of the clauses' (p. 162). Even if it could be demonstrated that there is some genuine sense of 'thematicity' such that one could say that each of the elements *well, but, then, Ann, surely, wouldn't, the best idea* is thematic (and I am claiming that this has not been demonstrated, only asserted) this would still not justify grouping them into the hierarchical constituent structure that Halliday gives. Though Matthiessen & Martin deny that he is treating *well but then Ann surely wouldn't* as intermediate constituents, they do regard *well but then Ann surely wouldn't the best idea* as one, but they offer no evidence to support that analysis. Semantically, there is no reason to think that it acts as a single unit of meaning, and syntactically there is nothing further to be done with it once one has got it (the relative order of the allegedly thematic elements can be stated without postulating a single Theme constituent). I have noted that both Halliday and Matthiessen & Martin claim that Theme is marked by particles rather than order in Japanese and Tagalog, but these particles surely do not attach to complex sequences like the alleged Theme of (73). Matthiessen & Martin imply the contrary when they say (§1.2) that Halliday's Theme^Rheme structure 'is surely not bizarre [but ...] has been crucial to numerous grammatical interpretations of various languages since the original formulation, e.g. Schachter & Otnes (1972)'. Schachter & Otnes do not in fact mention Theme^Rheme structure, and their 'topic' element does not cover anything comparable to Halliday's multiple Theme!¹² Curiously, although they reject my criticism of Halliday's textual constituent structure, they conclude the section on rank (§II.2) as follows: 'The problems arise when we rely too heavily on constituency outside the functional domain where it is a reasonable model, i.e. outside the experiential metafunction. The logical, interpersonal and textual modes of expression are different from constituency'. But that is precisely the point I made (for the textual and interpersonal metafunctions): constituency is not the appropriate concept for the strings of elements that Halliday labels Theme and Mood.

What we need from Systemic-Functional Grammar in this context is discussion of the kinds of criteria and evidence that are relevant to determining whether some string is a constituent or not. Consider — to return to the issue of hypotaxis — such examples as:

- (74) i *Kim gave Fred the key*
- ii **Fred the key Kim gave*
- iii **What Kim gave was Fred the key*
- (75) i *No-one believed that Fred had said the day would be fine*
- ii *That Fred had said the day would be fine no-one believed*
- iii *What no-one believed was that Fred had said the day would be fine*

A natural and standard explanation for the ungrammaticality of (74ii-iii) is that the string *Fred the key* in (i) is not a constituent. But Halliday's account of (75i) undermines this. Extrapolating from his analysis of a more complex example in Figure 7-4 (p. 196), we can take his structure of (75i) to be $\alpha^{\wedge}\beta^{\wedge}\gamma$ — which means that *that Fred had said the day would be fine* is not a constituent. We can't then attribute the ungrammaticality of (74ii-iii) to the non-constituency of *Fred the key*, because that would predict that (75ii-iii) should be ungrammatical too.¹³ Here then there is syntactic evidence for a string to be a constituent, whereas Halliday does not give it constituent status; conversely in (73) there is no syntactic evidence for the

constituent status of *well but then Ann surely wouldn't the best idea*, though Halliday does treat it as one. What principled basis is there for deciding constituency?

4. Subject

Halliday's Subject, like his Theme, has an expression aspect (Subject_E) and a content aspect (Subject_C). Thus when, with reference to *The duke has given away that teapot, hasn't he?*, he speaks (p. 76) of *the duke* (an expression) as Subject, he means Subject_E; but when he says, of *That teapot the duke gave to your aunt, didn't he?*, that 'the duke is Subject' (p. 77), he must mean Subject_C, since the duke is a person (compare also, p. 76, 'the typical Subject of an offer is the speaker, and that of a command is the person being addressed': again, he must mean Subject_C, since speaker and addressee are persons, not expressions). For someone or something to be Subject_C means that they are 'held responsible for the functioning of the clause as an interactive event' (p. 76).

I criticized Halliday's account on two grounds. The first was that his explanation of responsibility rests too heavily on offers and commands: 'It is perhaps easier to see this principle of responsibility in a proposal ... where the Subject specifies the one that is actually responsible for realizing (i.e. in this case for carrying out) the offer or command' (p. 76). Examples are *I'll open the gate, shall I?* (offer) and *Stop shouting, you over there!* (command). In such cases the concept 'responsible' matches its everyday use — cf. *The Macquarie Dictionary* entry for *responsible*: 'answerable or accountable, as for something within one's power, control or management'. But when we move from offers and commands to statements and questions, we lose contact with this everyday sense of responsibility, and I accordingly wrote: 'this generalization is very hard to follow: it stretches the concept of responsibility far beyond its normal sense'. The objection is not terminological: we are entitled to use old terms in new ways provided we explain what the new sense is, but this Halliday does not do. Consider the active-passive pair

- (76) i *You issued the invitations*
 ii *The invitations were issued by you*

We are simply not told what it means to say that in (i) the addressee, and in (ii) the invitations is the responsible element, the one on whom the validity of the information is made to rest. Unless this can be clarified the account is circular (the term Bazell used in his criticism of Theme): the only way of determining which element has 'modal responsibility' is to see which one satisfies the syntactic criteria for Subject_E.

The second ground for my objection concerned clauses where Subject_E is *nobody, there* or the like. *Nobody move*, for example, would be used as a command but surely does not fit Halliday's account whereby 'the Subject specifies the one that is actually responsible for ... carrying out ... [the] command'. Nor can responsibility be coherently assigned to "there".

Matthiessen & Martin's response to these criticisms is, to say the least, unhelpful. There are, firstly, two more misrepresentations to point out. At the beginning of §II.3.2 they attribute to me the view that there is no more to the concept of Subject than recognition criteria such as 'verb agreement, case, and position in declarative and interrogative clauses'. They observe that I do not in fact say how I myself would characterize Subject: this is correct, for as I was reviewing Halliday's work I concerned myself with discussing his characterization. If Matthiessen & Martin want to compare the two approaches, they should surely take the obvious steps to ascertain what mine is, namely look at my books (1984, 1988b). The approach to the definition or explanation of grammatical categories adopted there — one which probably owes most to the work of John Lyons (beginning with his important 1966 paper) — is to examine them at two levels, the language-particular and the general. At the language-particular level we are concerned with the grammatical criteria which determine what expressions in the language under investigation belong to the category. For English, position in declaratives and interrogatives, case, verb agreement, etc., provide distinctive properties of the Subject at the

language-particular level. At the general level we are concerned with what is common to categories across language — and this is very often a matter of their semantic basis. The semantic properties involved in the general definitions, however, are a matter of the characteristic meaning of the grammatically most elementary instances: they give the semantic basis for the category, not its meaning. It is thus not true to imply that I ignore the 'functional character of Subject' [Misrepresentation₁₃].

The second case of misrepresentation is in the earlier criticism of my method of argument (§1.5). I have already mentioned, in connection with the relation between Theme and topic, their claim that I make a 'practice of glossing what Halliday says and then arguing about [my] own glosses rather than Halliday's original interpretation': discussion of Subject is said to provide a second instance of this, glossing 'responsibility' as 'agentivity'. But it will be clear from the quotations given above that this charge is baseless. I was concerned with trying to figure out what Halliday means by 'responsibility'. Halliday tells us that this concept is easiest to grasp in offers/commands, and I point out that in these cases one can associate responsibility in the sense of agentivity with the Subject; this notion of agentivity, it will be noted, ties in with the features of 'power, control or management' in the *Macquarie* definition of 'responsible' cited above. But there is no sense in which I go on to argue from my own gloss 'agentivity': my argument is based on Halliday's own characterisation of the Subject in commands as the one responsible for carrying out the command, questioning how this is to be generalized to statements/questions [Misrepresentation₁₄]. Notice here that it is not just me who is unable to figure out what Halliday intends: compare Hudson (1986:798-799), who includes the account of Subject as the responsible element among the categories 'so vaguely defined that I could not reliably identify instances of them'.

Matthiessen & Martin's response, however, makes no attempt to clarify what is meant by 'modal responsibility': they prefer simply to accuse me of confusing it with agentivity. They begin their §II.3.2 with seven questions involving the Subject and Mood elements: 'Why is the combination of Subject + Finite used to realise mood selections?, ..., Why is Subject related to the selection of polarity the way it is?' They tell us that Halliday 'move[s] in on Subject in spoken dialogue in terms of the interpersonal metafunction: subject-status is interpreted in terms of modal responsibility'. They assert 'Halliday's interpretation of Subject does in fact put us in a position to account for questions such as those listed above' — and then turn to another issue! They do not even begin to show how Halliday's 'interpretation' accounts for their questions, and indeed for the last question they do not even say what the phenomenon to be explained is — what is the relation between Subject and polarity selection that is explained by modal responsibility? There is no argument, no demonstration, no explanation here: simply a bald assertion that Halliday's new approach enables us to provide an explanation. Matthiessen & Martin haven't answered my questions and objections concerning the notion of 'responsibility': they have simply put them aside. And they do not even mention the questions I raised concerning the Complement function.

5. Grammaticalization

§4 of my review dealt with various areas where I contended that Halliday's analysis is based on semantic considerations rather than grammatical ones, contrary to the principles that he set out on p. xx. I began there with the ideational functions in clause structure and then turned to various issues in the verbal group. Here I will reverse the order, beginning with three issues in the verbal group: the analysis of *know how to spell* as a verbal group complex, that of *have to* as a modal operator and the status of *will* as a future tense operator.¹⁴ These issues, in my view, are particularly clear-cut, and illustrate very strikingly the grammatical complications that arise as a result of Halliday's semantically-driven approach. It seems best to start with these, then: if we cannot clarify our respective positions relative to these relatively straightforward cases, there is not much chance of our doing so in regard to the more complex ones.

5.1 *Know how to do* as a verbal group complex

IFG analyses *doesn't know how | to spell* as a verbal group complex of structure $\alpha^{\wedge} \beta$ (p. 259). I pointed out (p. 172) that its structure is not provided for in the section on the verbal group, and suggested that since there is no reason to single out *how* from the other interrogative words, we will need verbal group complexes like *didn't know what | to do, didn't know who | to turn to*, etc., with the result that a great deal of our clause analysis will have to be duplicated in our account of verbal group or verbal group complex structure.

Matthiessen & Martin respond by claiming that there are indeed grounds for singling out *how* from the other interrogative words. They note 'that *how* does not exclude a specification of manner in the clause in which the verbal group complex occurs', so that it differs from the others in not having 'a participant or circumstantial role in the transitivity structure of the clause' (§II.4.2 (iii)(b)). This difference is illustrated in

- (77) i *He didn't know how to type*
 ii *He didn't know how to type with two hands*
 (78) i *He didn't know who to ask*
 ii **He didn't know who to ask Henry*

But this difference between *how* and *who*, etc., does not provide any argument for assigning to (77i) and (78i) the different analyses they are proposing: it has nothing whatsoever to do with the *know WH to* construction. Compare, for example:

- (79) i *How do you type (with two hands)?*
 ii *Who did you ask (*Henry)?*
 (80) i *I don't know how you type (with two hands)*
 ii *I don't know who you asked (*Henry)*
 (81) i *the problem of how to type (with two hands)*
 ii *the problem of who to ask (*Henry)*
 (82) i *No-one had shown how to type (with two hands)*
 ii *No-one had shown who to ask (*Henry)*

(The last pair is based on Halliday's *No-one had shown how to do it*, analysed (p. 217) as a clause complex, presumably with *no-one had shown* at α , *how to do it* at β .) We find exactly the same difference between *how* and *who* here, yet there is no suggestion that we should make any comparable difference in structure between (i) and (ii) in these pairs. The difference is going to need to be dealt with in our account of the individual interrogative words, but once we have done this it will cater for all the above data, without any need to assign different structures to (77i) and (78i). One can't argue for different structures for (77i) and (78i) by pointing out a difference between *how* and *who* without showing how the proposed structures enable us to account for the difference. Matthiessen & Martin don't attempt to do this and the reason they don't is surely that the difference between the proposed structures plays no role in accounting for the difference between *how* and *who*.

Matthiessen & Martin go on to say that '*know how to do* is part of a verbal group paradigm that includes *can do, be able to do, learn to do, fail to do*, and so on', whereas there are no comparable paradigms involving *know what to do*, etc. My point is that the similarity between *know how to do* and *can do*, etc., is semantic rather than grammatical, and that grouping them together significantly complicates the grammar. In the first place we need to add some ad hoc structure for the verbal group *know how*: I pointed out that the IFG section on verbal group structure does not cater for it, and Matthiessen & Martin conspicuously fail to fill that gap. Secondly, it requires that we impose a no less ad hoc constraint on the structure of (78i). Presumably this is a hypotactic clause complex with *he didn't know* as α and *who to ask* as β . The β clause dependent on a *know*-clause at α can be a finite or non-finite interrogative clause, but if it is non-finite we have to stipulate that the interrogative element cannot be *how* in Adjunct function. What possible explanation could there be for such a constraint? What reason is there to exclude *how* in this construction but not in (79)-(84)? Matthiessen & Martin are extracting an item from a

paradigm where it clearly belongs on grammatical grounds to place it in another to which its affinities are merely semantic. Is it just *know* that takes *how* in its verbal group in this way, or does the same analysis apply to any of the following: *learn, remember, ask, wonder, etc.*? And what criteria, other than paraphrase, can one bring to bear in making the decision?

5.2 *Have to* as a modal operator

IFG's Finite element is filled by a finite verbal operator which is either temporal (expressing primary tense) or modal (p. 75). The modal operators, which are classified as 'low', 'median' or 'high' are listed (ibid.) as follows:

- | | | | |
|------|-----|---|----------|
| (83) | i | <i>can, may, could, might</i> | [low] |
| | ii | <i>will, would, should, is to, was to</i> | [median] |
| | iii | <i>must, ought to, need, has to, had to</i> | [high] |

On p. 238 the same items — *save, unaccountably*, for the omission of *need, is to* and *was to* — appear again, classified now as non-sequent or sequent. Sequent forms occur 'following a past projection ... such as *they said*' (p. 179). Here *had to* is given as the sequent counterpart of both *must* and *has to*; it is the only one which appears only in the sequent column (*could, would, etc.*, appearing in both columns). On p. 336 Halliday refers to the 'full list' of modal auxiliaries, citing them as: *can, could, may, might; shall, should, will, would; must, need, ought to, is to, has to*. The absence of *had to* from this list is presumably due to its being subsumed under *has to*, of which it is the sequent form; and it is a reasonable inference from the omission of *was to* that this too is regarded as only a sequent form, corresponding to non-sequent *is to*. (The inclusion of *shall* in the full list but not in (83), however, suggests that its omission from the latter was an error.) We should note that the term 'modal auxiliaries' here is apparently a mistake for 'modal operators'. Operators are always finite, whereas the account of verbal group structure given on pp. 175-176 implies that auxiliaries are always non-finite — see, for example, Figure 6-15, where modal *could* precedes the Auxiliary₁ position: in what follows I shall distinguish operator and auxiliary in this way.

The inclusion of *has to* among the modal operators is discussed on p. 258. Halliday says that *has to do* was originally two verbal groups, but has 'evolved' into a single one with *has to* a modal operator — and the same account is given for *is to do* (p. 257). He does not, however, present any evidence for the claim that there has been such an evolution, such a reanalysis. And he does not mention that *have to* has the full range of inflectional forms, unlike the other modal operators in (83). This raises the question of how Halliday would analyse such examples as:

- | | | |
|------|----|--|
| (84) | i | <i>may have to do, will have to do, is having to do, has had to do</i> |
| | ii | <i>had to do</i> [non-sequent, as in <i>In those days we had to do our own washing</i>] |

Halliday cannot be unaware of the existence of such forms as these, and it is completely baffling that he fails to mention them when they present such obvious and major problems for his analysis.

There would seem to be two possible analyses within his framework. On the one hand, he might say that *have to* is a modal whatever its inflectional form. As such, it would have to be both a modal operator and a modal auxiliary — but it would be the only item belonging to both sets, and the inclusion of it as an auxiliary will greatly complicate the systems and structure of the verbal group. The systems will have to allow for more than one modality selection, and the possibility of examples like *Students have always had to have completed three years of pass level work before enrolling for honours* shows that the number of tense selections will increase too; the structures will have to allow for the appearance of a modal Auxiliary at a wide range of positions. What possible compensation could there be for this complication of the grammar?

The alternative is to say that the expressions in (84) are all sequences of two verbal groups, hypotactically related, like *may try to do, will try to do, etc.* The trouble with this is that

there is no explanation as to why *has to do* is treated differently from (84): what grammatical justification can there be for analysing it differently from the rest of its paradigm? On this account we will have a lexical verb *have* (the *to* will now belong in the dependent verbal group) — and it will be the only lexical verb in the language which has all inflectional forms other than the present tense. This analysis resembles that adopted by Matthiessen & Martin for *know how to*, as discussed in §5.1 above: *has to* is extracted from the paradigm to which it belongs grammatically and is placed in a paradigm with *must*, to which its resemblance is semantic rather than grammatical.

Let us consider the grammatical properties of the clear modal operators. In the first place they have certain properties common to both temporal and modal operators, including those that have been referred to acronymically as the NICE properties (Huddleston 1976:333-334, Palmer 1986:25, 90-91):

(85) Operators occur without 'do-support' in

- i Negatives (*She could not do it*: cf. *She didn't want to do it*)
- ii Interrogatives (*Could she do it?*: cf. *Did she want to do it?*)
- iii Code [ellipsis] (*I could do it and she could ___ too*: cf. *I wanted to do it and she did ___ too*)
- iv Emphasis (*She COULD do it*: cf. *She DID want to do it*)

I do not myself believe that these properties justify assigning the NICE verbs to a distinct structural position (for reasons argued in the above paper), but as Halliday does have a distinct operator position one would expect that these properties would play a significant role in determining the membership of the operator class. A second set of properties distinguish the clear modal operators from the non-modal (temporal) operators *be, have*:

- (86) i The modal operators have only tensed forms no base form, no present or past participle;
- ii They are paradigmatically contrastive and cannot combine syntagmatically (other than in coordination): *She must swim, She can swim, *She must can swim*;
- iii They show no person-number agreement with the subject;
- iv One of these verbs is required in the main clause of an unreal (remote) conditional construction

This last property is illustrated in

- (87) i *If Ed comes tomorrow, we can play tennis* [Real/Open]
- ii *If Ed comes tomorrow, we are playing tennis* [Real/Open]
- (88) i *If Ed came tomorrow, we could play tennis* [Unreal/Remote]
- ii **If Ed came tomorrow, we were playing tennis* [Unreal/Remote]

Consider now *have to* with respect to these properties. It differs from the operators with respect to (85) in that for most speakers the construction with *do* is at least possible, and for many the preferred or only possible form. As for (86), *have to* differs categorically from the genuine modal operators. (84i) shows that it lacks properties (86i-ii); the contrast between *She has to do it* and *They have to do it* illustrates the lack of property (iii); and the ungrammaticality of **If he brought his family with him tomorrow, we had to have a barbecue* shows that it lacks property (iv).¹⁵

I mentioned in my review (p. 172) the problem that is created for IFG's treatment of *has to*: I have presented the argument again here at somewhat greater length because Matthiessen & Martin's response simply makes no contact with it. They see the difference between my approach and IFG's as a question of 'whether our interpretation is morphological (from below, in terms of the rank scale) or syntactic (from above) (§II.4.2 (ii))'. They continue: 'If we approach the modals from above, we can see paradigms such as *can/could; is/am/are/was/were/able*

to//be/been/being able to and can suddenly looks like have to', and they tabulate the forms under finite and non-finite headings, essentially as in:

(89) Finite (finite)	in Predicator (non-finite)
<i>have to, has to, had to</i>	<i>have to, having to, had to</i>
<i>can, could</i>	
<i>are able to, is able to, am able to,</i> <i>was able to, were able to</i>	<i>be able to, being able to, been</i> <i>able to</i>

There are two points to be made here. The first concerns the distinction between morphological and syntactic criteria, which needs to be clarified. It goes back to the original Scale-and-Category days (Halliday 1961:261-262), where it arose in connection with Halliday's view that 'a class is NOT a grouping of members of a given unit which are alike in their own structure. In other words, by reference to the rank scale, CLASSES are derived "from above" (or "downwards") and not "from below" (or "upwards")'. He then goes on to say that as the traditional word-based distinction between morphology and syntax has no theoretical validity the terms 'morphology' and 'syntax' can be adapted to 'refer to direction on the rank scale', with upward analysis being morphological and downward analysis syntactic. This redefinition of syntax and morphology is reasonable enough in a theory that denies special status to the word, but it is then important that the terms should be used rigorously as redefined. But there is no such rigour in Matthiessen & Martin's appeal to the morphology-syntax distinction: it is not true that the grammatical differences between *has to* and *must* are morphological, a matter of their own internal structure. Of the eight properties mentioned above in (85) and (86), only two —(86i and iii) — are morphological: the remaining six are syntactic. Matthiessen & Martin are quite improperly invoking the syntax-morphology distinction to dismiss without argument a whole range of phenomena that provide evidence against their analysis. And it should be emphasized that there is no reason to accept that the two morphological properties are irrelevant: the description of the phenomena in question will be simpler if it can be made in terms of a category (namely modal operator) that has been syntactically established than if it has to be made in terms of some new ad hoc category.

The second point is that in their tabulation of the finite and non-finite forms Matthiessen & Martin have not arranged them systematically: the finite forms are arranged one below another, each on a separate line, whereas the non-finite ones are placed one after another on just two lines. The result is that *be able to*, etc., are misleadingly aligned with *can*. I have removed this inconsistency from my own tabulation in (89). And what emerges from this more systematic arrangement is not, as Matthiessen & Martin are claiming, that '*can* suddenly looks like *have to*', for *can* has no non-finite forms (cf. (86i)): what looks like *have to* is *be able to*. But this provides no support at all for the IFG analysis, for neither *be* (as it appears here) nor *be able to* is analysed by Halliday as a modal operator like *has to*. *Is able to do* is not a verbal group but a verbal group complex (see Table 7(18), p. 258): Matthiessen & Martin's tabulation argues for the same treatment of *has to do*, contrary to Halliday's analysis. Far from answering my objection to IFG, their brief discussion supports it: their morphological-syntactic distinction has no bearing on the issue at all. Indeed they present (89) — which relates to my (86i) — as a syntactic rather than morphological matter. They have failed to see that Halliday's analysis of *has to* as a modal operator caters only for the finite forms (and even then not for (84ii)), and have failed to take note of the points I made in the review concerning the grammatical differences between *has to* and *must*.

5.3 Will as tense and modal operator

IFG has *will* belonging to both the temporal and the modal operators (p. 75). As a temporal operator it marks the primary tense future; as a modal operator it expresses one or other of two types of modality, either 'modalization' (covering probability or usuality) or 'modulation' (more specifically, inclination). Examples (pp. 183, 335-336) are as follows:

- | | | | |
|------|-----|---------------------------------|----------------------------------|
| (90) | i | <i>She will arrive tomorrow</i> | Temporal: future |
| | ii | <i>Mary will probably know</i> | Modal: modalization: probability |
| | iii | <i>It will usually work</i> | Modal: modalization: usuality |
| | iv | <i>John will take you home</i> | Modal: modulation: inclination |

I argued against this analysis on the grounds that *will* belongs grammatically with the modals, and that the separation of temporal from modal *will* is motivated by semantic factors, not grammatical ones. My position is that English does not grammaticalize futurity in a future tense. Invoking the distinction between the language-particular and general levels discussed above, I would say that a general definition of future tense is that it is a grammatically distinct verbal construction whose characteristic use is to express future time: English has no future tense because there is nothing satisfying this condition of grammatical distinctiveness.

Matthiessen & Martin dismiss the argument that *will* belongs grammatically with the modals as 'morphological' (§II.4.2 (i)). This is a quite invalid response. There are two complementary grounds for saying that *will* belongs grammatically with the modals. The first is that it shares with them the properties listed in (86) above: the distinction that *IFG* makes between future and modal *will* is not reflected in any difference with respect to these properties. (86ii and iv) are syntactic, not morphological; and, as noted above, in the discussion of *have to*, the description of (i) and (iii) will also be simpler if we can state them in terms of the syntactic class modal operator: it is an advantage if our syntactic and morphological categories match. And as I have pointed out, in connection with (86i), in their discussion of *have to* Matthiessen & Martin present the paradigmatic relation between finite and non-finite forms as a factor relevant to a SYNTACTIC interpretation.

The complementary reason for putting *will* with the modals is that it is very different from the markers of tense (primary tense, in *IFG*'s terms). The morphological aspect of this is of course that it is an analytic marker, not an inflectional one, but it is the syntactic aspect that is important: far from being in paradigmatic contrast with the past and present tense markers, *will* can combine syntagmatically with either. Compare, initially,

- | | | |
|------|----|--|
| (91) | i | <i>Kim was able to swim when she was three</i> |
| | ii | <i>Kim is able to swim now</i> |
| (92) | i | <i>Kim wasn't willing to help us yesterday</i> |
| | ii | <i>Kim isn't willing to help us now</i> |
| (93) | i | <i>Kim could swim when she was three</i> |
| | ii | <i>Kim can swim now</i> |
| (94) | i | <i>Kim wouldn't help us yesterday</i> |
| | ii | <i>Kim won't help us now</i> |

I take it as uncontroversial that (91)-(92) exhibit a contrast between past tense (i) and present tense (ii). The relationship between (i) and (ii) is exactly the same in (93)-(94), and one would need very strong arguments, therefore, for not treating them as exhibiting the same tense contrast — especially in what Matthiessen & Martin acknowledge to be a 'semanticky' grammar. Inexplicably, however, *IFG* treats the modal and tense operators as mutually exclusive, so that no provision is made for (93)-(94) to differ in tense. Although I queried this in my review (p. 171), Matthiessen & Martin have chosen not to take it up: it is, nevertheless, essential to any comprehensive consideration of the status of *will*. *IFG*'s failure to mention examples like (93)-(94) was the kind of thing I had in mind when describing its coverage as 'quite selective and uneven' (p. 137), a remark which Matthiessen & Martin condemn as 'unproductively negative' (§I.5); it is inevitable that any account of English grammar the length of *IFG* will have to be very selective, and the remark was not intended as a specifically negative observation. Nevertheless, in the present context it does seem to me that the imbalance between the very brief account of tense and the quite detailed discussion of modality is a significant weakness, given the intimate connection between them. It is not possible to evaluate Halliday's analysis of *will* properly without considering the relation between tense and modality.

Broadly speaking, we can distinguish three uses of the past tense in English, which I shall dub 'past time', 'backshifted present' and 'factual remoteness', illustrated respectively in:

- (95) i *They took both cars yesterday*
- ii *[She said] she took him seriously*
- iii *[It'd be better] if she took no notice of them*

Notice that the backshifted present use is found not only when the past tense of the 'projecting' verb has the past time use, as in (95ii), but also when it has the factual remoteness use, as in *If I thought she took him seriously I would share your concern, but I am sure she doesn't*; here there is no past time associated with the taking, and it is therefore clear that the backshifted present use could not be subsumed under the past time use. (Halliday doesn't analyse the *took* of (ii) as a past tense form: he analyses it as a present tense form in a different tense system, the sequent one — this is implicit in IFG but explicit in, for example, Halliday (1976a:130). There is, however, no verb lexeme where we find any morphological difference, and hence I shall continue to refer to it as a use of the past tense, rather than a different form.) For verbs other than the modals, the factual remoteness use is restricted to subordinate clauses, and since the backshifted present use is likewise restricted to certain contexts it is clear that the past time use represents the primary or basic use. That of course is why we call this category 'past tense'— if (iii) represented the basic use, we would call it a mood.

We find the same three uses for *could* and *would*:

- (96) i *[I asked her to help me] but she couldn't/wouldn't* [Past time]
- ii *[She said] she could/would help me* [Backshifted present]
- iii *[It'd be good] if she could/would help me* [Factual remoteness]

The modals differ from other verbs, however, in two respects. The past time use is much less common, and not found with all members of the class or with all senses. For example, *might* is now virtually restricted to uses (ii) and (iii), and while the *would* of 'inclination' has a past time use, that of 'probability' (to use IFG's labels, for convenience) does not. Conversely, the factual remoteness use is not restricted to subordinate clauses, and is thus much more common than with other verbs. Factual remoteness is a modal rather than a temporal concept, and hence it is not unnatural that this use should be more common with verb lexemes whose lexical meaning involves modality. But these differences in the relative frequencies of the three uses and in contextual restrictions on them would not justify our saying that the inflectional categories for the modals are distinct from those of other verbs. This would lead to a loss of generalization in the statement of the rules for backshifting, and also of those requiring a form with the factual remoteness sense:

- (97) i *I wish/It's time he knew/*knows how to do it*
- ii *I wish/It's time he could/*can do it*
- (98) i *It'd be better if he knew how to do it himself*
- ii *It'd be better if he could do it himself*

Wish and *it's time* require a past tense in the complement: if *could* is not analysed as a past tense this rule will not be stateable in its proper generality. And the same holds for the rule that the operator in the protasis of an unreal/remote conditional must be a past tense.

This discussion of past tense has been necessary to provide a solid basis for my claim that *will* does not contrast with the tense markers but can combine syntagmatically with either. In this respect, as well as in respect of the properties in (86), it belongs with the modals. And there is no more justification from this matter of the combination with past and present tense than there is from the properties of (86), for singling out a futurity *will* distinct from modal *will*.

This, as I see it, is the essence of the GRAMMATICAL argument against accepting *will* as a future tense marker: it belongs grammatically with the modal operators (with which it is mutually

exclusive), not with the tense markers (with either of which it can combine). This argument cannot be dismissed as simply 'morphological'.

In the case of *will*, however, we do not find the conflict between grammar and semantics that we noted for *have to*. For *will* belongs with the other modals not only grammatically, but also semantically: it expresses modal meaning. The difference in meaning between a simple present tense form and *will* + base form is not one of time, but of modality. Consider the following examples:

- (99) i *That is the plumber*
 ii *That will be the plumber*
 (100) i *Australia meets Sweden in the Davis Cup final in December*
 ii *Australia will meet Sweden in the Davis Cup final in December*

In (99), spoken in response to a knock at the door, the time is present and the difference in meaning between (i) and (ii) is uncontroversially a modal one: IFG analyses (i) as unmodalized present tense, (ii) as modalized (and tenseless). In (100) the time is future and the difference between (i) and (ii) is still a matter of modality. (i) is here again epistemically stronger than (ii): to invoke a concept from Lyons (1977:808), one needs firmer evidence to provide 'epistemic warrant' for it than is the case with (ii). The normal context for (i) is one where the semi-finals have already been played and won by Australia and Sweden: it has thus already been determined that they should meet in the final and given that the final has already been scheduled for December the speaker can present their future meeting without any modal qualification. By the standards applying in ordinary speech (as opposed to those applying in epistemic philosophy) the speaker can present it as something that is effectively known. (100ii), however, could be said either after or before the semi-finals: in the latter case, the speaker is PREDICTING the outcome of the semi-finals. Given that there is no difference in SENTENCE meaning depending on whether it is uttered before or after the semi-finals, the sentence meaning of (ii) must be epistemically weaker than that of (i), and hence the difference in meaning between (ii) and (i) must be a modal one — as it is in (99). IFG makes a sharp distinction between (100ii), unmodalized future, and (99ii), modalized tenseless; but it makes no grammatical distinction at all between (100i) and (99i), both being unmodalized present tense. It thus treats the difference between (i) and (ii) in (99) as one of modality, but that in (100) as one of tense. If Matthiessen & Martin wish to defend the IFG analysis, they need to justify the way it handles the relationships within the four examples of (99)-(100).

There are, it seems to me, two factors which may underlie IFG's failure to handle the contrasts in (99) and (100) in parallel ways. One is the treatment of modal *will* as expressing the MEDIAN value of modality — that expressed also by the modal adverb *probably*, in contrast to the low value of *may* or *possibly* and the high value of *must* and *certainly*. I argued against this in my review (p. 171; see also Huddleston 1979: 335-336 for fuller discussion). Evidence that *will* is significantly stronger than *probably* is seen in the contrast between *That's probably the plumber, though I may be wrong* (perfectly normal) and *That'll be the plumber, though I may be wrong* (unacceptable because pragmatically inconsistent).¹⁶ Often the evidence one has when using *will* is only such as would, strictly speaking, justify *probably*, but the same tendency to use an epistemically stronger expression than one has strict warrant for is found also with *must*. If I say *Ed must have overslept* on the basis of his failure to turn up for an early rendez-vous, I am ignoring a host of other possible explanations for his non-appearance. And in examples like

- (101) *Bill is John's father and John is Tom's father, so Bill will be Tom's grandfather*

the epistemic warrant is such as to make a reformulation with *probably* quite absurd. As Palmer (1986:62) puts it, '*will* often seems, like *must*, to indicate that the judgement is based on known facts, and in particular, on what is usually the case, as in [*They'll be on holiday at the moment*]. It is for this reason that it is not equivalent to "probable"' (cf. also Lakoff 1970:840). The label Palmer suggests for it is 'Assumptive'. ('Assumptive' is a category of epistemic modality, and I shall henceforth refer to the *will* of (99ii) and (101) as epistemic *will* — or rather, more

specifically, as 'narrowly epistemic' *will*, because I believe that future *will* is also epistemic, in a broader sense of that term, as suggested by Palmer (1983:242) in the quotation given below.)

The second factor is that we inevitably know less about the future than we do about the present or the past, and that our epistemic warrant for assertions about future situations will much more often fall short of what would justify an unmodalized assertion. This accounts for the high correlation between futurity and *will*. But it does not follow that *will* means "futurity": we can use *will* to express 'assumptive' epistemic modality, to make predictions, about any of the three time spheres, and we can — when the evidence warrants it, as in (100i) — make assertions about the future without *will*. Compare again Palmer, continuing after introducing his term 'Assumptive': '*Will* is also used to refer to future time. This is not particularly surprising: the future is not fully known, but it is a reasonable assumption that it will ensue'. Unmodalized assertions about the future, like (100i), are pragmatically highly constrained in English. This is why it would, for example, be pragmatically anomalous (given standard assumptions about Davis Cup tennis) to substitute *beats* for *meets* in this example — and again the fact that the substitution of *will beat* for *will meet* would be perfectly acceptable in (ii) is evidence against analysing the *will*-forms as expressing unmodalized futurity. Further evidence for the view that *IFG*'s future tense operator *will* is semantically modal will emerge in our discussion of certain subordinate constructions below.

Although *will* is semantically modal, there is a sense in which it can be regarded as the most neutral of the modal operators, the one that introduces the least, or the least specific, qualification. This is reflected in the fact that it is, as it were, the 'default' modal with respect to the requirement that the main clause of an unreal conditional construction contain a modal (cf. (86iv)) — compare

- (102) i *If the twins are here, they are downstairs*
- ii *If the twins are here, they will be downstairs*
- iii **If the twins were here, they were downstairs*
- iv *If the twins were here, they would be downstairs*

(ii) has (iv) as its unreal counterpart, whereas (i) has no immediate unreal counterpart, with (iii) being ungrammatical by virtue of (86iv). The meaning difference between (i) and (ii) is, however, quite slight, and hence the unreal counterpart of the latter can be used to fill the gap resulting from the ungrammaticality of (iii). The examples in (102) involve present time; a comparable set involving future time is:

- (103) i *If you don't get here on time tomorrow, you lose your job*
- ii *If you don't get here on time tomorrow, you will lose your job*
- iii **If you didn't get here on time tomorrow, you lost your job*
- iv *If you didn't get here on time tomorrow, you would lose your job*

Again there is not a great deal of difference between (i) and (ii), but such as it is, it is clearly modal: the threat in (i) is somewhat more forceful, presenting the sacking as inevitable, as something that has already been determined. We have noted that there are pragmatic constraints on the use of an unmodalized present tense for a future situation, and in many cases only (ii) and (iv) will satisfy both pragmatic and grammatical constraints.

Let us turn now to Matthiessen & Martin's defence of *IFG*'s treatment of *will*. Their initial point is that 'a large part of the problem is that Huddleston only seems to accept the view from below — from morphology — and not the view from above — from the grammar of the groups'. I have already argued that this is completely without foundation: the argument given originally and developed further above, draws on morphology, syntax and semantics/pragmatics. It cannot be dismissed by a stroke of the pen in that way.

Matthiessen & Martin go on to 'mention' four 'grammatical arguments' in favour of the *IFG* distinction between future tense *will* and modal *will*. The first is merely an allusion to typological differences between languages. It needs to be developed much more carefully before

it can be critically evaluated: at this stage I would merely express my agreement with Palmer (1983:243), who — while endorsing the value of comparing different languages (much of his work in the last few years has been in this area) — writes: 'But I would suggest that it is unhelpful to ASSUME [my emphasis] that languages have future tenses. That assumption may obscure important typological features, not least that reference to the future, in a whole variety of languages, is treated very differently from reference to the past and present, and is often closely related not to tense systems, but to systems of modality (see Lyons 1977:816)'.

Matthiessen & Martin's argument (b) is that there are distributional differences between modal and future *will*, such that 'by and large, future *will* does not occur in dependent clauses providing logical or temporal conditioning ... but modal *will* is still quite unrestricted'. There are three points to make about this argument. The first is that the modal *will* whose occurrence in conditional clauses is 'unrestricted' is the one expressing 'modulation', not 'modalization': the *will* of modalization is just as much subject to ('non-categorical') restrictions here as is that of futurity. Contrary to the way Matthiessen & Martin present it, therefore, the distributional difference is not between temporal and modal *will*, but between the *will* of modulation (inclination) and the rest. Moreover, the restrictions on the latter apply also to the other modals of modalization: *if it may rain, if it must rain* and the like are just as unlikely (though not of course ungrammatical) as *if it will rain*. This point is made by Palmer (1983:242), who follows it up with the following observation: 'I have no doubt that *will/shall* is modal (and perhaps epistemically modal, in a wider sense of "epistemic"), like *may* and *must*. It is no coincidence that English uses what is morphologically and syntactically a modal verb as one device to refer to the future, but essentially to a "modal" future'.

The second point relates to the fact that the restriction on future *will* in conditionals is not categorical. Matthiessen & Martin acknowledge this, as we have seen, and refer the reader to Close (1980) for examples and discussion. The fact that *will* is sometimes found in conditionals raises the issue of what the difference in meaning is between the conditional with and without *will*. Consider the following pairs:

- (104) i *If the slick will come as far as Stavanger, [then of course I must take precautions on a massive scale]*
 ii *If the slick comes as far as Stavanger, [hundreds of miles of our coastline will be spoilt]*
- (105) i *If Claude will be here tomorrow, [there's no need to call him now]*
 ii *If Claude is here tomorrow, [we can ask what he thinks about it]*

(The first three examples are from Close.) What is the difference in meaning between the conditional clauses in each pair? Clearly, it is not a matter of the TIME of the oil slick coming as far as Stavanger or of Claude being here. The difference is, rather, a matter of MODALITY: in (i) we are, as Close puts it, concerned with predictability. In (104i) the speaker (a Norwegian minister) is talking about the need to take precautions before the arrival of the slick in Stavanger: there is a need to do this if the occurrence of that event is of the degree of probability expressed by *will*, i.e. if it is predictable; analogously in (105i). Close's conclusion is that the distinction between a future conditional with and without *will* is that the former involves 'assumed predictability', the latter 'assumed future actuality' — and that the assumed predictability covers both volitional (IFG's modulation) and non-volitional (IFG's future tense) cases. Thus although Close does work with a future tense for English, his discussion of conditionals brings the modulation and future tense uses of *will* together semantically, as both expressing the modal concept of predictability.

The third point is that taking *will* as a future tense marker makes no contribution to explaining why this *will* is relatively rare in conditional clauses: why should the future behave so differently from the present and past? If, however, we take it as expressing epistemic modality the distributional facts can be explained by reference to the distinction Lyons draws between subjective and objective epistemic modality (1977:797-798). One of the examples Lyons uses to illustrate the distinction is

(106) *Alfred may be unmarried*

In the subjective interpretation, 'the speaker may be understood as subjectively qualifying his commitment to the possibility of Alfred's being unmarried in terms of his own uncertainty' — and in this interpretation it would be natural for the speaker to add *but I doubt it* or the like. Lyons goes on to sketch a scenario where 'the possibility of Alfred's being unmarried is presentable ... as an objective fact': there is 'a community of ninety people; one of them is Alfred; and we know that thirty of these people are unmarried, without however knowing which of them are unmarried and which of them are not'.

As Lyons subsequently observes (1977:805-806), subjective epistemic modality is excluded from conditional clauses because it is not part of the propositional content: 'everything that comes within the scope of the conditional operator ("if *p*, then *q*") must be part of the propositional content'. Lyons has changed his example at this stage, but we can apply what he says to (106) as well. In the subjective interpretation, it might be followed by (107i), in the objective interpretation by (107ii):

- (107) i *If he is, we could invite him along too*
- ii *If he may be unmarried, let's add his name to our mailing list*

In the first case, the *may* does not contribute to the propositional content but expresses the speaker's qualification to the proposition "Alfred is unmarried"; in the conditional construction this proposition is not asserted and hence that qualification would be out of place. In the second case, the possibility is part of the propositional content and hence the *may* is retained in the conditional. (107ii) might be said by someone running an introduction agency: if there exists a possibility that Alfred is unmarried then it is worth putting his name on the mailing list. Lyons points out that the subjective interpretation is much more common than the objective — which accounts for the fact that 'few linguists have even considered the possibility that epistemic modality could be anything other than a matter of the speaker's attitude towards the propositional content of his utterance [and for] the common, but strictly speaking false, statement that the modal verbs cannot occur with epistemic function in conditional sentences in English'.

This distinction between subjective and objective epistemic modality accounts precisely for the distinction between (104i and ii): (i) corresponds to main clause *The slick will come as far Stavanger* interpreted with objective modality, (ii) corresponds to it with the more usual subjective modality. Contrary to what Matthiessen & Martin are saying, therefore, the facts concerning the occurrence of *will* in conditional constructions provide extremely strong evidence AGAINST the IFG analysis.

Their argument (b) refers to 'temporal' conditioning as well as 'logical'. I take it that the *if*-construction we have been discussing is the logical type, and that the temporal construction is that illustrated in

- (108) i [*Don't forget to give me the change*] *when you get back*
- ii [*Give me a ring*] *just before you leave*

Here too the evidence points to the opposite conclusion from that which Matthiessen & Martin draw. If *will* can be a non-modal future tense marker, why can't it occur in constructions like these? There is no explanatory answer to this, as far as I am aware. But if we take *will* to express epistemic modality (in the broad sense envisaged by Palmer in the above quotation) then a very natural explanation is available. The temporal clauses here are subordinate, and subordination is usually accompanied by varying degrees of what Lehmann (1988) has called 'desententialization'. More specifically, they have 'lost' their illocutionary force — or, to put it in non-dynamic terms, they differ from ordinary main clauses in not having any illocutionary force associated with them. Thus when I say (108) I EXPRESS but do not ASSERT the propositions that you get back and that you leave at some unspecified future time (cf. Searle 1969:29). More precisely, these propositions are pragmatically presupposed, taken for granted, and that is why the modal qualification expressed by *will* is out of place — and why they are not readily

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accessible to challenge in discourse: see the discussion of (31) above. And note again the distributional similarity with epistemic *may* or *must*, which likewise do not appear in this construction.

Matthiessen & Martin's argument (c) is that 'modal *will* alternates with other strategies for expressing modality — for example, *I think* and *probably* [whereas] future *will* does not enter into a paradigm filled out by interpersonal metaphors'. IFG's modalization *will* is not, however, in paradigmatic contrast with *probably* or *I think*: it can combine with either. And in this it is just like the so-called future tense *will*. This argument is semantic rather than grammatical, but it is invalid in either case. It implies that with present time situations *will* indicates median modality (probability) and with future time situations it indicates no modality at all, a distinction I have already argued to be unsustainable.

Matthiessen & Martin's final argument, (d), is based on the examples

- (109) i *She'll like fairy tales, does she?*
 ii **It'll rain later, does it?*

The *will* of modalization, unlike that of futurity, is said to allow a simple present in the tag. Matthiessen & Martin do not acknowledge that there is anything unusual about (109i), but it undoubtedly represents a marked departure from the normal host-tag relationship: normally, an operator in the host is repeated in the tag. What makes (i) nevertheless just possible is that there is, in context, little pragmatic difference between *She'll like fairy tales* and *She likes fairy tales*: the tag reflects the structure not of what was actually said but of what might easily have been said. The pragmatic difference between *It'll rain later* and *It rains later*, however, will normally be much greater and hence there will be nothing to license the marked tag. But if we choose an example-context pair where there is little pragmatic difference between the *will*-form and the simple present, then the marked version of the tag will be comparable in acceptability to (109i). Suppose, for example, we are watching a video-movie for the second time. In this context there's not a lot of difference between (i) and (ii) in

- (110) i *She will recover soon*
 ii *She recovers soon*

One could therefore imagine someone saying *She will recover soon, doesn't she?* Or, to take an example where a simple present with future time reference is perfectly normal, *There'll be a full moon tonight, isn't there?* There is no firm data from the tag construction that could possibly justify drawing a distinction between modal and non-modal *will*.

In addition to presenting these four arguments, Matthiessen & Martin invoke the support of Palmer and Wekker. 'Even Palmer (1974), who rejects the future tense in English on morphological grounds, notes that future *will* differs from other types of *will* in important respects'. Apart from the fact that Palmer's grounds for rejecting the future tense analysis were not morphological, it must be emphasized that however one interprets his 1974 discussion, his later work very clearly takes the view that the *will* of futurity is semantically as well as grammatically modal. This is quite evident from the passages I have already quoted from his 1983 and 1986 works: what he says there undeniably argues against Matthiessen & Martin's position on semantic grounds, and there is no sense at all in which they can invoke him in support of IFG.

As for Wekker, they speak of 'the important discussion' of grammatical arguments in favour of distinguishing between modal and future tense *will* in his book of 1976. That implies endorsement of Wekker's arguments — in general, if not in every detail; in fairness, therefore, we should examine what Wekker has to say. As a framework for his discussion he adopts Palmer's distinctions (1974:15-18) between complex and simple verb phrases and between primary and secondary auxiliaries. Complex phrases like *wants to take* contain 'catenative verbs' (*want* in this example), whereas simple verb phrases contain only a lexical verb together with any auxiliaries. The primary auxiliaries are *be*, *have*, and *do*, while the secondary

auxiliaries are the modals. Wekker's claim is that future *will* (and *shall*) belong with the primary auxiliaries, not the secondary ones. He gives six arguments (1976:14-17).

The first is that 'passivization of sentences containing the *will* and *shall* of futurity is always possible'. By this he presumably means that passivization does not change the propositional meaning: he contrasts future *John will meet Mary* and *Mary will be met by John* (equivalent) with volitional *John won't meet Mary* and *Mary won't be met by John* (non-equivalent), and goes on: 'since passivization is only normal with simple phrases without modals or with phrases containing primary auxiliaries only, this is a particularly important piece of evidence of the primary, non-modal, status of future *will* and *shall*'. But the premise here is just false: passivization is quite normal with epistemic modals: compare *John may/must/can't/will have met Mary* with *Mary may/must/can't/will have been met by John*, and so on. Passivization provides no evidence at all for distinguishing a future tense *will* from an epistemic modal *will*.

Wekker's second argument is based on the fact that the future tense auxiliaries do not normally occur in temporal and conditional clauses; this is the same as Matthiessen & Martin's argument (b), and we have already seen that these constructions provide very strong evidence for a modal rather than a future tense analysis of *will*.

The third argument is based on observations made by Lakoff (1970:839-841). The examples Wekker gives are taken from Palmer (1974:104), but in order to match them up with Lakoff's commentary I will revert to the latter's examples:

- (111) i *The animal you saw was a chipmunk: see there he is running up a tree*
 ii *That thing rustling in the bushes over there will no doubt be a chipmunk: let's wait till it comes out*
- (112) i *The animal the sorcerer got hold of was a chipmunk, but now it's a badger*
 ii *The embryo Dr. Sharp is chuckling over in his laboratory will no doubt be a chipmunk, if the cat doesn't get it first*

Lakoff (who adopts a past/present/future tense analysis) regards the underlined forms in (111) as 'false tenses', in contrast to the 'true tenses' in (112) — yet it is the occurrence of *will* in this 'false tense' construction that Wekker is invoking as an argument that it is a tense auxiliary. Lakoff's commentary on (111ii) goes as follows: 'the identity of the creature is not yet clear in the mind of the speaker, though the fact that something indeed exists at the moment of the utterance is uncontroversial. The speaker of this sentence, aware of the rustling, has nevertheless not connected it with certainty to the existence of a chipmunk'. This is to say that the semantic difference between (111ii) and the corresponding sentence with *is* in place of *will be* is modal, not temporal: surely the occurrence of *will* in this construction cannot be used as evidence FOR a future tense analysis. Notice, moreover, that the difference between (111ii) and (112ii) is in the context; the Palmer/Wekker example, *The man you will be talking to will be the Mayor*, could be interpreted in either way, but I can see no reason to accept that it is strictly AMBIGUOUS, rather than merely non-specific, as to whether the person in question is already Mayor at the time of speaking. There is nothing in the data here to support the view that *will* expresses an unmodalized future.

Wekker's fourth argument is that the past tense form of *will*, *would*, is not commonly used to express 'future-in-the-past, except, of course, in reported speech or as free indirect style'. To put it in the terms used above in our discussion of tense, the past time use of the past tense is not found with the *will* of futurity. But it isn't found either with the narrowly epistemic *will* of *That will be the plumber* or the epistemic *may* of *That may be the plumber*: there is no evidence here for drawing a distinction between tense and epistemic modality. Epistemic judgements are typically subjective, as we have seen, and it is therefore not surprising that they should be linked to the present.

The fifth argument is that 'unlike *may*, *must* and *can* for example, *will* and *shall* cannot undergo double negative marking. This means that there is only one way of negating the verbal group. Thus, there seems to be no possible distinction between negating *will* and negating the full verb

come, in *He won't come tomorrow.*' But again there is no evidence here for isolating a future will from the modal system. In the first place, Wekker seems to be talking about *will* in general, rather than specifically future *will*: the quotation contrasts *may*, *must* and *can* with *will* and *shall*, and the example given, *He won't come tomorrow*, presumably involves volitional *will* (for it is not distinguishable from Wekker's earlier volitional example, *John won't meet Mary*). Certainly there are no differences with respect to negation between future *will* and narrowly epistemic *will*, as in *He won't have seen her yet*. It is worth noting in this connection that Halliday characterizes the median value of modality (to which he assigns modal *will*) as 'that for which the negative is freely transferable between the proposition and the modality' (IFG:337), as in the pair *It's likely Mary doesn't know* and *It isn't likely Mary knows*: the implication is that with *will* too there is no contrast between propositional and modal negation. A second point is that the negative scope contrast we find with *may*, illustrated in the ambiguity of *You may not see her again*, involves different kinds of modality expressed by *may*. The two salient interpretations are "It may be that you will not see her again" (with epistemic *may* outside the scope of negation) and "You are not allowed to see her again" (with deontic *may* inside the scope of negation). Deontic *may* does allow a contrast, with "You are allowed not to see her again" a further possible interpretation (associated with a prosodically marked reading), but there is no possibility of a scope contrast in the epistemic case. The same holds for *can*. Thirdly, with *must* we do not have scope contrasts in either the epistemic use, as in *He mustn't have read it properly*, or the deontic use, as in *You mustn't tell them*. In respect of negative scope contrasts, therefore, there is absolutely no evidence to support Wekker's future tense analysis: there is 'only one way of negating the verbal group' in all the epistemic cases.

There is nevertheless another kind of difference at issue here, illustrated in the pairs:

- (113) i *It will finish tomorrow*
 ii *It will not finish tomorrow*
 (114) i *It may finish tomorrow*
 ii *It may not finish tomorrow*

In (113) one would be inclined to say that (i) and (ii) are contradictories — the statements they might be used in a given context to express could not both be true and could not both be false. But in (114) (i) and (ii) are clearly not contradictories — each indeed is an implicature of the other (cf. Levinson 1983:140-141). The fact that the *may*-pair are not contradictories is clearly due to the fact that in (ii) the negative is within the scope of the modal. The seemingly contradictory nature of the *will*-pair might therefore suggest that in (113ii) the negative has scope over the modal. I do not think, however, that this is the right conclusion to draw: I would see it, rather, as reflecting the fact, noted above, that *will* is the most neutral of the modals, the one where there is least difference between a modalized clause and its unmodalized counterpart. The *will* could then be outside the scope of the negation without blocking the lay intuition that (113i) and (ii) are contradictories. It must be emphasized, however, that this lends no support to a future tense analysis, because the narrowly epistemic *will* behaves in the same way:

- (115) i *He will have seen them yesterday*
 ii *He won't have seen them yesterday*

These appear to be no less contradictory than (113). Negation does not therefore provide the distinguishing feature that Wekker is looking for.

The final argument involves short answers given by informants to the question *Do you think Mary might go?* The informants were asked to complete the four frames:

- (116) i *Yes, I think she* ____
 ii *No, I don't think she* ____
 iii *Maybe she* ____
 iv *No, she* ____

The 15 informants gave the following responses: (i) 14 *might*, 1 *will*; (ii) 12 *will*, 3 *would*; (iii) 1 *might*, 10 *will*, 4 *would*; (iv) 14 *won't*, 1 *wouldn't*. Wekker asks where the *will* comes from in the responses, and suggests that it might be an underlying future tense element which is deleted in certain environments, such as after *may* and *might*, but appears intact in the above responses when the *might* instead is deleted. The idea that *might* is derivationally deleted from the responses is surely untenable: it is not a matter of its meaning being understood while not overtly expressed. Its non-occurrence in the negatives (ii) and (iv) reflects the fact that *She might not come* is not the contradictory of *She might come*, but an implicature of it (cf. the discussion of (114) above). Thus one cannot express the opinion that "She might come" is false by saying *She might not come*: one needs a formulation that excludes the possibility of her coming. *Won't* serves this purpose, but that doesn't mean that *will* is a future tense element. The data are equally consistent with it being a 'strong' modal: recall here the argument that narrowly epistemic *will* in *That will be the plumber* is inconsistent with a following *but perhaps it isn't*. (*Might* is not excluded from (iii) but, as Wekker himself points out, it is redundant, and hence it is natural to use the more neutral *will* in the response.)

One feature of the above data that is of some interest is the occurrence of *would* among the responses to (ii)-(iv). I have pointed out that one significant difference between the modals and other verbs is that the factual remoteness use of the past tense is found with the modals much more frequently, and in a much less restricted range of grammatical contexts, than with other verbs. Most notably, it is found in main clauses. I illustrated this earlier with unreal/remote conditionals, but it occurs in non-conditional contexts too. Compare:

- (117) i *You are right*
 ii *You may be right*
 iii *You might be right*
 (118) i *He's seventy*
 ii *He'll be seventy*
 iii *He'd be seventy*

Suppose I ask you a question, you give an answer, and I respond with (117). The most favourable response is of course the unmodalized (i), but most speakers perceive a difference between (ii) and (iii), such that *may* is more favourable than *might* — the latter presents the possibility of your being right as somewhat less than the former. This extra modal qualification comes from the past tense in its factual remoteness use. (118) might be used in response to the question *How old is he?* (i) presents his being seventy as something I know: there is no epistemic qualification. It is more assured than the others, but again there is a difference between (ii) and (iii), the latter being somewhat less assured than the former. The significance of the *would* responses in (116) is that we find this same modal use of the past tense associated with the *will* of futurity: it supports the case for putting future *will* with the modals.

I conclude, therefore, that none of Wekker's six arguments provide any evidence, grammatical or semantic, for selecting one use of *will* as a tense marker in contrast to all the others, where it is modal. Some of the phenomena he discusses are equally consistent with either analysis, but three of them — the sixth, the third, and especially the second — in fact provide evidence for the modal analysis.

One final point to make about the IFG analysis is that it forces us to make a distinction in many cases where the data will be indeterminate. On what basis, for example, do we decide that *She will arrive tomorrow* is non-modal future while *John will take you home* is non-tensed modal (to use the IFG examples cited in (90))? It is remarkable that Halliday is willing to pick out one use of *will* and polarize it from all the rest when the boundary is so fuzzy, whereas he does not make the much more straightforward distinction between temporal and modal uses of the past tense illustrated in (95i) vs (iii).

A REPLY TO MATTHIESSEN & MARTIN

It might be open to Matthiessen & Martin to say that the three issues discussed above concerning *know how to*, *have to*, and *will* are merely descriptive. In a sense this is true: nothing in the Systemic-Functional model itself requires the analyses I have been arguing against. But there is another sense in which it is very much a theoretical issue — and this is why I have felt justified in dealing with the *will* issue in such detail. These cases are not like that, for example, of how we analyse the 'accusative + infinitive construction' illustrated in *She believed them to have been lying*, where it is difficult to decide whether *them to have been lying* forms a single complement of *believe* or a sequence of two — there are arguments for both analyses. In the three cases discussed above, however, it seems to me that the evidence against the IFG analysis is quite overwhelming: I cannot see any grounds for regarding them as borderline. If Matthiessen & Martin nevertheless opt for the IFG analyses, this would seem to indicate a major theoretical difference with respect to the evaluation and justification of descriptions: it would be interesting to know precisely what it is about the above argumentation that they find invalid. In this connection it seems to me significant that among the four arguments that they mention one is repeated from the Wekker discussion — and that the one they have chosen to repeat from his six (the one concerning restrictions on *will* in conditional and temporal clauses) is precisely the one which from my perspective provides the strongest evidence AGAINST the future tense analysis. Matthiessen & Martin make a generalized accusation (beginning of §III.3) that I judge IFG against an inappropriate conception of grammar and its goals: how and why is the above argumentation inapplicable to Systemic-Functional Grammar?

It is worth adding here that the three-term recursive tense system has been a feature of Halliday's description of English from a very early stage, but I am not aware of any attempt during these thirty years to provide any rigorous justification of it against the two-term non-recursive system presented in Palmer (1974) and numerous other modern studies. Matthiessen & Martin's half-page discussion does not suggest any awareness on their part of what would be needed to fill that gap.

Note finally that although the essence of Halliday's verbal group analysis has remained unchanged for thirty years, IFG shows some significant differences of detail from earlier accounts: see Halliday (1976a,b,c). The modal operators given in the first two of these (Halliday 1976a:126, 1976b:141), written in the 1960s, are:

- (119) i *can, could, would, should, may, might*
- ii *am/are/is/was/were to, ought to, used to*
- iii *need, dare*

The IFG list quoted as (83) above differs from this in four respects, all of which seem to me to be semantically rather than grammatically motivated. On the one hand, *will* and *has/had to* have been added, and on the other *dare* and *used to* have been dropped. The grammatical similarity between *dare* and *need* (both of which can of course also be lexical verbs) is so great that it is difficult to see what could have led to the dropping of *dare* other than the fact that it does not express any of the clearly modal concepts. *Used to* likewise does not express modality, but whereas *dare* has been dropped altogether from the operator class, *used to* has been shifted from the modal to the temporal operators. There has, however, been no change in the tense system to accommodate it, so that no systemic account is given of the difference between *He lived there* and *He used to live there*. The list given in Halliday (1976c:192), an extract from his 1970b paper, is:

- (120) *will, would, can, could, may, might, should, must, ought to, need*

This is much closer to the IFG list, but differs in not including *is* (etc.) *to* (which is said to be a 'quasi-modal' not a 'true' modal) and *has/had to* (which is not mentioned at all). An interesting feature of this 1976c account is that it explicitly enumerates the 'well-known' grammatical properties of modals. The list is similar to my (85)-(86) above, and as argued there clearly does not justify the inclusion of *has/had to*. It would be very instructive, for those wanting to understand the historical development of Halliday's thought, to have a detailed explanation

of these changes; as I see it, they reflect a shift to a model of grammatical analysis where semantic criteria are allowed to outweigh grammatical ones.

5.4 Ideational functions in clause structure

Having discussed the verbal system at such length I will confine my remarks under the present heading to clearing up a major misunderstanding. Halliday argues that material and mental process clauses are grammatically distinct in English. He gives a number of allegedly grammatical differences between them, the first being that mental process clauses always contain one participant that is human or 'human-like'. 'Expressed in grammatical terms', he goes on, 'the participant that is engaged in the mental process is one that is referred to pronominally as *he* or *she*, not as *it*' (p. 108). I observed in my review that this property 'involves what in the generative literature is called a selectional restriction, and these have been compellingly argued to be semantic rather than syntactic in nature' (p. 168), and followed this up by noting that exceptions to the generalization that Halliday expresses in 'grammatical terms' — examples like *It can't see us* — are not ungrammatical. Matthiessen & Martin seize on the fact that I did not acknowledge that Halliday admits this himself, and ask: 'Whose position is Huddleston arguing against? Surely it cannot be Halliday's' (§II.4.1 (1)). But it can, and is. The issue is not whether Halliday claimed they were ungrammatical, but whether they are — and the fact is (as we are agreed) that they are not. This is what differentiates the ideational functions from such interpersonal functions as Subject: the grammatical properties which — at the language-particular level — define the Subject in English do involve grammaticality. **Him died*, **You is welcome* and the like are ungrammatical. Hence my argument that the ideational functions are 'not characterized by distinctive properties that are at all comparable with those that define the Subject ... This difference in the nature of the distinctive properties cannot be explained by saying that the first four are ideational functions while the last is interpersonal [but indicates, rather] that the Subject is a grammatical functional while the others are not' (p. 169). Matthiessen & Martin do not see that the difference I am concerned with is whether the properties determine grammaticality. I made no suggestion that the distinctive features defining some grammatical function need to be concerned specifically with case, verb-agreement, position in declaratives and interrogatives, and so on, but only that they need to figure in rules distinguishing grammatical from ungrammatical. The ideational functions differ from the Subject in that regard: they are not grammaticalized in that they are not defined by reference to features involved in rules of grammaticality.

6. Conclusion

Matthiessen & Martin begin and end their paper with quotations from Foucault concerning polemical writing, in which the writer confronts the other person as 'an adversary, an enemy who is wrong' and has as object to abolish him 'as interlocutor, from any possible dialogue'. I emphatically reject the implication that my review falls into the genre described by Foucault. In particular, the implication that I regard Halliday as an enemy to be defeated is wholly without foundation and deeply offensive. On the contrary, I have a great admiration for him, and acknowledged in my review 'my own deep indebtedness to [him], with respect both to his influence on my thinking in linguistic theory and the grammar of English and also to the practical help he gave in my postgraduate and postdoctoral career' (p. 140, n. 2). I emphasized at the outset my view that *IFG* 'contains innumerable original insights and valuable observations: it has a great deal to offer anyone interested in the grammar of English' (p. 140). Nor am I hostile to systemic grammarians in general: I am currently engaged in a research project on English grammar involving a team including, at my invitation, two scholars who have worked closely with Halliday in recent years, Peter Fries and Peter Collins.

When I was a student and junior colleague of Halliday's in the sixties I was able to argue and disagree with him in an amicable and constructive atmosphere; it is distressing that when I now express in print my disagreement with the way his theory has developed I am treated — not by him, of course, but by Matthiessen & Martin — in a manner befitting some kind of traitor. For it

is ironical and sad that it is their paper that bears a remarkable resemblance to the polemical writing described by Foucault. I have noted numerous places where there are gross and blatant misrepresentations of what I said; I have to assume that these are not wilful but it is impossible not to believe that they stem from a consciously hostile reading of my review. Matthiessen & Martin certainly give the impression of being more interested in trying to belittle the critic than in answering the criticisms. They frequently use expressions such as 'simple' or 'of course' to trigger derogatory implicatures, and in a couple of places they descend to what can only be described as a level of abuse: 'Halliday's interpretation of Subject in English differs sharply from the traditional notion and more recent notions and, predictably, Huddleston objects' (§II.3.2), where the 'predictably' implicates that I am objecting simply because the interpretation is new and different, or — more explicitly — 'the pathology of Huddleston's critique (i.e. if an idea is new and different it must be bad)' (§II.4.1.(ii)(3)). I do not believe that this is a fitting way to conduct a debate in an academic journal: abuse is not an acceptable substitute for reasoned argument. If Matthiessen & Martin choose to take up again the issues discussed in this paper, I urge them to adopt a less emotive, more co-operative approach.

It should be emphasized, in conclusion, that many of the queries and objections I raised in my review are to be found also in Hudson's review of 1986.¹⁷ The remarkable similarity between our reviews no doubt reflects our shared background: we both worked with Halliday during the sixties, using his model of grammar as the framework for our research, and we both came to question it and move away, though in different directions. Hudson tends to express himself in more forceful terms than I do, but there is no great difference in the balance between positive and negative evaluation to be found in our reviews. Nevertheless, in what could be used as a textbook example of misleadingly selective quotation, Matthiessen & Martin pick out one 'quite positive' passage from Hudson to contrast with my 'unproductively negative' comments (§I.5). This is the only reference to his review and as such gives a totally false impression of the relation between the reviews. The paragraph following that from which the Hudson quotation is taken (p. 794) begins: 'The shortcomings are, unfortunately, numerous', and his summary evaluation, six pages later, reads as follows:

(121) In summary, then, *IFG* is a disappointment. There are lots of good ideas and insights which have been poorly developed and presented. If the intended user is the nonlinguist with a need to analyze texts grammatically, *IFG* may be helpful, but I doubt it — it would be very hard to use as a reference manual, too many categories are vague, and too many analyses are debatable. If the debatable analyses turn out to be wrong (in some sense), then I suppose it goes without saying that it is worse than useless to apply them to texts. If on the other hand the intended reader is the professional reader or student of linguistics, it is unlikely that *IFG* will make much impact.

That is hardly 'quite positive'.

The general criticisms that Hudson makes include: the failure to provide justification for the analyses, especially where they break with established tradition (p. 796 — he speaks of *IFG*'s 'ex cathedra tone'), the lack of 'any attempt to anticipate predictable objections to the less standard analyses' (p. 798); the 'many places in the book' where the categories are too vaguely defined for one to be able to identify instances of them; the failure to address the question of 'how we decide which of two competing analyses is the nearer to the truth'. More specific points include: (i) the vagueness of the definition of Theme as 'what the clause is going to be about' or 'point of departure', and the lack of any principled basis for connecting the unmarked Theme of imperative *You keep quiet*, namely *you*, with the 'I want you to do something' meaning; (ii) the vagueness of the definition of Subject, as noted above; (iii) the lack of any clear principle governing the choice between $\alpha \beta \gamma$ and $\alpha \beta \alpha \beta \beta$ (p. 808); (iv) the inconsistency in the interpretation of the $\alpha \beta \gamma$ representation in the clause complex and in the nominal group; and so on.

Given that there is so much common ground between my review and one written by a linguist of Hudson's standing and achievements, it is unfortunate that Matthiessen & Martin have been so ready to dismiss my objections and questions as simply reflecting an inability on my part to see

what is obvious or a knee-jerk rejection of what is new. They have had virtually unlimited time and space for their response: my review appeared over three years ago, and they have been able to perform the unusual feat of writing a response considerably longer than the review it was responding to. There was an excellent opportunity to clarify a range of issues where Systemic-Functional Grammar is unfathomable to outsiders and to justify *IFG* theory and description in the face of the reasoned arguments that have been levelled against it. That opportunity, unhappily, has been missed.

Footnotes

¹ It would not be helpful to organize my paper in such a way as to list these all together, but as they arise during the course of the discussion I will annotate them as '[Misrepresentation₁]', and so on, in order to substantiate this claim. The tone of Matthiessen & Martin's paper and the scale of misrepresentation are such that I felt it important to write my reply in time for inclusion in the same volume: I am grateful to my wife and children for putting up with the period of virtually total neglect this has necessitated.

² I use the notation $\alpha \beta \gamma$ without any implication of sequential order; when it is relevant to specify the order I shall use Halliday's ^ symbol: $\alpha^{\wedge} \beta^{\wedge} \gamma$.

³ There are no examples of this structure in 'TS', but Halliday does have $\beta <\gamma> \alpha$, with γ included in β , which is similar (e.g. *If before I'd left you'd telephoned I'd have come*, p. 37).

⁴ Hudson has developed a non-constituency dependency model where the answer to this question would be 'words' (he contrasts his model with Halliday's in his 1986 review of *IFG*), but there is no reason to think that Matthiessen & Martin take that view.

⁵ And it was, after all, Halliday's work that I was reviewing, not Hudson's or the whole systemic literature: I don't know why Matthiessen & Martin should find it 'strange' that I don't draw attention to Hudson's analysis.

⁶ To say that it is comprehensible is not to say that I accept it: see next paragraph.

⁷ I have substituted italics for Lyons' single quotes to bring the notation into line with that used in this paper.

⁸ At least not in the relevant sense of *nothing*. It might be possible to get *as for nothing* in a philosophical discussion, with *nothing* interpreted as "nothingness", French "le néant", for in that sense it is a possible topic.

⁹ I am grateful to Higuchi Hisashi for the Japanese data. I would also like to acknowledge valuable help from Jill Bowie in discussion of the concept of Theme.

¹⁰ The actual wording he uses is 'The meaning is "I want you to tell me ..."', but there can be no doubt that 'meaning' here is validly interpreted as Theme_C. On p. 48 he says: 'the natural theme of a question is "I want to be told something"'; and this interpretation matches the more explicit account given in 1967:212-213.

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¹¹ I would not agree that *didn't* is not a surface-constituent (I would take it to be an negative inflectional form of the verb lexeme *do*), but that clearly does not affect the argument. Nor would it undermine Bazell's point to note that Halliday does not regard occurrence in first position as 'defining' Theme: it is still the criterion which determines what element is Theme.

¹² Note also, in connection with our earlier discussion of the existential *there*-construction that the Tagalog equivalent of this construction has no topic element (Schachter & Otanes 1972:81).

¹³ That Halliday would take (75ii) to be grammatical can be inferred from his use of the example *That John had run away no-one believed* on p. 197.

¹⁴ I use bold italics for the verb considered in abstraction from any particular inflection — i.e. for the lexeme.

¹⁵ *Must* does not readily occur in an unreal apodosis, but there is nevertheless a clear difference in acceptability between *If he had been at the meeting he must surely have told her* and **If he had been at the meeting, he had surely to have told her*. The marginality of *must* in this construction is attributable to the fact that it does not exhibit the inflectional contrast that we find in *can* (*can/could*), *may* (*may/might*), *will* (*will/would*); *have* of course does exhibit this contrast (*has/had*), and thus if it were a modal operator one would expect it to occur freely in an unreal apodosis.

¹⁶ As we see from (83), IFG puts *ought* higher than *will*, but the acceptability of *He ought to have finished it yesterday, but he didn't* in contrast to the unacceptability of *He will have finished it yesterday, but he didn't* is evidence that *will* is stronger, not weaker, than *ought*.

¹⁷ The two reviews were written independently. Hudson's was completed before mine, and the journal containing it reached me just as I was making the final corrections to my MS, enabling me to add a short reference to it on p. 139.

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