

REVIEWS

ROGER BROWN, *A first language: the early stages*. Cambridge, Mass.: Harvard University Press, 1973. Pp. xi + 437.

It is particularly appropriate that the first volume of this journal should carry a review of this book, for Roger Brown has done more than anyone else in the past decade to stimulate and direct research into psycholinguistic approaches to child language. But the quantity of his work brings its own problems for the student of child language, for the publications of Brown and his colleagues have been scattered throughout a wide range of books and journals, and it has accordingly been difficult to develop a comprehensive picture. Brown has done everyone a service in drawing together all the published reports, theses, working papers, etc., and attempting to present a coherent and comprehensive account of the state of child language research – at least, in respect of those aspects with which he has been most closely associated. It is my belief that he has succeeded. The book provides a fine mirror of the aims, achievements, weaknesses and failures of the 1960s. It can safely be used as a foundation coursebook or orientation text. It is, above all, a basis for continuing the discussion.

This is the first of two volumes which will together provide an account of Brown's view of five stages of language development. The 'early' stages referred to in the sub-title are I, 'Semantic roles and grammatical relations' and II, 'Grammatical morphemes and the modulation of meanings'. Stages III, IV and V (Book 2) will deal with simple sentence 'modalities' (questions, imperatives, negation, etc.), embedding (complementation, indirect questions and relative clauses), and coordination, respectively. Each stage corresponds to what Brown sees as a major process of sentence construction and understanding; a stage 'derives its name from the process that seems to dominate it' (180). The process may be apparent in other stages too, but its developmental path is concentrated at one point. The five stages constitute 'the core of English sentence construction and . . . of language generally' (32). The book is therefore almost totally about syntax, and those aspects of semantics arguably mediated by syntax. There is next to nothing in this book about the development of pronunciation or vocabulary. Two main aims are involved: 'to present evidence that (1) knowledge ['concerning grammar and the meanings coded by grammar'] develops in an approximately invariant form in all children ['and languages' (59)], though at different rates'; and (2) 'the primary determinants of the order are the relative semantic and grammatical complexity of constructions rather than their frequency or the way in which parents react to them' (58).

The book has four main sections: 'an unbuttoned introduction', the Stage I survey, the Stage II survey, and Conclusions, and I propose to go through the

book in that order. The introduction is unbuttoned, as he puts it, because he wishes to introduce in an informal way – and bearing in mind the needs of the non-linguist – enough information about the general characteristics of English sentence construction to underpin the descriptive analyses made as part of his survey of development. His way of doing this is to report on his own experiences of learning Japanese in a language-teaching course; by comparing the two languages he argues that, despite their many differences, they share the same general design characteristics. These are illustrated with reference to the five major processes named above. He then proceeds to a discussion of the performances of the ‘linguistic apes’, Washoe and Sarah. He gives a fairly detailed account of the very great differences between the approaches used with these apes, as a prelude to asking if their achievements match up to language. His discussion, with its largely negative conclusion, is full and fair – though in relation to Washoe I miss a critique of the premise of the whole exercise. The Gardners have tried to teach Washoe the American Sign Language, ‘a system which is believed to have most of the properties common to human languages generally’ (33). The reference for this claim is to an unpublished M.A. dissertation (though there are now more accessible sources, e.g. Stokoe 1972). But the claim is debatable, and needs to be debated. Is ASL a language in the same sense as spoken language? Evaluating the extent of the differences is a task which has only recently begun to be investigated (see the discussion in Crystal & Craig, forthcoming, for example), and meanwhile the question of exactly what it is that Washoe has been taught remains open. Certainly its relevance to any discussion of child language is questionable, and this whole section, in my view, is a distraction from the general line of argument being made by Brown in this book.

Brown proceeds (51 ff.) to give background information about the genesis of the research, the children used, and the main characteristics of his analytical apparatus. Stage I (61–245) opens with a useful collation of all the sources of data that can be interrelated within Brown’s framework of ideas, the desideratum being that the work should be based upon some explicit method of developmental ordering (such as mean length of utterance – discussed further below). In addition to Brown himself, that means Bloom, Bowerman, Kernan, Rydin, Tolbert; and Braine and Miller & Ervin are considered to be comparable – all of which provides 24 grammatical analyses of 18 children. Other work, especially by Leopold, Grégoire, Park, Bar-Adon, McNeill and Blount, is referred to. Given that Brown tells us so much biography about the data, it is a pity that some further evaluation of its adequacy was not given. For instance, how much actual utterance was there in the ‘4 hours, taped’, ‘12 play sessions, taped’, etc.? The children are evidently extremely variable, e.g. Kernan’s Sipili is 6½ hours, but only 852 utterances emerged. Rydin’s Viveka was taped for 4½ hours, and produced 895 (though it is unclear whether this is a sample or the total);

Tolbert’s Pepe, the only lower-class child, was taped for 4 hours, and he got 791. It is helpful to have such data brought together, but it is a pity that so many basic questions remain undiscussed. Should not the differences in quantity be somehow taken into account? Are these totals not extremely small for any discussion of grammatical norms? How much of the data was unusable or ambiguous? Above all, there is the question of what exactly Brown means by the term ‘data’. He considers all the biographical information about the children, samples, etc. to be ‘the primary data to which I shall make reference’ (74). But he does not often refer directly to this data. What he does regularly refer to are the ANALYSES made by Bloom, Blount, and the others – analyses which then seem to be subsumed under the heading of ‘data’ and largely taken for granted thereafter. But is it legitimate to rely so much on the analyses of others, especially when their theoretical foundations differ so markedly (e.g. Bloom vs. Bowerman)? I would have welcomed Brown’s views about the analyses made by his colleagues at this point, in addition to the basic information about their data.

Brown goes on to outline various ‘characterizations’ of the data falling within Stage I: first of all, two ‘lean’, non-semantic characterizations, and then three ‘rich’ characterizations, which all use semantic notions. (a) A characterization in terms of telegraphic speech is summarized, investigated, and found wanting without considerable redefinition of the criteria for establishing the grammatical ‘functors’ that are said to be omitted. (b) The notion of pivot and open classes is then examined systematically, first with reference to Braine, then Miller & Ervin, then Brown, Fraser & Bellugi, then McNeill; the various criticisms that have been made of it (the distributional evidence, the claim of its universality, and the question of its adequacy as a representation of the child’s grammatical knowledge) are outlined, and the conclusion is drawn that probably ‘the notion of pivot grammar should simply be jettisoned altogether’ (111). Characterizations of types (a) and (b) are so misleading, in fact, that they should be ‘dropped from introductory textbooks’ (403), and replaced by relatively ‘rich’ characterizations which, because of their semantic bases, Brown feels are much more superior and promising as a means of establishing the patterns in child speech. (c) Schlesinger’s analysis of early utterances in terms of a model of processes of sentence production and comprehension is outlined, and compared with Bloom’s. (d) This is followed by an outline of McNeill’s thesis postulating a set of grammatical relations; Brown pays particular attention to the notion of predication (which is compared with that of De Laguna) and to topic and comment. (e) Fillmore’s case grammar, which obviously impressed Brown greatly, is then outlined and applied in detail to the data from Adam.

The direction of the argument at this point, then, is towards establishing a basic set of semantic relations capable of accounting for the various utterances used by the child (or, as Brown puts it, for the child’s ‘intentions’). He reviews

the evidence of word-order (147 ff.)<sup>1</sup> in establishing these relations, and discusses their applicability to one-element utterances. The major meanings of Stage I, requiring at least two morphemes to be expressed, are then discussed in detail (168 ff.). He begins by explaining the 'pivot look' of early grammar as resulting from the child's regularly expressing three basic operations of reference, which he labels NOMINATION (e.g. *this, see, here*), RECURRENCE (e.g. *more, another*), and NON-EXISTENCE (e.g. *all-gone, no*). These are coordinated with other words to produce a set of minimal two-term relations, viz. Agent + Action, Action + Object, Agent + Object, Action + Location, Entity + Location, Possessor + Possession, Entity + Attribute, and Demonstrative + Entity. These may be further combined to yield 3- or 4-term compositions (e.g. Agent-Action-Object), or one of the terms may be expanded hierarchically (e.g. Agent-Object with Object expansion, as in *Eat nice lunch*). These, the 'prevalent relations' (178), account for about 70 per cent of most samples. A further seven relations of low frequency are outlined (179). Brown then gives three tables in which the number of cases of each relation is given. There is inevitably arbitrariness, but on the whole the statistics justify the breakdown into prevalent and 'other' relations (with the exception of the Samoan child, Sipili, whose 'prevalent' relations were only 30 per cent compared with 65 per cent of 'other' relations; and Eve and Sarah also have a very high proportion of 'others'). I would however have liked to see a more extended discussion of the problem of productivity at this point. When statistics get so low, as they do for 'other relations', as also for some of the concatenated prevalent relations and all of the expanded ones, I wonder whether there is enough support for talking about semantic types at all. It is likely that some of these relations were learned as unanalysed wholes, or 'prefabricated routines' (see Clark 1974). Brown accepts (181, also 212) that fixed routines have a part to play in explaining the early development of sentence modalities (interrogation, etc.), but this notion must be considered in general at this stage.

Having given a general outline of the main meaning relations, and their gross statistics of occurrence, and after an excursus on his theory of cumulative sentence complexity, Brown returns to a detailed examination of each relation. Most of his basic eight relations, I imagine, will make fairly obvious sense to most readers, and would not be considered particularly controversial. I did however have some difficulties over the grounds for distinguishing three of the relations: Entity + Locative, Entity + Attribute, and Entity + Demonstrative. Brown accepts (195) that '*here* and *there* can function, and in Stage I most often do,

[1] But I am unclear about the ultimate validity of his arguments about word order. Statements such as 'the child's first sentences preserve normal word order' make claims whose content is unclear. In the absence of an imitated model, and given some sentence, in the majority of cases it is impossible to see what would count as counter-evidence to the claim. For example, if we have a child who says, as daddy approaches in a car, *daddy car*, and then *car daddy*: whatever he means, the existence of such pairs makes it difficult to prove the above statement one way or the other.

as demonstratives of a sort'. On the other hand they are stated to be locative in discussing Action + Locative. And it is at least open to discussion whether locative at this stage cannot be considered as a sub-class of Attribute. The difficulty is one of which Brown is of course well aware - of attributing too much adult analysis to the child utterance - but a more systematic discussion of the criteria for making these distinctions would have helped. There then follows a brief discussion of the eight relations with reference to Piaget, Brown inclining to the view that they are universals arising out of an extension of the various properties of sensori-motor intelligence. And Part I of the book concludes with attempts to write a generative grammar of the data in terms of Schlesinger, Chomsky, Bloom and Fillmore, which I shall refer to in more detail below.

Brown's account of Stage II may be more briefly summarized. It begins with a discussion of the range of factors affecting the definition of functors, and outlines the categories whose development he is concerned with investigating. There are fourteen grammatical morphemes in all: present progressive, regular and irregular past, plural, articles, uncontractible and contractible copula, uncontractible and contractible auxiliary, possessive, regular and irregular 3rd person, and the two locative prepositions, *in* and *on*. Brown is careful to give a full discussion to the methodological problems involved in setting up a set of morphemes of this kind and establishing their order of acquisition. In particular he points to the considerable period of time which may elapse between the appearance of a morpheme and the point where it is almost always supplied when required. A decision has to be made as to when performance is sufficiently predictable to indicate that acquisition is fairly plausible, and he chooses (following Cazden) the very stringent criterion of 90 per cent accuracy (for three successive two-hour samples) in those environments where the use of the grammatical morpheme would be considered obligatory. The fourteen morphemes were selected 'primarily because obligatory contexts can be identified for them and because they are frequent enough to yield continuous data' (270). The categories are generally coherent (apart from the two groups of 'irregular' forms, where I see no grounds for putting together all the irregular past tenses and 3rd persons into single classes). Brown discusses each morpheme in turn, illustrates the order of their acquisition in three children, compares his results with other work using controlled and spontaneous data in English (the Berko test, in particular), and refers to what is known about the development of these morphemes in other languages. His general conclusion is that the order of acquisition is approximately invariant for these children, and he thus proceeds to a lengthy discussion of why. He postulates three possible determinants of the order of acquisition; grammatical complexity (specified in terms of derivations from Jacobs & Rosenbaum 1968), semantic complexity, and frequency of parental use. His conclusion is that parental frequency seems to have no influence on the order of acquisition, but that semantic and grammatical complexity does.

His point about frequency is well taken, but his discussion of grammatical and/or semantic complexity (it is sometimes difficult to say which is which) I found unnecessarily obscure, because I am not clear what a number of Brown's pre-suppositions are. In particular, I find his attitude to the notion of simplicity confusing. He spends a great deal of time working out which and how many features and transformations are involved in the derivation of his morphemes, but seems to reach little definite conclusion, other than to say (311), 'Generally, the constructions we have considered follow an order of acquisition that corresponds with a rough order of grammatical complexity. But complexity defined in terms of the numbers of features and rules, regardless of the character of the features and rules, may be meaningless.' None the less, statements based on this assumption abound in this part of the book, though one continually gets the impression that Brown is not particularly happy in making them, especially when he has to plug so many gaps in the Jacobs-Rosenbaum grammar for himself.

The book concludes with a discussion of some of the outstanding problems. There is a section on variability in the data - largely a discussion of the resemblances between his data and that of Labov and others on Black English - and one on segmentation problems in relation to certain anomalies in his data. There is a section summarizing his conclusions, and the book is rounded off with a bibliography and a very full index.

Apart from some of the minor critical points already made, criticisms of the approach which this book represents can I think be made under two general headings: inexplicitness in the notion of utterance, and uncertainty in discussing linguistic criteria of analysis. Taken together, they do not amount to a rejection of the whole of Brown's approach, nor of his empirical findings; but they do show the limited generalizability of some of his results, and do constitute, in my opinion, a powerful case for caution in accepting some of his theoretical conclusions.

#### *Mean Length of Utterance (MLU)*

The concept of MLU is central to Brown's approach. The various stages are derived from samples based on MLU, e.g. Stage I begins as soon as MLU rises above 1.0, and ends at 2.0; Stage II goes from 2.0 to 2.50, etc. The total shared developmental stretch of data was divided into five points 'as nearly as possible equidistant from one another in terms both of MLU and upper bound (56)'. 713 consecutive complete utterances at each point for each child were analysed in detail. Why 713? 'An accidental consequence of the size of the transcriptions from which the first samples were drawn' (56).<sup>21</sup> Brown used the notion in the first place because he felt that age would not be a satisfactory longitudinal control, rate of acquisition being so variable. 'MLU is an excellent simple index of

grammatical development because almost every new kind of knowledge increases length . . . especially if measured in morphemes' (53-4). This is said to be so for the stages recognized in his study. By Stage V, though, the child 'is able to make constructions of such great variety that WHAT he happens to say and the MLU of a sample begin to depend more on the character of the interaction than on what the child knows, and so the index loses its value as an indicator of grammatical knowledge' (54). MLU has basically a pragmatic justification: it is a useful comparative tool: 'two children matched for MLU are much more likely to have speech that is, on internal grounds, at the same level of constructional complexity than are two children of the same chronological age' (55). Brown accepts that more than one index of development is desirable, but in the absence of alternatives, the idea that any yardstick is valuable, even if the units of measurement are totally arbitrary, is eminently defensible.

But only if it is consistently applicable. And my main concern with MLU, having tried it on my own data, is that I cannot operate with it consistently. I keep having to make ad hoc analytic decisions. Brown admits it is 'quite inconsistent' in relation to adult language; but it seems to me not much less so for children either. To make it work consistently, Brown needs to be far more explicit, both about the notion as such, and about his way of working with it. The criticism warrants detailed justification, in view of its centrality to the approach, and the most convenient way of doing this is to make a critique of Brown's rules for calculating mean length of utterance and upper bound (i.e. the longest utterance), as given in Table 7 (54). The first rule runs thus: 'Start with the second page of the transcription unless that page involves a recitation of some kind. In this latter case start with the first recitation-free stretch. Count the first 100 utterances satisfying the following rules.' There is a minor query and a major one to be made about this. The minor point is that I would like to be told why the fluctuations which are apparently characteristic of the first page of transcription are any more significant than those occurring later in the texts. (And what is a recitation?) The major point is that I would dearly like to be able to count the first 100 utterances, if ONLY someone would tell me what an utterance is! It is staggering that nowhere in this book does Brown give us a definition of this central concept, or at least operational criteria for identification. But one of the first things that hits you when you analyse linguistic data - and particularly that of young children - is the problem of deciding where one unit of speech ends and the next begins. It is crucial to the distinction between holophrase and telegraphic speech, for instance, that the definition of utterance is well founded; or again, when Brown writes his chapter on coordination, decisions about whether two speech units are parts of the same sentence or are linked separate sentences will be very much dependent on utterance criteria being made explicit. It is not as if the problem is merely an occasional one: it shouts out at the analyst all the time. For instance, I do not know how many utterances to count

[1] Being accidental, it is therefore surprising how anxious some researchers are to stick to this sample size!

in Brown's terms in the following transcription of a 3-year-old playing with some toys (/ marks tone-unit boundaries):

and it goes up/ up the hill/ and it goes up/ in the hill/ and it takes us on up the hill/ and it goes up the hill/ up up up/ and crash/ crash down there down that hill/ I know how many I would count in MY terms, as I would use intonational and other criteria as part of the definition of my units – but these would perhaps not be utterances in Brown's sense. Brown is surely making use of these factors, but they are not referred to, nor are the utterances of Brown's children ever given in transcription (apart from a couple of stress contrasts later in the book). At various places he does refer to the importance of intonation (e.g. 153, 255, 338). For example, he seems to be of the opinion (which I share) that the prosodic features of language are of primary significance in accounting for language development in the early stages, when he says, 'it is the use of intonation contours to mark word sequences as in construction, rather than word order that is the single universal syntactic device of Stage I. And it is ultimately the relational interpretability of these constructions, heard in context, that justifies attributing relational semantic intentions to the child' (43). But if this is so, one would expect a much greater attention to be paid to the intonational facts of utterance, and, for instance, to providing transcriptions of otherwise unanalysable sentences, e.g. Gruber's *It broken, wheels* (referred to on p. 132), or *No, no Daddy chair, home soon* (196). (The commas seem to be being used in different functions in these examples.) To be sure that sentence pairs like *Gregory fix it* and *Fix it Gregory* display no semantic contrast (156), intonational markings are needed; and there are dozens of such problems in Brown's book. Whether the data are available, I do not know. He says at one point – surprising as it may seem – that only one of his children, Sarah, had a prosodic and paralinguistic transcription made of her data (52). And generally, he seems to be taking intonation to be much less of a problem than most linguists would (cf. his remark on p. 148, for instance): 'There is no problem ordinarily in distinguishing a two-word utterance from two single-word utterances because the child ordinarily controls prosodic features which make the difference obvious even to the phonetically untrained.' In short, it is unclear how systematic Brown has been in his use of prosody to define utterances. And in the absence of explicit guidelines, one must conclude that Brown is also relying on semantic intuitions to identify the utterances which are later to be given a semantic analysis. The dangers of circularity in such a procedure are evident; but in any case, if a semantic notion of utterance is permitted, then one wonders what distinction is to be made between the concept of 'utterance' and that of 'sentence'.

The sort of thing I would like to know about the Brown data, for example, is whether a given sample was ever independently analysed for utterances (not for transcription), and what the degree of overlap between the analysts was. Only by knowing the confidence limits of the method can one use the method with

confidence. The same point applies to Rule 2 for calculating MLU. 'Only fully transcribed utterances are used; none with blanks. Portions of utterances, entered in parentheses to indicate doubtful transcription, are used.' How much doubtful transcription was there, and what was the basis of the doubt? Analysis of doubtful cases often brings to light illuminating information, if not about the data, then about the analytical methods used. Also, how much untranscribable utterance was there, and why was it so? Anyone wanting to do a Brownian study would want to see whether their own samples were comparable in terms of situational obscurity. A simple statistic would suffice.

Rule 3 says: 'Include all exact utterance repetitions (marked with a plus sign in records). Stuttering is marked as repeated efforts at a single word; count the word once in the most complete form produced. In the few cases where a word is produced for emphasis or the like (*no, no, no*) count each occurrence.' But what constitutes an 'exact' repetition? Must it be segmentally exact only, or prosodically exact also? There is no clear dividing line between repetition for emphasis, hesitation, rethinking, stuttering, and so on. What does one do with cases like *that's my . my bus*?

Rule 4 states: 'Do not count such fillers as *mm* or *oh*, but do count *no, yeah*, and *hi*.' This is a good example of a 'rule' which cannot possibly work without intonational and other such criteria. *mm*, for example, can be either hesitation, phatic noise used while another is speaking, or a response. Presumably Brown would not exclude *mm* when used as a response, synonymous with *yeah*. But then, what is a filler?

Rule 5 says: 'All compound words (two or more free morphemes), proper names, and ritualized reduplications count as single words. Examples: *birthday, rickety-boom, choo-choo, quack-quack, night-night, pocketbook, see saw*. Justification is that no evidence that the constituent morphemes function as such for these children.' Here there are many problems. Again, some kind of prosodic criterion is needed to avoid the danger that morpheme-sequences for the child are assumed to be sequences of words, when the child may be using them as compounds – though compounds which are unfamiliar to adults. For example, how can one be sure that *daddy shoe* is not a compound without it? Brown's justification can tell us nothing about any given point in a child's development, as we only know what a compound is once some minimal semantic pair emerges – and this may not be for some time, or may never happen. There must have been many cases in Brown's data where there was no evidence of independent contrastivity, but items were none the less classified as simple or compound words. Indeed, bearing in mind the relatively small sample sizes involved, I would imagine that if he used his criterion rigorously he would end up with far more compounds than he would want to see. There is however a more serious problem. Rule 5 hints at how to handle compound words; but what are we to do with multi-morphemic items of greater length – idioms and 'prefabricated routines',

in particular? Here are a set of problem examples. *Good morning, How do you do?, It's raining cats and dogs, Rise and shine*, and most proverbs, rhymes, etc. What does Brown do with these? They cannot be counted in the same way as non-idiomatic constructions, for they lack the internal structural potential of such constructions (e.g. the usual range of transformations do not apply to *How do you do?* – \**How were you doing?*, etc.). So are they classed as a set of unanalysable items? There is no evidence that they are, and my conclusion is that Brown must therefore be grouping together what does not belong together, and that any descriptive statements must be treated with caution. I shall return to this point, but it is worth noting here that the problem of unanalysable utterances will get greater as children get older.

The problem of negative evidence arises again in Rule 6. 'Count as one morpheme all irregular pasts of the verb (*got, did, went, saw*). Justification is that there is no evidence that the child relates these to present forms.' I am sure he is right to take irregular forms as single items (but why does the rule restrict itself to forms of the verb? what about irregular nouns, adjectives, etc.?), but once again his criterion is unusable and would require, to be used consistently, the past tense of many REGULAR verbs to be counted as single morphemic items also – for surely not every regular verb will display clear relational evidence of the kind required either. A much better argument here would be to say, quite simply, that any morphemic analysis of these forms is bound to be arbitrary, and that the analysis which involves least structural assumptions is to be preferred. After all, while the morpheme concept has been of great value for some languages, mainly the agglutinative ones, it is generally recognized that English is not one of those languages in which the concept is straightforwardly applicable (as all the discussion of zero morphs, etc. in the 1940s indicated). (But nowhere in fact does Brown go into the justification for choosing morphemes as his unit of measurement, as opposed to, say, syllables, stressed syllables, intonational units or something else. Nor does he justify his choice of a particular morphological model. For my part, I would have thought that, if a measure of length is going to be used at all, then this ought to be as independent of matters of linguistic analysis as possible, to avoid charges of circularity. (Naturally MLU would be a good index of constructional complexity if the same linguistic notions were the basis of the definition of both.) Were alternatives to the morpheme, which figures so prominently in Brown's analyses later in this book, ever investigated? Many of the problems in these rules might have been by-passed in that way, as they seem to be consequences of the choice of a morphemic method.)

Rule 7 runs: 'Count as one morpheme all diminutives (*doggie, mommie*) because these children at least do not seem to use the suffix productively. Diminutives are the standard forms used by the child.' The switch from 'these children' to 'the child' makes one wonder how general Brown is intending these

rules to be; but in either case the rule seems unnecessarily strong, and extremely difficult to apply precisely, without some specification of what counts as productivity. Is Brown working with some notion of functional load here? No criterion is given. Nor do I see how Brown makes his notion of diminutive work. *dog-doggie* makes sense; but *mommie*, presumably from *mom*, does not. Why is *mom* not an abbreviated form of *mommie*? I see no reason why productivity should have been allowed to influence one's judgements about length in the first place.

Rule 8 runs: 'Count as separate morphemes all auxiliaries (*is, have, will, can, must, would*). Also all catenatives: *gonna, wanna, hafta*. These latter counted as single morphemes rather than as *going to* or *want to* because evidence is that they function so for the children. Count as separate morphemes all inflections, for example, possessive {s}, plural {s}, third person singular {s}, regular past {d}, progressive {ing}.' This rule is another dependent on vague functional considerations, and again, there are problems over applying it consistently. For example, what does one do with *aren't, won't*, etc.? And how does one show contrastivity to be operative? For example, on p. 71 he explains that *got* and *did* are not counted as having a past morpheme 'because my preliminary once-through on the data showed that these were not used contrastively with *get* and *do* and the like in Stage I nor were they used consistently in a semantically appropriate way'. But how can these notions of contrastivity, consistency, and appropriateness be made explicit? It is beyond me, and it is beyond Brown also. Having cited Zellig Harris's rigorous distributional criteria, he says simply that with child speech 'one can never meet these demanding criteria' (71). The honest analyst is thus left with two alternatives: either you give up altogether – and maybe aspects of child language study ARE intractable – or you give up claiming to be doing systematic linguistics. Brown does neither – though in fairness to him, his tone is wise and cautionary at many points in this book. Still, there is a world of difference between 'doing linguistics' and 'doing linguistically-sophisticated psychology'. If Brown wishes to give his analyses the ring of confidence which one can sometimes derive from a linguistic approach, then his methodological principles must be made more explicit.

A general point about the Brown MLU measure is that the rules which try to make it work are orientated solely towards English. This is defensible, given Brown's research field, but it of course raises the question of how far the notion can be applicable, without drastic modification, to other languages, particularly those that are typologically at a far remove from English, and where notions of 'filler', 'compound', 'diminutive', etc. may need redefinition. Most of the problems posed for English, as I have argued, seem to be a consequence of morphological problems. It thus seems likely that the more a language uses morphological processes, the more problematic the calculation of MLU will become. And so it is. On p. 68, we are told that 'Studies of highly inflected

languages, like Finnish, Swedish, and Spanish, all report some difficulty in adapting our rules of calculation, invented for English.' Brown says that the variation in values dependent on decisions not covered by his rules is small; but the figure given (0.10) is not trivial and cannot simply be dismissed. It should have been explicitly discussed, particularly in view of the widely held feeling that one of the most important tasks for child language research in the next decade is to substantially increase the amount of data on languages other than English. Many researchers will try to apply the Brown model, and they will be faced with a large number of problems, both of principle and of analytic detail, which (the danger is) they will 'solve' by arbitrary means. This has already happened, it seems, in the case of Park's study of German, reported on p. 71, who 'started out to calculate MLU values, but running into the problems that inflectional languages raise, he', sadly, 'gave up doing so' (71). Under such circumstances, it is humble, but not very responsible of Brown to say: 'Park's method of counting does seem to exaggerate the child's knowledge but we cannot afford to be critical of him because our own counting rules . . . are not really very well rationalized' (71).

None of this is to deny the pragmatic value of the descriptive norms and explanations discussed throughout this book, of course. On the whole, Brown's observations on the development of English match very well one's own usually much more fragmented observations. In our work at Reading on syntactic profiles for assessing language disorders, for instance (see Crystal, Fletcher & Garman, forthcoming), we have been using a notion of syntactic stages of development which is similar to Brown's in many respects, though it is not based on MLU, and aims to take in developmental information a little earlier and later than that covered by his five stages. We have found that most of Brown's descriptions correspond to those established for our children. But rather than seeing this as a proof of MLU's validity, we see it as a good reason for dispensing with it altogether. The only reason given for developing an MLU calculus is to avoid having to rely on age, rate being so variable (53, 72). That there is some variability is undeniable, but I do not see it as such a problem as Brown evidently does, and our evidence is that similar results to Brown's can be obtained without having to use a sampling superstructure which has creaky linguistic foundations. Brown himself sees MLU as one out of many possible 'interim external indices' of development, and looks forward to the day 'when we have found evidence of reliable internal semantic and grammatical change . . . we can identify a child's construction level in these terms and ignore the various external indices' (72). That day may not be far off.

#### *Problems of linguistic analysis*

A linguist can only admire the extent of Brown's linguistic knowledge, and wish that he had half as much awareness of psychology with which to buttress his own

failings. Perhaps one day Berlitz will open a cognition department for linguists! One of the most important features of this book, in fact, is the way in which the concerns of both disciplines are so lucidly and appropriately interrelated. He is none the less very aware of and ready to admit his own limitations in linguistics; when he is unsure of his linguistic ground, he says so. In no way, then, would I want to take issue with him on aspects of his approach which he is quite ready to concede are controversial or suspect. But there are a number of places where he seems to be adopting a linguistic position which might be criticized, without being aware of the grounds for the criticism. It is this kind of argument which I want to concentrate on in this section. The clearest example is in relation to his outline of generative grammars in the style of Chomsky, Schlesinger, Bloom and Fillmore, which I think is the least satisfactory part of the book. Brown begins by establishing certain sentence types as basic facts to be represented, 'maximally "uncommitted" to any particular formalism' (203), he claims. These are declarative, interrogative, imperative and negative. But he does not seem to be aware that negative for many linguists sits oddly with the other three modalities; it is a complex question whether negation is best viewed as a property of sentence, clause or phrase. Nor is any mention made of an exclamatory sentence type, though this is a major feature of grammatical tradition. And in fact he does not include all interrogative sentences under the heading 'interrogative': what he calls 'information requests resembling *wh*-questions' are taken separately, though why this semantic distinction should be retained in a section avowedly dealing with the 'analysis of form' (201) is unclear. Be this as it may, Brown, after listing the basic sentence types, makes other central distinctions - sentences with a main verb, those with the copula (or copula omitted), and noun phrases - and then proceeds to a discussion of various problems encountered in establishing these 'basic' facts. But what strikes me about these basic facts is how controversial they are! Brown is continually having to argue for a particular position, so that far from being presented with a typology that is 'as neutral and generally agreed upon as possible' (202), one is left with the impression that any attempt of such a kind is ill conceived. Here are some examples of the kind of basic problems Brown encounters.

His main difficulty I think arises out of his model of adult English, which he wishes to use on the grounds that, firstly, child utterances are 'moving towards the adult form of the language' and that sooner or later a description of child language must take account of this, and secondly that 'most child utterances are identifiable as imperfect versions of one or another type of adult English sentence or constituent' (203). While this is a reasonable position to advocate, it depends crucially on how 'sooner' one claims this can be done without under- or over-estimating the child's knowledge of his language, and also which adult sentence types you posit the child's sentences to be related to. Brown takes the view that the basic sentence type involved is AGENT-ACTION-DATIVE-OBJECT-LOCATIVE

(e.g. *Mother gave John lunch in the kitchen*). This pattern never occurs in its full form in his data, however – he therefore refers to it as an ‘implicit paradigm’ (204). But the consequences of this position are considerable. He finds himself immediately having to explain why so many possible fragmentary combinations did not occur in his data, and has to argue that it is desirable to eliminate the dative category from the discussion on grounds of infrequency. He also finds himself having to say that ANY one-, two-, three- or four-element construction is a ‘fragment’ of this larger hypothesized sentence type. He does not seem worried by this, but it worries me. Take the following:

There is, sometimes, reason to believe that a single-word utterance is functioning as a fragment of the agent-action paradigm, as, in short, an agent or an object or whatever. Assigning interpretations to single words is, we know, an even more uncertain undertaking than assigning them to short sequences, since there is no word order to offer guidance. However, reference context or a parental gloss or something about the sequence of the child’s utterances does occasionally suggest an interpretation for the single-word utterance. For instance, Adam once said *Ball* and then *Hit Ball*, and that suggests that the first utterance was an object. (205–6)

To me, this suggests no such thing. This is indeed ‘undeniably weak evidence’ (206). Then a little later the argument continues in the same direction, with reference to isolated attributes and possessives, e.g. *Mommy* (= ‘Mommy’s’).

Since attributives and possessives are sometimes explicitly constituents of larger constructions and since there is sometimes contextual evidence that they ought to be analysed as constituent fragments, one must consider the possibility that all possessives and attributes standing alone should be regarded as constituent fragments even though there is usually no clear contextual evidence (209).

Having gone this far, one might as well go the whole way, and Brown does. He asks us to consider the more general possibility: ‘that all utterances which as surface forms are not complete sentences, just as possessives and attributes are not, should be regarded as constituent fragments of complete sentences from which other constituents are missing’ (209). And thus, ‘perhaps all utterances are derived from implicit complete sentences’ (209).

All this, we must remember, is in a section which claims to be ‘as neutral and generally agreed upon as possible’ (202). But it is far from neutral. It is only a short step from here to a view that all utterances are derived from a single implicit complete sentence by some generative process, so it is not surprising that Brown finds the grammars of Chomsky *et al.* to be ‘rich’ and ‘promising’. But why should this be so? On what grounds is the maximal sentence type chosen in the first place? Brown seems to be saying that it is a major sentence

type in adult speech. It is not, in terms of frequency. It may well be more ECONOMICAL to conflate the various possible combinations of actor-action, action-object, etc. into a single type, but Brown nowhere presents an argument from economy, and in any case this argument needs to be made for adult as well as child language. But at least with adults we can argue plausibly that *John* in answer to the question *Who decided to come to the party?* is relatable to an underlying sentence type *John decided to come to the party* (or maybe it should be *John decided to come*, or *John decided*). The crucial question is whether this kind of reasoning (and thus this kind of model) is applicable to children of this age. Brown evidently thinks so. But where is the evidence? Brown says ‘The judgment that a given constituent is missing is partly guided by knowledge of what is obligatory in adult grammar and partly by interpretations of child utterances based on the reference context. There is one other kind of evidence: the “replacement sequence”’ (207), Braine’s term for a sequence in which the longer utterances contain all the morphemes of the shorter. But the argument from adult grammar does not help answer the question about child competence; we all know (as Brown takes pains to say himself) that referential interpretations are slippery; and replacement sequences have their own problems – for example, in our data when a child saw an ice-cream van and said *me*, immediately followed by *get me some*, how should one set about interrelating the inter-sentential elements here? In fact, evidence from replacement sequences is totally dependent on contextual interpretation anyway, for the utterances constituting the sequence have to occur in a short period ‘during which there is no detectable change in the eliciting situation (i.e. nothing happens in the environment to indicate that the utterances are not equivalent in meaning)’ (207).

It is a pity, in all this, that Brown has restricted the discussion to grammars that fall within the generative scheme of things. If, for example, he had looked at the grammatical tradition recently synthesized by Quirk, Greenbaum, Leech & Svartvik (1972), he would have found answers for many of his descriptive questions, e.g. their section on elliptical processes is fuller than any previous (cf. Brown’s complaints on pp. 239–40). But Brown seems reluctant to look at language in any other way than as a single system of generative process, or even to question the premises as to why it is necessary to think in generative terms at all. He is ready to argue about specific derivations (e.g. 108 ff.), but not, it seems, about the very notion of derivation. Yet surely there is increasing evidence in the literature to suggest that a homogeneous explanation in these terms is unattainable. Faced with the range of data about word order (156–7) which ‘offers something to disconfirm almost any hypothesis’, Brown asks ‘With what conceivable hypothesis could the results described be consistent?’ (157). The beginnings of an answer, it seems to me, can lie only in an approach which does not take a single universal strategy as axiomatic. Brown seems to accept that there may be many roads to adult language that the child may follow (e.g. 110, 158), but he

makes little of the implications of this position for the choice of linguistic theory to formalize these findings. Likewise, I miss a critical discussion of another premise of the generative approach, that there is 'a grammar' implicit in each stage of the child's development, which it is the business of the linguist to establish. Can the sampling procedures and the notion of stages be reconciled with a view that language development is essentially a continuous process? Is the small sample of data collected for each child an adequate corpus on which to base grammatical rules? How predictive can rules be, when the language system is changing so rapidly? And how are these rules to be tested? Native-speaker intuition is not readily accessible at the early stages, despite the ingenuity of the De Villiers, the Gleitmans, and others, to get at it. And when is it legitimate to begin talking in terms of rules at all, instead of collocations of lexical items?

Apart from these general questions, there are many places in this book where a linguist would want to criticize, largely on grounds of matters of descriptive detail. Here are a selection.

p.19. 'Imperatives have an "understood" subject *you* (singular or plural) and no other subject.' But cf. *Someone shut that door!, John come here; the other children stay where you are!*

p.24. Intonation, as well as pause, is required in order to distinguish restrictive and non-restrictive relative clauses in English.

p.29. The early use of *and* does not seem to correspond to the logical notion of conjunction as closely as Brown suggests. Typical 3-year-old story-tellers seem to use it more with a hesitation function, to begin with, e.g. *Daddy in the garden and - and - he fall over.*

p.83. 3rd person present singular *-s* 'is perfectly redundant or predictable if the subject is expressed' - except, of course, for that class of nouns (*sheep, postman, etc.*) which are invariable.

p.136. Here Brown repeats a common fallacy about 'stative' verbs, that they cannot be used in the imperative or progressive functions. 'One does not order someone to *know* or say *I am knowing . . .*'. But it is not difficult to find counter-examples within the 'class' of statives as a whole, e.g. *Now, see here!, See!, What's Johnny wanting for his birthday?, Go on! Like it!* (said jocularly to someone who, having expressed a dislike of some food, and then being given some, finds it is not as bad as was thought, is reluctant to admit it). This is an important point, as it happens, as Brown builds an acquisition argument on the static/dynamic distinction (140 ff.) which claims that these two classes of verbs are distinguished very early on, the child learning to use the inflections with the one class but not the other. (Brown says that Adam and the others NEVER used the progressive with a stative verb.)

p.154. In his criticism of holophrases as one-word sentences, he takes a sentence to be 'a structural specification of relationships among elements', and argues that holophrases cannot be sentences by definition. This he claims is the

'usual sense' of the term sentence. But it should be pointed out that there is a very strong tradition of talking about 'minor' or 'non-favourite' sentence types in English linguistics (see e.g. Quirk *et al.* 1972, Bowman 1966, as well as earlier references in Bloomfield, Hockett and others) within which the notion of a one-word sentence is quite acceptable.

p.206. 'Nonimperative sentences must have subjects'. But as Brown himself says a little later (240), this is not true for adult colloquial speech, where this elision is common. And this variety is hardly a marginal one.

p.253. 'The past inflection indicates that a process named by a verb began and ended before the time of speaking. The plural inflection indicates that the thing referred to by a noun exists in more than one instance.' Brown corrects the first statement himself on p.332, but the second one is allowed to stand, despite all the famous countability examples (*oats* and *wheat, etc.*). It is curious how Brown at times introduces some quite traditional and outmoded notions of form-function correspondence in grammatical categories into his book, when he is so conversant with the most recent developments in theoretical linguistics. A further example is in his attempt to present the semantic difference between present simple and progressive without paying systematic attention to the co-occurring use of temporal adverbials (319). If he had, the conclusion that 'the implication of temporary duration is not at first a part of the meaning of the present progressive' (319) would surely have to be modified, to take account of examples like *pick pussy minute* (= 'pick up pussy in a minute').

Apart from the positive views expressed about the development of early syntax, the comprehensive scope of Brown's approach allows him to demarcate very clearly the boundary lines of our knowledge, and his discussion in these cases is always clear and informative, though regrettably often brief, e.g. his references to the nature of parental speech and of parental interpretation of child speech (149 ff., 239 ff.); his discussion of the problem of ascertaining the child's awareness of grammatical word order, which suggests to him that 'this aspect of English syntax is not a single competence emerging all at once in stage I but rather a series of performances maturing at different times' (244). Of particular interest are his speculations about the role of redundancy. He makes the very important observation that the variability in a child's word order, etc. is often considerable, and that 'while the child may have relational intentions to communicate he shows little or no concern with the adequacy of their communication . . . It is as if the child expected to be understood - as indeed he will be by adults in the same situation and having memories in common with him' (161). A little later he develops the theme (167-8) of the child's 'apparently capricious deletion of constituents' and word-order variations, and suggests that as his range of situations and interlocutors broadens and becomes more complex, so he is forced to adapt sentence form accordingly, making it gradually independent of its immediate context, so that 'In the end it can be written in isolation on a

piece of paper and understood by all who speak the language' (168; cf. also 205). This is a matter of social as well as cognitive pressure. The point is briefly developed later (241) with reference to discourse rules: children have to learn 'that in order to communicate some things it is necessary to say more and, in addition, that it is obligatory by convention to express certain things even when communication would not be impaired if one did not'. These are cogent points, and they need to be thoroughly discussed. It is a pity, however, that Brown does not go into the implications for linguistic theory, where notions of redundancy, indeterminacy, and the sociolinguistic issues implied do not sit comfortably alongside orthodox conceptions of the form of grammars.

This book, then, rests on too uneasy linguistic foundations for my liking; it is too uncritically eclectic. The kind of synthesis of descriptive information which Brown wants to provide becomes less convincing when one tries to keep in mind all the theoretical simplifications which have had to be made in order to arrive at it. It is not that his results are counter-intuitive; but his methods and reasons for reaching a particular result are often highly debatable. He would be the first to admit it, and throughout this book he often does, with a sense of humour, charity and humility that all of us could learn from. But there comes a point in time when any sequence of hunches, probabilities and assumptions, no matter how well-motivated, develops (in me, at least) a sense of cumulative uncertainty, and by halfway through this book, not even the repeated summaries were able to convince me that the book rested on an implicitly coherent theoretical foundation. Reading it as a collection of insights into most aspects of early grammatical development, one cannot but be satisfied. Reading it also for its empirical findings, there is a great deal to be gained, and I look forward to Volume 2 with keen anticipation. But reading it for its theoretical conclusions, one must be disappointed – and if the final discussion of complexity is anything to go by (404 ff.), I suspect that Brown too is far from happy.

Brown has chosen to write his book in an informal, unhurried style, and has printed it likewise (the lines are left unjustified throughout). The only trouble with it is that dozens of errors have been left by the process, the majority are trivial, some are needlessly obscuring. Misreading due to bad punctuation is particularly common, e.g. no commas after *location* (9, line 11), *English* (11, 38), *first* (31, 27), *circumstances* (39, 20), *1.75* (58, 36), *small* (68, 29), *combinations* (69, 3), *literature* (96, 38), *grammar* (99, 40), *possessives* (196, 5); unexpected commas after *and* (38, 19), *9* (69, 11); no close bracket after *arguments* (121, 10), *nonexistence* (172, 28); no italics on *danced* (26, 33). We have a number of misidentifications, some of the more serious being *through* for *though* (10, 39), *track* for *truck* (158, 29), *use* for *us* (212, 33), *propositional* for *prepositional* (233, 24), *Fig. 14* for *Fig. 41* (301, 41), *present progressive* for *present perfect* (364, 35). A quote from Slobin mid p. 98 is made unintelligible due to a dash being replaced by a period. There seems to be an unnecessary B in Rule 4

on p. 108. A complete line of print is given twice on p. 99; at least one line is omitted on p. 189 (line 38). A reference to Slobin 1971 on p. 87 lacks an *a* or *b* marker; Labov (1971), referred to on p. 266 is not in the Bibliography; and Miyako on p. 6 is Mayako on p. 420. These errors, plus the occasional awkward sentence structure (e.g. 159, lines 27–9), mar what is otherwise an extremely easy book to read, from which point of view I have already found students very ready to thank him.

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S. M. ERVIN-TRIPP, *Language acquisition and communicative choice*. Essays selected and introduced by Anwar S. Dil. (Language Science and National Development Series of the Linguistic Research Group of Pakistan.) Stanford: Stanford University Press; London: Oxford University Press, 1973. Pp. xvi + 384.

Characteristic of a great part of the present linguistic scene is its interdisciplinary orientation. This can be noticed not only in the existence of composite disciplines, labelled by compounds with *socio-*, *psycho-*, *ethno-* etc. as determinants and *linguistics* as determinatum, but also in pure linguistics, as has been shown through its recent contact with logic, information theory and other fields.

The stability or change of orientation in the scientific production of a scholar may reflect itself in a representative selection of his or her essays over a long period of time. The Language Science and Natural Development series, published by Stanford University Press, gives us the possibility of following various scholars in this perspective. The present volume is the seventh in the series. It covers nearly 20 years' work of an author represented through 17 essays, and gives evidence of a widening of orientation, from psychological approaches to the inclusion of sociological ones, and presents very clear indications of the possibilities of combining the two fields of psycholinguistics and sociolinguistics.