

Suffering a relarosp: a reply to Connolly

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It is a pleasure to have the opportunity to read this commentary, and to comment upon it. In an interdisciplinary field, everyone benefits from feedback, especially when it arises out of routine application, as occurs in clinical practice or teaching. My main regret in recent years, in fact, is that so few people who have taken the trouble to read and use our work have bothered to send us their comments, especially when they are published in working papers of limited circulation. It is more than a matter of the normal academic courtesies: there is less likelihood of permanent misrepresentation, and more chance of real progress in our understanding of this complex field. The present venture thus represents a step in the right direction, which I have found most helpful – and which I hope will be of general interest. I shall first provide brief comments on Connolly's (C's) points, following his numbering, and then make a few general remarks.

Comments

2.1.1 Many of C's points arise out of a difference of opinion about what it is useful to put on a profile chart as a first approximation. LARSP is only a means of developing a sense of what is going on grammatically in a sample. If too much information is incorporated, we will not be able to see the clinical wood for the trees (many people feel it is too detailed). If too little information is put in, we will not be able to distinguish one clinical wood from another (many people feel it is not detailed enough). I do not find stereotyped utterances to be sufficiently common to warrant the kind of subclassification C recommends, but undoubtedly a microprofile of the Stereotyped box would recognise the distinctions he sets up, and in a P who uses a great many stereotyped utterances, this kind of subclassification would be made.

The reference in (c) to 'the child' perhaps warrants the comment that the procedure is of course not restricted to children.

The final sentence I do not find a problem. The clinician who uses LARSP will already be familiar with the sample (s)he made, and of course may have had considerable contact with P, such that any idiosyncratic stereotyped patterns will be quickly apparent.

2.1.2 There is no easy way of handling the problem of ellipsis. It is a notion which needs strict control, otherwise almost anything can be called elliptical. The tightest control comes when you stress the linguistic recoverability of the omitted language, and we pay particular attention to T's context, in this respect. Even so, the first edition of LARSP was criticised for allowing too much under the heading of ellipsis.

and we accepted this point, setting up the category of Reduced. Of course, as soon as you introduce a distinction, you have to expect marginal cases, and here indeed are some further problems. But I would claim that there are fewer problem cases now than there were before, thanks to this new category.

However, it will never be possible to set up a simple 2/3-term system which will handle all the types of ellipsis which exist (see Quirk et al 1985: Ch.12). C's solution is certainly an alternative way of addressing the problem, but I would expect to find marginal cases even here, which only a microprofile analysis would be able to focus upon in a proper way. I don't think our approach is too bad, for a first approximation, especially when you take into account the principle that our profile tries to reflect the realities of clinical samples, and not normal adult conversation. If clinical samples threw up lots of cases of the kind C cites, I would be more worried.

I do not understand C's worry about 'spontaneous ellipsis'. If I say 'I've bought 33 books. 33 books!', it would be perverse not to analyse the second as an elliptical kind of sentence, using the first sentence as the context for recoverability.

2.1.3 I think it would be worth experimenting with this. It could be helpful for Ps with a marked phrase bias.

2.1.4 I have no objection to the Killingley suggestion, if this problem is encountered.

2.1.5 'Intensifier' means 'intensifying adverb', indeed, but as only a highly restricted sub-set of adverbials can occur in this slot, I'm not sure that the use of 'Adv' (which suggests *any* adverb) would be clearer. As the point also worries Killingley (1981), though, it will require further consideration.

2.1.6 The LARSP chart was in fact around before GCE came out, and before I knew which term Quirk et al. were going to plump for. 'Predeterminer' would do just as well.

2.1.7 Again, I see these distinctions as the business of a micro-profile. The circle-S convention is a good idea, though less mnemonic than NP, VP, etc. One good point about it is that it would give a bit more space on the transitional lines.

2.1.8 'Underanalysis' raises the microprofiling question again. The hierarchical issue involved would, I hope, have been introduced in a previous Structure of English course, and our simplification thus appreciated for what it is.

There is a principle at stake here, which is worth reiterating (though it goes well beyond C's comment, and C himself, I know, would agree with what follows). It seems to be the case that, in some places, LARSP is being used as a kind of mini Structure of English course. Sometimes, even, it is used as an introduction to Linguistics! Whatever the practical considerations which have led to these developments, the result is wholly undesirable, and leads to all kinds of complications. To teach English grammar through LARSP is, quite simply, back to front. LARSP needs to be seen in the context of a course of study in which appropriate linguistic notions (to do with hierarchy, classification, etc.) are introduced in a general way, and principles of analysis and simplification motivated. Without this, students are bound to learn the procedure arbitrarily, without a real sense of why various decisions were made. We routinely recommend flexibility, in using LARSP, in the sense that categories may be regrouped, subclassified or provided with alternatives, if occasion warrants. But this obviously presupposes that T knows what (s)he is doing – and how can this be, if T has never been given a general course on English linguistic theory and analysis? What I consider grossly unfair, of course, is when, having *not*

given one's students this more general course, LARSP is then criticised for failing to make good the students' lack of general awareness. Killingley (1981), for example, criticises LARSP for (amongst other things) not sufficiently discussing the theoretical distinction between sentence and clause, for failing to introduce the distinction between endocentric and exocentric, and for failing to discuss the concept of 'word' as a linguistic unit. But all of this, as they say, is somebody else's problem.

2.1.9 This point certainly needs to be noted, as it has not been made clear in our publications.

2.1.10 Quirk et al make all kinds of distinctions which we conflate, in the interests of simplicity. Once again, whether it is worth introducing this contrast depends on clinical frequencies. As samples rarely have much in the Coord (NP) box, I doubt whether it would be. I'd let a microprofile take care of it, in a relevant case.

2.1.11 It is the general notion of 'subordinator' which I find most important from a clinical viewpoint, subsuming both subordinating conjunction and relative pronoun. The similarities in P performance between the two categories are striking, I feel. On the other hand, there are evident differences (especially the clause element function of the latter). What is misleading, I agree, is my tendency to illustrate this category ('little *s*') only from conjunctions, and to talk loosely (in Crystal 1982) of 'subordinating conjunction'. A better gloss for 'little *s*' would be 'subordinating word/item'.

2.1.12 This is a general criticism of grammatical models of this kind, and is not specific to LARSP. A course on the Structure of English would clarify. I have no objection to people using subscripts, if they wish.

2.1.13 It seems to me that VS is a clear indication of inversion, as it stands – as opposed to the formulae SVO, etc.

2.1.14 This is a good point. The use of something like (X) (+) seems warranted.

2.1.15 If it is useful to do this, then there is space at the top of the Q column to incorporate the information. But anyone who does this must realise that the kind of information they are including is not the same as that found elsewhere on the chart. The theoretical issue has been argued at length in several places (e.g. 1969, and with reference to the present procedure, 1979:16–17). From a therapeutic point of view, also, the issues involved in taking account of P's use of intonation are quite different from those where P is manipulating formal grammatical contrasts. I prefer to take account of this kind of issue on the PROP chart, where tone unit structure and function is explicitly related to grammatical units.

2.1.16. In my view, the usage can be shown to be consistent, if you paraphrase the symbols as follows:

- 1+ a (sentence) element in addition to 1
- X+ a (clause) element in addition to X
- XY+ a (clause) element in addition to XY
- 3+ a (clause) element in addition to 3.

2.1.17 I would appreciate some reaction on this point, which had not occurred to me.

2.1.18 This is not my experience. I have far more trouble getting people to remember not to count Zero responses in with the P totals in Section B.

2.2.1, 2.2.2, 2.2.3, 2.2.4 Agreed. It should perhaps be noted that the double marking of Pron causes a problem when one tries to automate the analysis.

2.2.5 The point behind this, of course, is that there is no uncertainty over the fact that two elements are involved.

2.2.6 This point should certainly be made clearer. The occurrence of Adverbials after *be* is of course possible, as noted in Crystal (1979:75).

2.2.7 The relevant sentence should read: 'main verb in a clause . . .'.

2.3.1 While there are indeed acquisitional arguments about the status of Stage IV, I have never heard a clinician dispute the usefulness of drawing a line between Stages III and IV, which permits some direction to be imposed on what would otherwise be a long and complex period of development. Also, while LARSP makes no explanatory claims about the existence of these stages, I feel that in due course there may be some useful correlations established between syntactic length and complexity and such notions as auditory memory span, at which time the III/IV distinction may receive some theoretical motivation. In the meantime, it should be noted that the clearest syntactic differences between Stages III and IV are to be found in the Question column, and in the Phrase column.

The principle works clearly enough for Stages I-IV. To apply it to Stages V-VII, one must remember that double marking is involved. When a sentence is assigned to Stage V, its clauses are simultaneously analysed at clause level, phrase level, etc. A sentence such as *The man kicked a ball and it went into the goal* will have only two marks at Stage V (one under Coord. 1, the other under *and*), but several more marks higher up. The 60 percent principle implies recognition of *all* marks related to a particular sentence. Similarly, to assign a sentence to Passive, *it*, etc., or a part of a sentence to Complex VP, etc., involves the same considerations. I must admit, though, that I have never tried to work out the 60 percent principle fully with reference to Stages VI and VII, in view of the relatively sporadic nature of the constructions listed there. When these Stages are more fully described, it will be possible to evaluate its application in these areas.

2.3.2. Of course one always wants to make empirical foundations more secure. But, notwithstanding our own cautious expression, what actual evidence is there that our chronological norms are wrong? They were, after all, based on a synthesis of available acquisition work which has not been questioned. And (with the exception of tag questions) I have not come across relevant findings from other projects which indicate that these norms are inaccurate: the correlation with the findings of Wells' Bristol project is very good (Wells, personal communication), apart from tag questions, which the Wells data indicated were at least a Stage earlier than that recognised in the 1976 edition. Note also the way LARSP comes out 'in the middle' of the comparative studies carried out by Miller, Klee, Paul and Chapman (1981).

We have always expressed the hope that some kind of standardisation procedure would be devised. An early application of mine to DHSS for funds was not successful, but a recent award has been made by MRC to Fletcher to develop a standardised assessment procedure based on LARSP. We therefore hope to be able to provide a positive answer to this criticism in due course. In the meantime, our view is that we do not expect regional or sociolinguistic variation to pose much of a problem: the total amount of regionally-distinctive syntactic/morphological variability is not as great as all that.

2.3.3 Anyone can do this if they want. We often do use percentages when we interpret our charts, in addition to our use of raw scores. Do we have to make a recommendation on this point? On the other hand, percentages should never replace raw scores: to say that 30 percent of P's sentences were at Stage I means something very different if the raw scores are 3 out of 10 or 30 out of 100.

2.3.4 Our intention was to be helpful to those who rely on simple linear measures, so that they could obtain an initial impression of how a LARSP profile related to their work. As a basis of comparison, what else can you do? It should also be noted that a linear relationship between MLU and age has been demonstrated (Miller & Chapman 1981).

2.3.5 The trivial answer to this comment is that it is not a gap – derivational forms can be logged under Stage VII Other (as a relatively late development)! However, I would not seriously want to allow derivational morphology into the LARSP approach: it is thus not a ‘gap’, but a principled omission. Derivational information is more satisfactorily handled in relation to a lexical procedure.

2.3.6 It would appear that we are moving in the direction C wants us to go, in our new error box, which contains far more detail than the 1976 edition. Adj error would of course find its place in the Other category, where there is space to write it out, if need be. It is an empirical question whether this category of error turns up sufficiently often in clinical samples to deserve separate mention. I know of no statistics, but I have not found it so common. The more radical proposal, of a vertical error dimension, is interesting, though I think most of the categories on it would remain largely unused. Would anyone like to try to work it out, to see?

2.3.7 We have the same hopes, and are working in this direction ourselves. But the main outlay of time, on transcription, will be with us for a long time. Scanning by ear is of course done routinely by anyone experienced in the use of the procedure, and we always recommend this, when the case is not too complex, and the need for careful case records is not paramount. For every one P who is formally LARSPed, there must be a dozen who are ‘notionally’ LARSPed. LARSP is not just a profile chart; the chart is only a means to an end, never an end in itself. Our primary intention has always been to motivate people to think systematically about abnormal grammatical development, and to develop a critical sense of the arbitrariness of linguistic categories, so that they are not fooled by terminology. While one may dispute the nature and relevance of individual categories, I find it difficult to conceive of arguments which contradict its underlying philosophy. We have made this point often enough, but, it seems from C’s bibliography, there are still people about who think that this philosophy is something the clinician can do ‘without’.

2.3.8 This is something on which I am currently working, in relation to an MRC project on linguistic diagnosis.

2.3.9 Nothing is simple, in this business. It is important to recognise, however, that many of the points at issue in the above depend on differing judgements as to what should count as a ‘first approximation’ and what should go in a more detailed profile analysis of an individual category. But C’s reference to the difficulties must be interpreted: once the empirical facts are known about an area (such as pronoun development), constructing the micro-profile is quite straightforward. It’s the lack of detailed acquisitional information which is the problem, not the clinical procedure as such.

General comments

The view, attributed to Bickerton, that a grammatical assessment ought to pay attention to the interaction with other levels, is a truism, and I know of no-one who would dispute it. In making a LARSP analysis, I have always recommended that it be

considered in relation to other aspects of P's language, and indeed all aspects of P's behaviour and case history. The reason why we adopted a 'narrow' approach to grammar is explained in several places (such as the opening pages of Crystal, Fletcher & Garman 1976): it seemed the clearest and most practical place to start, in the context of the available grammatical and acquisitional data which was available in the early 1970s. As soon as LARSP was published, I began work on the other profiles – first PROP, then PROPH (with Fletcher), and finally the two PRISMs. The strengths and weaknesses of these procedures are now being explored. If, in due course, the strengths seems to outweigh the weaknesses, there will be a point in attempting some kind of integration, and supplementing the information by socio-linguistic profiles, of the kind hinted at in Crystal 1981: Ch.6. The required empirical work on social interaction is slowly appearing: see, for example, McTear (1984) and Letts (1984). However, I remain sceptical of the practicability of devising suitably integrated syntactico-semantic procedures, given the present state of theoretical semantics. The best one can do is be aware of the most relevant points of connection (such as the discussion in Crystal 1981: 119, ff.), and supplement any one procedure in an ad hoc way (as is done for syntax, using a verb valency model, in Fletcher (1984)).

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