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DISCUSSION

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Is Modularity Matching correct or useful?

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In his review of Crain & Thornton's (C&T) (1998) *Investigations in Universal Grammar* (IUG), Drozd raises many substantial concerns that call into question the correctness and usefulness of the Modularity Matching Model, and the adequacy of the experimental designs put forth to support it. My comments will focus on Modularity Matching. First, I elaborate on Drozd's suggestion that C&T's comparisons between Modularity Matching and other (potential) models do not provide convincing support for their model. Second, I suggest that Modularity Matching, while a strong claim, considers such a narrow range of phenomena in child language that its usefulness in explaining language development in general is likewise constrained.

As Drozd points out, the Modularity Matching Model elaborated in IUG is an exceptionally clear statement of the strong nativist position on language development in children: that children are identical to adults in their knowledge of linguistic principles, in the parameters available to them, and in their language processing mechanisms. The stated goal of IUG is to lay out exactly how experiments should be designed to test children's linguistic competence given the assumptions of Modularity Matching, and this is achieved admirably. However, an implicit goal of the book is to explain and defend Modularity Matching as compared to other models of acquisition, and here, like Drozd, I found myself unsatisfied.

To really defend Modularity Matching, the most appropriate comparison in my opinion would be with emergentist or information processing views of acquisition such as that outlined in Elman, Bates, Johnson, Karmiloff-Smith, Parisi & Plunkett (1996). Emergentist models of acquisition are diametrically opposed to Modularity Matching, particularly in their claims about the extent and role of children's processing abilities, and it would be very informative to see an open assessment of one in the terms of the other. However, C&T choose to keep their comparisons 'within the family' of generative work, and therefore compare Modularity Matching in detail only to the so-called COMPETING FACTORS MODEL. In my view, C&T's critiques of the experiments discussed under the Competing Factors Model are indeed

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justified. They highlight the need to provide appropriate presuppositional content to make test sentences felicitous, the need to separately analyse results from different sentence types (ambiguous vs. unambiguous) and subject types (children who do vs. do not have a given constraint), the need to control for the difference between preference of interpretation and knowledge of principle, and the need to establish appropriate performance criteria (statistical significance is too weak). Although these are all very valuable methodological insights, it is not clear that they are relevant to an assessment of the Competing Factors Model. Most of the poor design features of the experiments reviewed do not appear to have arisen as a result of the model, as might be inferred from the logic of C&T's argumentation. There is no evidence that researchers specifically designed act-out tasks which failed to meet presupposition requirements, for example, because they predicted that this would best reveal children's use of memory or extralinguistic knowledge. Rather, it appears that a belief in (at least some tenets of) the Competing Factors Model led the researchers to be satisfied with the results they found, and thus they weren't driven to see some of the methodological and analytical flaws in their experiments. The strong claims of Modularity Matching which expect adultlike performance from children do provide this motivation, but so would more attention to solid experimental methodology in the light of the insights achieved in this area over the last decade.

Further, although C&T's critiques rightly show that the experiments reviewed have flaws that need to be addressed, they neither disprove the adequacy of the Competing Factors Model nor support the adequacy of the Modularity Matching Model. In each case, C&T suggest clear methodological improvements for the experiments reviewed. In a small subset of those cases, new experiments have been run with the improvements in place, but the results obtained still do not reach the 90% criterion set by C&T. For example, Crain's (1982) rerun of an act-out task yielded only 82% correct responses, even after a large number of 'other' responses were removed from the analysis. Unless the experiments in question are all conducted again with the methodological improvements in place, and unless they all show children performing virtually like adults, the validity of Modularity Matching is still very much open to question.

Another of Drozd's main claims is that Modularity Matching is limited in its usefulness for explaining children's linguistic performance. In fact, C&T are quite clear about the intended limits of Modularity Matching: to explain children's knowledge of linguistic constraints specified in UG that are prohibitions against certain sentence forms and certain sentence-meaning pairs (pp. 12–13). Children are expected to have and follow the same prohibitions as adults, except in certain circumstances: when sentences are ambiguous, when parameters have not yet been set to the appropriate

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language-specific values, when expected presupposition criteria for the sentence being tested have not been met, when parsing test sentences puts an excessive burden on cognitive resources, or when children's knowledge is tested with tasks that have inappropriate or confusing demands (*cf.* p. 121). Some of these exceptions indeed seem well-justified under any theory, such as that children's linguistic knowledge cannot accurately be inferred from tasks with confusing demands. But other exceptions name factors that may well provide a significant part of the explanation of children's early language performance. It is clearly an essential part of the scientific method to set aside potentially confounding factors in order to reach the core set of phenomena that are integral to a clear explanation of the question at hand. But one wonders in this case whether we can yet be so sure that some of the factors set aside are really as peripheral as assumed or implied by C&T.

In fact, even within the domain of structures under consideration by Modularity Matching, it is clear that some factors set aside by C&T have explanatory power. Let us take null subjects as an example. Perhaps the most prominent current nativist explanation for null subjects is the 'two-process' approach – one for null subjects in non-finite clauses, and another for null subjects in finite clauses (Bromberg & Wexler, 1995). Null subjects in non-finite clauses are taken to be a natural extension of two facts: that child language allows infinitives in main clauses (Wexler, 1998), and that adult languages (even those that do not otherwise allow null subject) allow null subjects with infinitives (e.g. Mary asked—to leave). Null subjects in finite clauses, in contrast, come about through an over-applied process of topic drop, whereby a subject that is established as a topic in the discourse context can be omitted (e.g. Hyams & Wexler, 1993). Although topic drop is a grammatical option in adult obligatory-argument languages, it is restricted to very specialized contexts. Children have yet to fully grasp the contexts in which topic drop is licensed, with the result that they omit arguments too frequently. However, the nativist approach has no explanation for why children over-omit topics, which features of topichood children rely on in determining which subjects to omit, or why not all subjects of non-finite sentences are omitted. Here, the discourse-pragmatic approach to language development provides important insights. Research following this approach characterizes well what a 'topic' is in discourse, and shows that children are sensitive to factors like newness vs. givenness in discourse and joint attention in context, that children's subjects are overt significantly more often when the referent of the subject is not accessible from the discourse or situational context, and that subjects are more likely to be overt the less accessible their referents are (e.g. Clancy, 1997; Allen, 2000, in press; Skarabela & Allen, 2002). Applying the discourse-pragmatics approach thus deepens our understanding of the null subject phenomenon in ways that the nativist approach cannot. In the best of

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all worlds, it would be ideal to consider a variety of factors to make an acquisition model that is as far-reaching as possible, rather than focusing a model so narrowly that it is safe at the expense of ignoring interesting questions for which answers can and should be sought.

Crain & Thornton's book is indeed a valuable contribution to the field of language development in stating clearly the strong nativist position on language acquisition, and in elaborating in detail the experimental methodology required to support this position. Even far outside the strong nativist domain, C&T's experimental designs have significantly affected the care with which people formulate their own experiments. However, this book has not proven to me that Modularity Matching is the right or complete account for the area of language it sets out to explain, or that C&T have yet determined all and only those factors which are relevant to a full understanding of children's knowledge of syntax.

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