

COMMENTARY

A Linguistic Approach to Developmental Dyslexia

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Bryden, McManus, and Bulman-Fleming (BMB henceforth) challenge the bold hypothesis put forth by Geschwind, Behan, and Galaburda (GBG) in several ways. They reexamine the empirical validity of the hypothesis by conducting a thorough survey of the literature and question the coherence of certain concepts that are used rather widely. In this note, we take BMB's critical analysis one step further and examine the criteria commonly used to establish a diagnosis of dyslexia. As we show, the literature contains a wide variety of views and diagnostic methods, all of which putatively characterize one and the same dysfunction. Yet we argue that heterogeneity of definitions (resulting from very different views of the nature of dyslexia) necessarily leads to heterogeneity of the subjects that are picked out and, as a consequence, to an unclear picture when it comes to experimental results. This kind of criticism, we believe, is missing from BMB's paper as a result of a mistaken view of the nature of human linguistic abilities, a view that unfortunately prevails in clinical circles. We thus sketch an outline for an alternative and consider its potential merits.

The study of brain/behavior relations involves an investigation of the

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physical properties of neural tissue, coupled with a functional analysis of behavior. It is the latter, we believe, that is replete with conceptual pitfalls in the present case. To analyze behavior, one needs a theoretical framework, without which the analysis becomes quite murky. In many complex behavioral domains, coherent functional analyses are unavailable, and thus, relating these behaviors to the brain becomes a rather treacherous business.

In this context, consider Anomalous Dominance (AD) and the critique BMB offer to GBG's view. BMB point out, correctly in our view, that "If AD is to have any theoretical utility as a concept then it must be strictly defined. . . ." Looking at the available definition of AD, they argue that its vagueness ". . . leaves open to question just what should be considered 'anomalous'. . . ." As a result, different experiments use different selection criteria for AD: some use handedness (or degrees thereof), others use handedness and language lateralization, and yet others use the former two dimensions, as well as visuo-spatial lateralization criteria. This of course may have huge effects on the experimental results. Any decision one makes regarding selection criteria may skew the data in some direction.

These objections are valid not only to AD. BMB's critique, in fact, points to a problem much wider in scope. It may be advisable, that is, to look at the various definitions and diagnostic criteria used in developmental dyslexia, especially in those studies that seek to support GBG's theory, which BMB review in their critique.

We conducted a survey of characterizations, diagnostic methods, and other definitions of developmental dyslexia that are included in all the articles that BMB mention which investigate the interaction of dyslexia with other factors (immune disorders, neurological disease, etc.). We found that the definitions are so varied as to make the groups in different studies not only way too inclusive but also to a large extent incomparable. BMB seem to be somewhat aware of this problem and note that "The existence of significant heterogeneity among studies for some conditions suggests that better definitional criteria should be used in future research. . . ." We think, however, that the problem is much more severe: It is not just a question of better criteria in future studies. Rather, it casts serious doubts over the existing literature. These conclusions will thus make many of the GBG claims doubtful, but for reasons that BMB do not consider, namely due to dubious diagnostic measures that are used in many of the studies.

Below, we bring several representative examples. As our main illustration, we start with a study by Galaburda, Sherman, Rosen, Aboitiz, and Geschwind (1985). The reason for this choice is twofold: First, these authors provide crucial evidence for the theory GBG wish to promote; second, the diagnosis of dyslexia established in this study was later used

in at least one other important neuroanatomical study (Livingstone, Rosen, Drislane, & Galaburda, 1991).

Galaburda et al. conducted a postmortem study of four deceased dyslexics, for whom some data on reading skills happened to be available. They focus on neuroanatomical issues and argue for a neural correlate of dyslexia. While their neuroanatomical investigation seems convincing, they provide no clear characterization of the functional disability, and the diagnostic tests are varied. In one case, the Gray Oral Reading Test was administered; in another, the same test, together with WRAT; a third case was tested on the WRAT alone; and the fourth had no "formal" testing, and the only indications we have for his dyslexia are "results of an informal spelling test. Tutor said: '. . . performance was like that of children in the third grade.'" (p. 223). While Galaburda et al. readily acknowledge that "there is a controversy as to whether developmental dyslexia is a single entity" (p. 222), this neither leads them to question the diagnosis of their subjects nor makes them weaken their conclusions regarding the neural substrate for dyslexia. We think, however, that these tests may have tapped a broad spectrum of cognitive abilities, and we cannot be sure that any of these tests probed directly those reading skills that are affected in developmental dyslexia. Reading problems, after all, may be a consequence of many types of problems—lack of experience, poor instruction, memory problems of various sorts, visual impairment, developmental dysphasia, and so on. Dyslexia, however, is a narrower functional entity. So, while we are aware of the logistical difficulties of conducting postmortem studies which presuppose behavioral tests, we believe that in such cases one should be extremely cautious in ascribing a definite diagnosis to a deceased patient. We are not convinced, that even one of these patients was dyslexic, even though we find the neuroanatomical findings very intriguing.

Having illustrated our point, we now review the diagnostic methods one encounters in the literature. BMB review studies that include both learning disabilities and dyslexia. Our review contains only those studies among the ones BMB review, which refer to dyslexia explicitly. We show that even within this group of studies, which in itself is much more restricted than BMB's empirical domain, the diagnostic methods are varied and poorly motivated. A partial typology of these will suffice to underscore our claim.

Impressionistic criteria, no "formal" test. Schachter, Ransil, and Geschwind (1987), for instance, used questionnaires in which dyslexia was established only through self-report. Satz and Fletcher (1987) used an even more surprising diagnostic: they asked teachers to indicate whether or not a given student was reading disabled. The results of such studies, we believe, may not lead to serious conclusions of whatever kind (see also Pennington, Smith, Kimberling, Green, & Haith, 1987).

Standard reading tests. Such tests are used, in many cases, in conjunction with other criteria (family history of reading disability, special education, neurological handicap, etc.). Many reports are very vague as to the nature of the reading tests used: they may be, for example, "standard criteria used in the Norwegian education system" (Hughdahl, Synnevag, & Satz, 1990), or "fit the operational criteria for dyslexia on standardized testing" (Pennington et al., 1987), "had history of reading at least two years below grade level while at school" (Hier, LeMay, Rosenberger, & Perlo, 1978; Behan, Behan, & Geschwind, 1985) or "professional diagnosis" (Hansen, Nerup, & Holbeck 1986). As we noted earlier, the fact that a student has a difficulty in reading (or that his/her teacher perceives this to be the case) does not mean that this individual is dyslexic. Thus, generic reading tests and questionnaire are not enough for the establishment of a sound diagnosis of dyslexia. We thus believe that the vagueness, variability, and arbitrariness of these methods speak for themselves, leading once again to serious doubts regarding the validity of their empirical findings, as well as generalizations that GBG, as well as BMB, seek to make over the whole class of studies.

Discrepancy between IQ and reading achievement. This criterion has been used by several groups (e.g., Hynd, Sermud-Clikeman, Lorys, Novey, & Elipulos, 1990; Bishop, 1984). It first measures IQ, as well as reading ability on some standardized test, and then establishes some (arbitrarily set) cutoff criterion for the relative difference between the scores on both tests. Although better than the previous diagnostic method, it is still insufficient: first, the reading tests that are used cannot establish a specific diagnosis of dyslexia (rather than a mere reading problem of some unspecified type). Second, there is no reason to believe that a discrepancy between test scores is a necessary or sufficient condition for dyslexia.

Let us summarize our conclusions so far: For a claim regarding the relationship between dyslexia and the brain to go through, one must distinguish this disorder from other reading disabilities. This is not done, usually. Worse yet, even within the studies on dyslexia, we find unsound diagnostic methods, ranging from impressionistic ones to unmotivated disjunctions and conjunctions of arbitrarily constructed diagnostic procedures. A thorough revision of test procedures is thus in order. We think that there is a very clear direction one should follow, namely, the diagnosis of developmental dyslexia should be based to a large extent on linguistic parameters. This suggestion is consistent both with our view of language as a rule-governed functional entity, as well as with neuropsychological findings to the same effect. Crain, Shankweiler, and their colleagues (e.g., 1986, 1987) have tested dyslexic children on a variety of linguistic tests and showed selective impairment along grammatical dimensions which they attribute to the phonological component of working memory. Similarly, Vellutino (1987) acknowledges the contribution

of linguistic factors to dyslexia. Finally, Kean (1984) has also argued that developmental dyslexia is a linguistic problem.

These claims are based on a limited range of tests. They are interesting and important, we think, because they investigate the most important dimension of the mechanisms that underlie linguistic (and reading) behavior. Limited as they are, these results are highly suggestive, and we can only speculate that once a wider range of linguistic tests is devised and carried out, the true nature of developmental dyslexia will emerge. This is so because linguistic tests will correctly distinguish dyslexia from other problems and will also tell us whether developmental dyslexia is a single entity, or comprises a class of disorders. An approach that commends linguistic testing and error analysis may discover, we believe, that there are phonological, attentional, and visuo-spatial dyslexias (as there are different aphasias and acquired dyslexias), and that such an internal typology will distinguish dyslexia from generic reading problems on the one hand, and correlate its various forms with neurological and immune factors on the other (Bishop, 1990, makes a somewhat similar proposal). Once this path is taken, the dispute between BMB and GBG may be over. The weak correlations BMB expose in GBG's database may be a mere consequence of the weakness of the diagnostic methods. If our proposal is adopted, the way these correlations will be done in the future will be more refined, leading hopefully to clearer results, since they will be based on coherent functional analyses of dyslexia.

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