

The acquired dyslexias and normal reading

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Humphreys & Evett's (H & E's) theoretical discussion of normal function is very reasonable, and the coverage of the relevant literature, particularly the neuropsychological, in their review is thorough. I am less happy, however, about the inferences they draw from the acquired dyslexia studies they describe. They argue that "converging evidence" is needed "for the claim that acquired dyslexic reading consists of a reduced set of normal processes; without this the data must be treated conservatively." It is indeed standardly accepted in the field that the inference from impaired behavior in individual patients to the organization of normal function is not a straightforward matter, and converging evidence is obviously important (e.g. Caramazza 1984; Shallice 1979). However, inferential problems also exist in drawing theoretical conclusions from the results of experimental paradigms using normal subjects, and their assessment neglects the potential that strong neuropsychological dissociations provide for counterintuitive conclusions about normal function.

The rather pessimistic position of H & E on the theoretical implications of neuropsychological findings seems to be a consequence of the inferential methodology they adopt. In recent work on the acquired dyslexias three types of approach have been used. H & E's target article is primarily based on the symptom-complex approach (e.g. Coltheart 1980a) in which a group of symptoms that occur together across a number of patients is treated as the empirical starting point for inferences to function. In its weak form, in which the clustering is probabilistic, the methodology is now widely believed to be theoretically untenable (e.g. Caramazza 1984). Clusters can arise for contingent anatomical reasons (see, e.g., Poeck 1983), and also, as H & E point out, the same observed symptom can be produced through impairments at different stages of the processing route. It is therefore not very surprising that, as H & E demonstrate, the use of this approach in the acquired dyslex-

ias, with syndromes like deep dyslexia and surface dyslexia being treated as functional entities, is now looking very sickly.

Alternative approaches do exist. Each patient can be treated as an entirely separate test of theory (e.g. Morton & Patterson 1980). The attempt can also be made to select predominantly "pure" single-component impairments for study by using a dissociation-fractionation procedure (e.g. Beauvois & Dérouesné 1979a; Shallice & Warrington 1980). The weeding-out process can be taken further, for instance, by trying to eliminate phenomena based on compensatory strategies (see Henderson 1982). When this approach was applied by Shallice and McCarthy (1985) to the cluster of patients commonly labelled surface dyslexic, the majority were eliminated as probably using a laborious sounding-out process for reading somewhat analogous to the classic letter-by-letter reading pattern. Only two were held to produce relevant evidence for functional inferences: the patients HTR (Shallice, Warrington & McCarthy 1983) and MP (Bub, Cancelliere & Kertesz 1985). HTR had no greater difficulty with nonsense syllables than matched regular words and MP was virtually perfect. They made few incorrect GPC (grapheme to phoneme correspondence) assignments, especially MP, and there is little sign of the visual errors that H & E also consider to be a property of "surface dyslexia."

H & E do discuss these patients, but the characteristics they consider are open to interpretations other than those they favor. Thus, they discuss the rare errors of the *yacht*-*"yat"* type made by HTR which include both an irregular lexical component (the *ch*) and a regularization one (the *a*). Their explanation is that the difficulty of the patient "may not be in accessing lexical phonology per se, but in accessing lexical phonology for whole letter strings." I find this explanation obscure and do not understand why it is preferred to the alternative explanation that the errors reflect weakened and slowed activation by morphemic correspondences in a multiple-level "phonological route" (see Shallice & McCarthy 1985). Also, when considering the effects of degrees of irregularity H & E make a comparison between clearly noncomparable conditions from different experiments and neglect within-experiment comparisons. More critical (and odder, given their overall position) H & E ignore the convergence in pattern of results with effects found in normal subjects (e.g., Parkin 1982; Parkin & Underwood 1983).

H & E's treatment of MP reflects another problem: the need to view the characteristic of theoretical interest within the setting of a patient's overall problems. H & E argue that if nonlexical processing were intact for MP, lexical decisions could be made on the basis of nonlexically derived phonology. As MP scored at only a 75% level on lexical decisions the authors infer that she is impaired at a nonlexical level. Yet it is known that patients who have difficulty in accessing semantics can have difficulty with lexical decisions even for stimuli that are satisfactorily processed phonologically (e.g. Warrington 1975), and this appeared to be the case with MP, judging from the clinical description. It seems unwarranted to use this as evidence for impaired nonlexical processes. Another example of this type of problem occurs in the discussion of phonological alexia in which patient WB is treated as a particularly pure case. Yet on the criteria developed in the defining analysis of the syndrome which used a dissociation-fractionation methodology (Beauvois & Dérouesné 1979a), the patient would be excluded as the effects of her expressive aphasia probably cannot be differentiated from the results of any impairment to processes specific to reading.

Such points may seem extremely detailed. Unfortunately, on the methodology advocated here, assessing the relevance of the acquired dyslexias to understanding the operation of spelling-to-sound translation processes requires detailed discussions of the findings in individual patients. It is probably not possible to do this in a review as condensed as that of Humphreys & Evett. Interested readers should consult Patterson, Marshall and Coltheart (1985) for a variety of conflicting views.