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The nature of hemispheric specialization: Why should there be a single principle?

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As I understand it, the question asked by the authors is how best to characterize the functional differences between the hemispheres. They do not explicitly discuss the rules by which an answer should be sought. The approach implicitly followed first by clinical neurology, then by experimental neuropathology (including the split brain studies) and the neuropsychology of lateral differences in normal subjects has consisted of cataloguing the operations and performances on which the hemispheres differ. Bradshaw & Nettleton (B & N) are scrutinizing the catalogue, often with considerable ingenuity, in search of "a more fundamental, antecedent mode of specialization" that could replace the inadequate verbal-nonverbal dichotomy in accounting for the totality of the evidence. I suppose most readers will share my impression that no convincing unitary pattern emerges.

The analytic-holistic distinction has impressed many people by its explanatory power, especially in dealing with the role of operating modes, a topic of much current interest (see Morais & Peretz 1980, for an individual-differences approach). Yet it must be admitted that, as used so far in neuropsychology, the concept of holistic processing is extremely vague and probably lumps together different notions such as preattentive segmentation of the field, configurational description, template comparison, etc. I suspect (although I have had no time to check the idea on a large sample of the literature) that many of its explanatory successes are *a posteriori*. One example is Patterson & Bradshaw's (1975) finding that matching schematic faces to a memorized target produces left hemifield superiority when nontargets differ from the target on all features, and right hemifield superiority when the difference affects only one feature. The authors, although acknowledging that other interpretations are possible, suggest that this finding is consistent with the notion that the right hemisphere proceeds by "holistic Gestalt matching." An alternative analysis of the task, that the three-differences condition where all features are equally valid for the decision, allows concentration on one single local detail, would have predicted a larger right-side superiority in that condition. My colleague José Morais, who is more optimistic than I am about the prospects of the analytic-holistic distinction, nevertheless suggests in his recent review (Morais 1980) that there is an urgent need to integrate neuropsychological findings with corresponding developments in general cognitive psychology, like the work of Garner (1980).

B & N ultimately seem to favor the notion that left hemisphere superiority in afferent and efferent temporal discrimination might be the gist of both its analytical processing and its involvement in language functions. The correlation which is established between analytical and time-dependent coding would still need considerable elaboration to be fully convincing. On the other hand, the idea that language production and comprehension are basically sequential processes has come under serious criticism (Poeck & Huber 1977).

So, it does not appear that we are ready to substitute a simple characterization of hemispheric specialization for the catalogue of recorded differences.

The question one ought to ask is why it is that we should expect hemispheric differences to be reducible to one single principle. The function of those differences is a problem that can only be meaningfully analysed in evolutionary terms, by trying to identify the advantages that resulted from lateralization of cerebral functions and favored evolution away from symmetrical organization. Declared candidates for the role are control of articulatory movements, division of labour between the hands, and isolation of analytical thought from interference from more primitive "holistic" forms, but these do not, of course, exhaust the possibilities. It does not seem that we shall be able to decide in the near future, but whatever the eventual answer, the important point concerning the present discussion is that any new capacity which appears under a particular environmental pressure can then produce other effects which in turn can guide further evolution. The fact that organs can change functions, and also serve many at the same time, is a biological platitude. Consider the sting apparatus of hymenoptera: Most ants use their sting and associated venom components for defense purposes; in ants of the genus *Atta*, however, such as the Texas leaf-cutting ant *Atta texana*, the same structures seem to function only for depositing trail pheronomes (Hermann, Moser & Hunt 1970).

Nearer to us, what is the function of the hand: walking, feeding, climbing, carrying babies, using tools, making tools, signing, loveplay, writing, or something else?

The complexity of the present picture of hemispheric differences is thus not necessarily due to limitations of our conceptual or experimental approaches, but may reflect the true multifactorial nature of the underlying processes.

A minor point concerns Bever's (1971) claim that REA (right ear effect) is only found for words in sentences, not for random word strings: This observation was in fact not based on conventional dichotic recognition tests, but on the click location task, where the perceived temporal position of a click superimposed on a stream of speech depends on whether the click is delivered to the left ear and the speech to the right one or *vice versa*. Bever was making the assumption that this effect was due to hemispheric specialization, but we found that it is actually inverted if a language written right-to-left, such as Hebrew, is used in the test instead of a left-to-right one like English or French (Bertelson 1972). It is thus doubtful that the effect has much to do with brain asymmetry. The particular finding that the effect is not observed with random strings, which was only mentioned in one sentence, has, to the best of my knowledge, not been published in full.