

Correspondence

Dear Sir, **No selectivity in literature review:
reply to Hemsley and Clark**

In their critique of my position paper on cognitive behavior modification (Ledwidge, 1978), Hemsley and Clark (1980) question my conclusions because of the “selectivity” I am alleged to have shown in the data I considered relevant to the topic. I would appreciate the opportunity to defend myself against their charges.

My paper included a critical review of *every* article on cognitive behavior modification published in any of the four behavior therapy journals between 1963, when the first issue of *Behavior Research and Therapy* appeared, and July 1976, when I began writing the article, as well as all controlled CBM—behavior therapy comparisons published in journals other than the four behavior therapy journals or cited in behavior therapy texts. There were honest oversights in this massive undertaking (e.g., Miller *et al.*, 1975) but none of the studies cited by Hemsley and Clark were eligible for inclusion in my review. The reports of Holroyd (1976) and Moleski and Tosi (1976) were not available until after I had completed my review of the literature in June of 1976; those of Kanter (1975) and Thompson (1974) are unpublished doctoral dissertations and I did not include *Dissertation Abstracts* in my review of the literature (one must set limits in an undertaking of this size). The follow-up findings of Di Loreto (1971) were not included because I confined myself to outcome at the time of treatment termination, since many studies do not include follow-up data. The Maes and Heimann (1972) study, contrary to Hemsley and Clark’s allegation, was included in my review (pp. 366, 372).

I am also faulted for assuming that “learning theory . . . is free from cognitive concepts such as expectancies” (p. 55) and my mistake is confirmed by a quote from Mackintosh (1974). The succinct and incisive rebuttal by Rachman and Eysenck (1966) to Breger and McGaugh’s (1965) claim that the concept of expectancy is central to learning and behavior therapy, cannot, I think, be improved upon:

“They are, of course, free to make any preference choice they like, . . . however, one would require some direct evidence indicating that Expectancy × Value theories give rise to different and more efficient methods of treatment than Drive × Habit theories before entering into any formal argument” (p. 165).

I agree with the authors that research on the differential changes in cognitive and behavioral measures as a function of mode of intervention would enhance our understanding of the relationship between behavior change and cognitive change. So far, research in this area, most of it conducted by Bandura and his colleagues, attests to the superiority of behavioral procedures for changing cognitive processes (Bandura, 1977).

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