

- (2) BEWLEY, T. H., BEN-ARIE, O. & JAMES, I. P. (1968) Morbidity and mortality from heroin dependence. 1: Survey of heroin addicts known to Home Office. *British Medical Journal*, *i*, 725–6.
- (3) ——— (1968) Morbidity and mortality from heroin dependence. 2: Study of 100 consecutive inpatients. *British Medical Journal*, *i*, 727–30.
- (4) ALARCON, DE R. & RATHOD, N. H. (1968) Prevalence and early detection of heroin abuse. *British Medical Journal*, *ii*, 549–53.
- (5) GARDNER, R. & CONNELL, P. H. (1970) One year's experience in a drug-dependence clinic. *The Lancet*, *ii*, 455–9.

### 'MEANING AND VOID'

DEAR SIR,

A review as muddled factually and conceptually as Dr Berrios' review of my book, *Meaning and Void: Inner Experience and the Incentives in People's Lives* (*Journal*, September 1978, 133, 270–1) compels a reply. Since space restrictions do not permit a reasoned point-by-point rebuttal to the review's lattice of misrepresentations, this letter can only indicate the nature of the principal discrepancies.

Dr Berrios misrepresents me as equating 'meaning' with 'incentive' and of setting incentives up as 'a kind of *primum mobile*'. In fact, on p. 24 I wrote "The idea that incentives control behaviour . . . manages to hide as much as it reveals", and I go on to point out its circularity. Most of the book from that point on is devoted to nailing down what this 'pedestrian truth' (Berrios) may mean in terms of specific functional relationships among psychological processes and conditions—the conditions that govern attraction to objects and that determine the rise and fall of value, the role of affect in this process, the effects of frustration, and the clinical implications. The incentive-related *systems* involved are certainly regarded as pivotal features of human life, but this is very different from representing incentives as prime causes.

The review wonders about the relevance of '138 American students talking about the importance of meaning in their lives'. In fact, that isn't what they talked about, and that paragraph further misrepresents the function, number, and diversity of the samples involved in that four-page section of the book.

Contrary to Berrios, the book never refers to lack of meaning as a cause of depression or as a cause of anything else, other than to reflect a motivational basis for attempts to alter one's state of consciousness.

The review misrepresents several chapters as unoriginal rehashes of stale material. The reviewer

noted the 'expected' references but ignored the rest, as well as the original integrations. For example, are expectancy-value formulations of suicide really as customary as all that? How many books have systematically formulated principles of value change, or have traced the role of affect and habituation in value, drawing on the experimental as well as clinical literature? Above all, this book develops original current-concerns and incentive-disengagement approaches to motivation.

The review misrepresents the book as espousing a 'view of depression based on learning', a view that much of Chapter 5 is specifically devoted to rejecting.

There is much more to be said. Berrios's review simply does not fairly represent the book. I urge you to consult it yourself.

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### ELECTROSLEEP

DEAR SIR,

I was interested to read your recently published study of methadone withdrawal with electrosleep by Professors Gomez and Mikhail (Gomez and Mikhail, 1979), and to learn that they had found electrosleep successful under controlled conditions, but was disappointed by the brevity of their discussion which made no mention of possible mechanisms and only mentioned four previous studies. I am not sure whether, by this, they were implying that electrosleep is so well accepted that discussion is unnecessary, or so peculiar that discussion is impossible . . . Neither of these situations apply, and I suspect that many of your other readers would also welcome the authors' fuller discussion of the results of their otherwise admirable paper.

At my own review some years ago (Hall, 1973) over a hundred previous articles on the subject were brought to my attention, and there had even then been two international symposia held at Graz in 1966 and 1969, a controlled trial carried out by American workers (Rosenthal and Wolfson, 1970) and the subject had been reviewed in several of the foreign science bulletins put out by the United States Library of Congress (Ivanovsky, 1967, 1968 and 1969) since electrosleep had been introduced by Livenstev in 1949. Despite one's inevitable scepticism about a treatment which is pleasant, quick, economical and without side effects, and which several eminent neurophysiologists have quite properly explained to me is scientifically far from respectable, an admittedly