Goodmayes we did not find a single example of the progressively deteriorating course which is the traditional stereotype of chronic schizophrenia. All the patients had either been maximally disabled at the time of first admission to hospital, or their deterioration had ceased to progress at least ten years previously; the end-state described by Bleuler (1972) in *Die schizophrenen Geistesstorungen im Lichte langjahriger Kranken- und Familiegeschichte*.

This evidence that chronic schizophrenia tends to stabilize is supported by a number of long-term studies, including Bleuler's own personal follow-up of over 200 patients and Daum, Brooke and Albee's 20 year follow-up of 253 patients, and accords well with clinical experience.

This is not, of course, to suggest there will be no chronic schizophrenics in the community, but taken in conjunction with evidence that the most severe and crippling forms of the illness are less common than in the past (Hogarty, 1977, *Schizophrenia Bulletin*, **3**, 587–99) it predicts a more hopeful future than the tenacious myth of inevitable, progressive deterioration.

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Reference

Daum, C. M., Brooks, G. W. & Albee, G. W. (1977) Twenty year follow-up of 253 schizophrenic patients originally selected for chronic disability. *The Psychiatric Journal of the University of Ottawa*, **2**, 129–32.

NO LUNG CANCER IN SCHIZOPHRENICS?

Dear Sir,

I was prompted by the letter from Dr D. Rice (*Journal*, January 1979, **134**, 128) and by the recent death of one of my chronic schizophrenic patients to look at post-mortem records at Rainhill Hospital—made available to me by Dr A. S. Woodcock, F.R.C.Path. In the past five years post-mortem examination has confirmed the presence of lung cancer in eight patients. Three with no previous psychiatric history had an acute psychotic episode of the type familiar in this condition; two had long-standing recurrent depressive illnesses; three were typical chronic schizophrenic patients of at least twenty years duration before the terminal illness. Two of them had been continuously in hospital (since 1953 in one case and 1956 in the other), while the third had been maintained at home, thanks partly to a supportive family. Histologically the tumours were: oat cell, poorly differentiated squamous, and a well differentiated papillary adenocarcinoma.

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BRITISH POLICY ON OPIOID MISUSE

Dear Sir,

Professor G. Edwards (January, 1979, **134**, 1–13) refers to a paper of mine (1) by the wrong title, date and page, and misquotes some figures from it. He has made the mistake of combining results from my study with those of a previous one by Bewley *et al* (2), though he lacks the necessary data. The passage in his article should have read: 'of 112 opioid users whose deaths were reported in the United Kingdom, 24 were not known to the Home Office before they died'. These deaths deserve more attention than Edwards has given them because they represent some of the price paid for the present British policy.

The prescribing of NHS heroin or methadone—whether this is done by general practitioners or by specially licensed doctors—does not protect against the high morbidity, mortality and infectious nature of opioid misuse (1, 4). There is, therefore, an alternative option to the ones Edwards has proposed. This is to stop the prescribing of opioids for self-administration altogether, and for medical personnel to administer them to patients considered suitable for maintenance treatment. The advantages of this approach are that it would diminish the above risks, officially acknowledge that the medical risks are too great to justify using medical means (prescribing opioids) for social ends ('keeping the Mafia out') and enable different maintenance schedules to be tested. Certain problems would remain such as when to start maintenance treatment (5) for a 'new case' or for one who has relapsed, and when to stop because, say, a patient is misusing illicit drugs. The disadvantages would include the logistics of implementing this scheme and the possibility of stimulating a criminally organised black market.

Although it may have been justifiable in 1967 to be so fearful of what might happen, Edwards shows that there is less cause for alarm today and that the present policy should be reviewed.

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References

(1) Gardner, R. (1970) Deaths in United Kingdom opioid users 1965–69. *The Lancet*, **2**, 650–3.

https://doi.org/10.1192/bjp.134.6.649b Published online by Cambridge University Press

(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 in-patients. *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 Livenstsev 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