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# Correspondence

## SEEBOHM AND CHADWICK

DEAR SIR,

If Dr. Ferguson (*Journal*, July, 1970, p. 126; February, 1971, p. 251) has read any of the histories of public health in Britain (e.g. Fraser Brockington's, or Chapter 12 of *The Bleak Age* by J. L. and Barbara Hammond), he will know of Chadwick's medical precursors, Frank in Germany, Percival and Ferriar in Manchester, as well as of Chadwick's medical colleagues, Kay and Southwood Smith. Public health did not spring fully armed from Chadwick's 'social insights'. He will know also that *The Times*, from 1841 onwards, was, as the Hammonds put it, 'a powerful and steadfast friend in the cause of public health'; its hostility towards Chadwick was a personal one, widely shared, as well as being directed against the particular administrative set-up of which he was the centre. To quote the Hammonds again, 'A Control Board on the provocative model of the Poor Law Commission was a lamentable blunder . . . it is difficult to understand how Ministers came to choose the most hated man in England as a member.' This hatred derived from Chadwick's harsh and rigorous administration of the 1834 Poor Law, founded on the fallacious principle of 'less eligibility', which, as Fraser Brockington says, 'ignored all that we now know to lie at the root of poverty'.

I wonder whether Dr. Ferguson would defend this particular 'social insight' of Chadwick's—especially just now when the cry is 'Back to Speenhamland'? Would he consider those who fought against the Act and its application—including the youthful author of *Oliver Twist*—to have been merely 'resistant to change'?

Dr. Ferguson implies that Chadwick's fall in 1854, and the dissolution of his Board, involved a repudiation of the whole public health principle; but he must know that the work was continued under the Privy Council, with John Simon as its Medical Officer, and that it was the advances made during this period that paved the way for the Public Health Act of 1875.

Chadwick's eventual 'rehabilitation' had, of course, nothing to do with the discoveries of Pasteur and Koch—which, incidentally, he never accepted. As with other veterans—Lord Brougham is a good contemporary example—his earlier asperities and obstinacies faded into insignificance, and he was revered as the great pioneer of the past and the wise counsellor of the present.

Chadwick was right in many things and wrong in others, but it is hard to see what relevance all this has to the administrative questions raised by the Seebohm Report and the Act implementing it.

For the rest, I cannot help deploring Ferguson's stale 'resistance to change' ploy (directed at Dr. Pilkington of all people!), to which one can reply that new and emergent professions are naturally prone to ambitious empire-building. Similarly, charges of 'medical chauvinism' might be countered by ones of 'medical defeatism'. Would it not be better to keep to a sober discussion of the merits of the case, as indeed Dr. Ferguson has done elsewhere?

My reactions to Dr. Ferguson's letters have, of course, been as he would have predicted from my 'age and status range'. My only excuse for writing, apart from a dislike of false history, is that I have had the unique experience of being (simultaneously) President both of the R.M.P.A. and of the Association of Psychiatric Social Workers. There did not, at that time, seem to be all that much divergence between our respective 'insights'.

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## DR. SCHMIDEBERG AND PSYCHO-ANALYSIS

DEAR SIR,

The late Ella Sharpe, who was my training analyst (1941–44), once said to me 'If you are looking for ideal parents or an ideal band of brothers and sisters then don't join the British Psycho-Analytical Society'.

I have often recalled this saying with relish, and have passed it on to colleagues, and to students whom I have trained.

The operative word, of course, is 'ideal'. Psychoanalysts don't have to be ideal (or infallible), any more than do children, parents, teachers, editors, politicians or what have you. It would be appalling if they did have to be so, and still more if they *were*!

Much of what Dr. Schmideberg (*Journal*, January, 1971, pp. 61–8) describes concerns our pioneers, both here and in other countries. Paradoxically,

we suffer from and are indebted to their courage, enthusiasm and initiative, among other things; perhaps, even, their living and dying. They were subject to ordinary human emotions, and, like all pioneers, they made many mistakes, some of them with serious and even tragic consequences.

We don't have to forgive them, and we cannot repay them. But it behoves all of us who work in the field of mental health, in psychiatry or psychotherapy, in any form whatever, to seek to recognize, acknowledge and wherever possible put right or modify the mistakes *we* are making now—in other words *to know and take responsibility for whatever we are doing, both good and ill.* This is one of the basic tenets of psycho-analysis itself. It is sad that Dr. Schmideberg has apparently now jettisoned it altogether.

It would be an impertinence to offer this comment to Dr. Schmideberg herself, but there may be some among your readers who might care to consider it.

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## THE CLASSIFICATION OF DEPRESSIVE ILLNESS

DEAR SIR,

I hope you will permit me to make a brief comment on the almost perennial topic of the classification of depression, and in particular on the distinction between the categorical and dimensional systems which has been so cogently argued by Professor Eysenck (*Journal*, September, 1970, pp. 241–51). I wish to make a point which is often overlooked by those who seek to defend or refute the existence of this or that 'disease entity'. There is, in fact, no uniformly satisfactory medical diagnostic system based upon categorical entities, such as Professor Eysenck implies, though fragments of many such systems survive because of their practical value. Disease entities are convenient abstractions, not independent and mutually exclusive states. While aetiology, pathology, treatment and prognosis tend to intercorrelate in such entities, there are few conditions where these different aspects of diagnosis have a correlation coefficient of unity. Professor Hamilton (*Journal*, September, 1970, p. 348) illustrates this by reference to paratyphoid, and thereby provides an additional argument in support of his comment that 'the categorical and dimensional models are therefore not as different as Professor Eysenck suggests'.

Whether a categorical or a dimensional system is used will depend partly on the purpose of the classification, as well as on the presence or absence of meaningful discontinuities in the data, yet the criteria for determining the presence of a diagnostic entity are rarely made explicit. While making some concession to the dimensional approach by reference to the 'relative preponderance' of different symptoms, Gurney *et al.* (*Journal*, September, 1970, pp. 251–5), in their paper on the treatment of affective disorders, demonstrate the advantages of superimposing on their data a categorical model. It would be difficult to express their conclusions so concisely without using diagnostic entities. However, as Hughlings Jackson wrote in 1874, 'All classifications in all sciences make distinctions more exact and abrupt than any that exist in nature'.

The type of diagnostic classification used will thus depend in part upon its function. Teachers, nurses, drug firms and the Registrar General will continue to use a categorical disease entity classification because of its practical simplicity, whereas those concerned with research and with individual patient management may favour a dimensional approach because of its greater sophistication and flexibility. Both models are but a pale reflection of 'the majesty of all governing nature'.

MAX HARPER.

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REFERENCE

JACKSON, J. HUGHLINGS (1874). 'On epilepsy and epileptiform convulsions.' In *Selected Writings of John Hughlings Jackson*, Ed. by J. Taylor, Vol. 1 p. 202. London: Staples Press.

## COGNITIVE TESTS IN THE DIAGNOSIS OF DEMENTIA

DEAR SIR,

I would like to draw your attention to an omission in the paper, 'The Validity of Some Cognitive Tests in the Diagnosis of Dementia' (*Journal*, August, 1970, pp. 149–56).

The omission is on page 155 in describing the Orientation Test. Item 12 was not included in the manuscript. This item reads, 'How long have you been in hospital?' (score one point if patient knows he is in his first, second or third week).

I apologize for this omission.

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