

Pauling (1970), with his regime of vitamin C to combat the common cold, based on his assessment of the evidence as a scientist?

Have clinical trials produced firm conclusions about the effectiveness of any treatment in psychiatry? If so, which are the methodologically sound studies? If not, considering the number of studies that have been conducted, would it not suggest there is an inherent difficulty in the design of the 'double-blind' method itself?

As an example, consider the evidence for the use of tricyclic antidepressants in depression in general practice. Hollyman *et al* (1988) found amitriptyline to be effective. By contrast, Porter (1970) found no difference between imipramine and placebo. Interestingly, Porter did not pretend his trial was double-blind, because he recognised that no trial of this kind can be conducted under completely blind conditions. In fact, he openly declared his bias that tricyclic antidepressants probably had no specific action in depression illness, although they may suppress anxiety and agitation by their sedative effect. He argued that his attitude towards the effectiveness of the drug might neutralise the influence of the breaking of the blind. The bias of Hollyman *et al* (1988) is less clear. Has their use of double-blind methods eliminated potential expectancy effects? It is a legitimate question. I am not suggesting it is easy to answer, but some evaluation may be possible with evidence from participants' guesses about medication status. An insistence on statistical purity in the analysis might produce a lack of awareness of the fallacy of the method.

The problem is that the results of 'double-blind' studies tend to be automatically accepted as scientifically valid. A misleading self-deception is encouraged that trials can be conducted double-blind, and the role of expectancies is underestimated. I understand the wish for a scientific basis for psychiatric treatment, but professional status should not mean that the challenge to double-blind methodology goes unnoticed (Oxtoby *et al*, *Journal*, 1989, 155, 700–701).

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Demand for psychogeriatric services

SIR: We were interested to read the paper by Christie & Wood (*Journal*, August 1990, 157, 228–231), and share the authors' concern over the failure to match resources to increasing demand, but wish to make two points. Firstly, from our own experience we doubt the findings can be easily generalised to other areas as the authors suggest and secondly, we are concerned that the problem of functional illness in old age may be overlooked because dementia so preoccupies the debate about the ageing population and service provision.

We have had 336 admission episodes (over the age of 65) during the past two years of which 44% were dementias (34% excluding planned respite care), 33% affective disorders, 12% schizophrenias (early and late onset), 1% acute confusion and 10% other conditions, mostly adjustment reactions or medical problems. Christie & Wood report an astonishing 76% of their admissions to be dementia and only 16% functional illness, whereas over 45% of our episodes were functional, over 53% if dementia respites are excluded.

Accepting that the Crichton Royal study only included patients over 69 years of age, these differences are considerable. Christie & Wood cite similarities between Blessed & Wilson's Newcastle study (*Journal*, 1982, 141, 59–67) and the early Crichton Royal study (Christie, *Journal*, 1982, 140, 154–159) – both performed in the mid-1970s – as evidence that their findings are of more than parochial interest. However, the Newcastle admission rates more closely resemble our own than the Crichton Royal data at that time with 41% functional, 43% dementia (Newcastle) and 29% functional, 50% dementia (Crichton).

These differences may reflect widely-differing clinical practices, varying illness prevalence, different community provision, social and family support or other factors that distinguish deprived city catchment areas like Liverpool and Newcastle from rural areas like South-West Scotland. Without far more information about these variables it is difficult to interpret the Crichton Royal findings or see how they may translate to other areas. The requirement for long stay dementia beds, for instance, depends on several variables (Blessed, 1988), including the provision of residential care, and the private sector contribution to this varies eight-fold nationwide (Joint Colleges' Report, 1989).

Contrary to the experience at the Crichton Royal, our major difficulty is providing in-patient beds for the functionally ill, which again may be a local problem. Functional patients will always require in-patient services while one could argue that the long-term care of dementia need not, or should not, be in a hospital ward. We no longer have long-stay dementia beds and it seems highly likely that all areas will find their long-term care beds being dismantled. Our concern is that while we pursue a largely futile case for more long-term dementia beds, the problem of acute functional illness may be forgotten.

Functional illness remains more prevalent than dementia, active psychogeriatric services generate increased demand for in-patient treatment for functional illness (Joint Colleges' Report, 1989) and the 'graduate' population in the community continues to increase. This will inevitably drift into the psychogeriatric domain and may prove a considerable drain on resources. It will be a mistake to underestimate the future demands of functional illness in old age.

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How old are the elderly?

SIR: I read with interest the paper by Cook *et al* on depression and previous alcoholism in the elderly (*Journal*, January 1991, 158, 72–75). The majority of psychogeriatricians in the UK deal with clientele above the age of 65, and in some cases the age limit goes to 75. Interestingly, the authors consider subjects above the age of 55 as elderly and the mean ages for subjects with alcoholism and no alcoholism were 57.7 years and 62.5 years respectively. Probably this reflects the differing views of what age is considered as being elderly?

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The 'new cross-cultural psychiatry'

SIR: Professor Leff has the advantage of having his letter (*Journal*, December 1990, 157, 932–933) pub-

lished in the month following mine. My own letter (*Journal*, November 1990, 157, 775–776) languished somewhat after its submission in March and the original argument may no longer be so fresh to readers of the *Journal*. In his editorial preceding my review of the 'new cross-cultural psychiatry' (Leff, *Journal*, March 1990, 156, 305–307; Littlewood, *Journal*, March 1990, 156, 308–327) Leff made a number of errors of fact and interpretation, to two of which he replies.

My point on the 'existence' and gender of smallpox deities was essentially factual. Professor Leff answers more generally on evaluative questions, inevitable for a pragmatically-orientated psychiatry as opposed to a more distanced if nuanced anthropological position. I am far from certain that "an anthropologist is neutral as to whether or not people die of smallpox"; I for one am not. As I described in my paper, clinically-applied anthropology, including understanding of local beliefs about sickness, is now a part of the provision of clinical services (Chrisman & Maretzki, 1982; Kleinman, *Journal*, August 1990, 157, 295–296), a development which both of us appear to value.

The fundamental difference between the two disciplines seems now to be one of the degree of 'objectivity' claimed. Neither, of course, are context-independent reflections of some transcendental reality existing independently of our procedures of observation, but it is interesting that social anthropology, once regarded by biomedical science as somehow dealing in 'soft' data, seems here to have acquired a harsher objectivity (cf. Clifford & Marcus, 1986) in a way psychiatry has not, conflating as it does fact and value whilst mistaking the latter for the former. There is an irony here in that disciplines which allow for observer bias suddenly seem to switch from extreme subjectivism to super objectivism.

I would, of course, hardly quarrel with Leff's restatement of the value of our examining local meanings before carrying out comparative studies. Indeed this may be taken as the central 'motif' of the 'new cross-cultural psychiatry' (Kleinman, 1977). Nor would one be surprised that this procedure might not prove to be feasible, either for economic, organisational or ideological reasons. But our failure to carry out a study of local contexts must be accompanied by an appreciation of the limits of the data we can collect without it. Inevitably, a purely epidemiological study employing diagnostic criteria derived from one society alone will lead to our 'conventional error'. Attempting to remove culture from the whole study initially to control for it as an independent variable later leads to a fictitious construction of the whole field, in which culture is simply