

and therefore they do not counter the arguments of previous research. Finally, using patients as their own self-controls is not really the best method of controlling. With such methodological differences, the only paper with which theirs is comparable is that of Kennedy *et al* (1983). The only remaining argument to support their claims of superior methodology is that a good instrument (the LEDS) was used. Unfortunately, good tools are never a substitute for correct selection, critical judgment, and a fair design which risks erring on the side of caution rather than drive to a pre-selected outcome.

However, the publication in its current form raises an even more important issue. The paper was presented at a College meeting, and from the floor I had a chance to point out to the presenter how he was consistently misrepresenting my work and what I considered the faults in sampling to be. It obviously made no difference. It is a sad state of affairs when publishability is allowed to over-ride what one might call etiquette if nothing else, and it becomes even sadder when our academic teachers undersign the deed.

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SIR: Our study was essentially a methodological one. Only one previous paper (Chung *et al* 1986) had used the LEDS to collect events and had carefully dated the onset of illness. These authors also failed to find an increased rate of severe events before the onset of mania.

Notwithstanding the above information, Ambelas' study (*Journal*, February 1987, **150**, 235–240) remains a case-note study. He relied on patients' hospital records to elicit life events and to date the onset of mania. We have previously pointed out the drawbacks in this method (*Journal*, June 1987, **150**, 875).

From past studies we had expected to find a relationship between life events and the onset of mania. However, our results indicated that illness episodes

in a group of patients with established bipolar disorder did not appear to be preceded by events. Further research using the LEDS in first manic episodes is indicated.

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Racism, diagnosis and treatment

SIR: In their not unuseful analysis of black and white patients admitted to hospital under Section 136 (*Journal*, March 1990, **156**, 373–378), Dunn & Fahy make the surprising statement that it “challenges the findings of Littlewood & Lipsedge (1981), who suggest that treatment is independent of diagnosis in this [black] group”. If they are talking about the paper I think they are (for they have got the title wrong in their references), I should point out that not only did Lipsedge and I not make the statement they claim but the study itself is not concerned with psychiatric treatment at all. More importantly, they are somewhat cavalier about dismissing the possibility of racism in their own data. Selective discrimination on the grounds of perceived ethnic status, whether in medicine or in other institutions, or in everyday encounters, is not something best seen as located in individual attitudes, but in the whole way social action in a society may operate. In the review which they cite (Littlewood, 1986), I was at pains to point out that racial bias in psychiatry is hardly likely to be a conscious and overt penalisation of certain patients. The alternative, however, is not to associate differences in diagnosis and treatment simply with differences located in the patients themselves. We need, painstakingly, to reconstruct how certain assumptions, behaviours and actions of individuals, both the putative patients and the professionals, interact in certain contexts. Such studies require surveys of theoretical assumptions, attitudes to the other, and perceived dangerousness: studies using video material of actual instances, besides an examination of the political economy of psychiatry itself.

To take an instance. One possibility is that doctors, whether through their training or through