Aggressiveness, anxiety and drugs

SIR: Kirov (*Journal*, December 1989, **155**, 846) equates the control of anxiety and aggression: drugs which control anxiety control aggression, and those which aggravate anxiety aggravate aggression.

Clinically, this is often not true, particularly in the field of child protection from abusive carers. Alcohol or tranquillisers can make the parent/carer more dangerous. The mechanism for this is that the parent already hates, resents, or is frustrated by the dependent child. Theoretically, the aggression itself could be dulled by the anti-anxiety drug (alcohol, benzodiazepine or barbiturate). However, cortical restraint is also damped down, releasing or aggravating whatever aggressive propensities remain.

This throws a caution on Professor Kirov's last paragraph, in which optimism is expressed that anti-anxiety drugs will control aggression.

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Limitations of double-blind trials

SIR: Newcombe (*Journal*, February 1990, **156**, 282), in his letter on double-blind trials, continues to defend randomised controlled trials in psychiatric research (Newcombe, 1988), without apparently recognising their limitations (Kramer & Shapiro, 1984). In particular, he does not accept the fallibility of the double-blind and criticises Oxtoby *et al*'s (1989) suggestion that the ability of participants to guess their drug status should be used as a retrospective criterion to exclude certain results. Dr Newcombe's caution about this latter suggestion is justified, although the recording of guesses of whether patients were taking active drugs or placebo can be of value. As a recent example, Marks *et al* (1988) found that assessors' guesses after the end of treatment were mostly right. They did not assess the blindness of patients but suggested that such checks might yield similarly sobering data. Moreover, Oxtoby *et al* (1989) advocate reworking results when the double-blindness has been disproven, and the
of its efforts is a lot more complicated than Leff supposes. For example, in the Kasongo vaccination project in Zaire (Kasongo Project Team, 1981), while a high coverage of measles immunisations was achieved and led to a noticeable reduction in measles mortality, the overall mortality was not affected. The same number of children perished, but from other causes. Was the medical intervention successful? By what criteria do we judge?

know whether the issues that Dr Newcombe raises about upsetting the randomisation and similar problems would be relevant to this study.

Perhaps a more appropriate conclusion to draw from this debate is that clinical trials are unlikely to be definitive in the scientific sense that Dr Newcombe would like. Interpretation of results is inevitably important, which may explain why there is so much controversy about the effectiveness of psychiatric treatment.

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Reviewing reviewers

SIR: I am writing to defend Macdiarmid's review of Ellenberger's *The Discovery of the Unconscious* (*Journal*, January 1990, **156**, 135–139) against what I consider to be a naïve and rather arrogant criticism by Brooks (*Journal*, May 1990, **156**, 747).

I would consider that skimming, followed by selective 'dipping', is an important part of the reviewers art. This may indeed proceed to more thorough reading and re-reading, depending on the nature and merits of the text being considered. Macdiarmid himself clearly appreciated this long book as a reference text and for selective re-reading. In this he was realistic as well as admirably honest.

I take issue with the suggestion that reviewers must
paper which, contrary to Leff's interpretation, neither idealises nor romanticises insanity, but rather demonstrates how intellectual innovation can arise from a particular society's reponse to the inversion of normal behaviour by two messianic leaders who experienced episodes of psychosis.

Dr Littlewood's review includes some suggestions on how clinically applied anthropology can, for example, enhance the role of the liaison psychiatrist attitudes inhibit the creative enterprise. Psychotherapists who work with creative people know this.

Additionally, I suspect that Dr Brooks may himself have committed the crime of which he accuses Macdiarmid. He suggests in his letter that Macdiarmid fails to take note of the proportion of the book given over to Janet, and its emphasis on the importance of the work that came before Freud and influenced his thinking. For me this came over clearly in the review.

I liked Macdiarmid as a writer. He showed respect for an important and scholarly book. In particular I liked his elegant and witty 'attack-from-within' on some of the more pretentious aspects of the psychotherapeutic establishment. On reading Macdiarmid I return with renewed appreciation to Ellenberger. Surely this was an excellent review!

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ROC analysis

SIR: I hesitate to take issue with Snaith & Owens' (*Journal* May 1990, **156**, 744–745) recommendations about presenting the results of a relative (or receiver) operating characteristic (ROC) analysis. ROC analysis examines the ability of a screening instrument to discriminate cases and non-cases across the whole spectrum of morbidity by plotting sensitivity against false positive rate for all possible cut-off points. However, there are good reasons for displaying a smoothed ROC curve rather than "a series of straight lines joining the points" as they suggest.

Unless the sample size is very large in relation to the number of scoring categories, the selection of a cut-off point from the *actual* as opposed to *estimated* data can be very misleading. Random bunching of response scores can result in apparently excellent results (in terms of sensitivity, specificity and overall misclassification rate) with a chosen cut-off point, whereas a slight alteration in the cut-off produces a much poorer result.

The best way to avoid giving a false impression is to show the smoothed ROC curve with 95% confidence intervals on either side. A convenient computer program (ROCFIT) is available which calculates the maximum-likelihood fitted ROC curve and other parameters (Metz *et al*, 1984).

The optimal cut-off point (that is, the best trade-off between sensitivity and specificity) is at the point on the ROC curve which is the greatest perpendicular distance from the diagonal. Of course, *any* cut-off point may be chosen for a particular purpose: the smoothed ROC curve will give a maximum-likelihood estimate of the resulting sensitivity and specificity.

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The Lomax affair

SIR: I read Harding's reappraisal of Lomax's contribution with great interest (*Journal*, February 1990, **156**, 180–187). However, lest the impression be gained that asylums were places of brutality and inhumanity in general, it should be pointed that Lomax described conditions as he found them between 1917 and 1919 – a period when conditions were highly unusual. Not only had many younger medical staff been called up, leaving men past retirement age to manage alone, but many attendants too had gone to war, leaving the asylums grossly under-staffed. Nor was that all; some asylums had been taken over for war casualties and thus other asylums (presumably including Bracebridge and Prestwich) became grossly overcrowded.

There were also severe shortages of food – a matter beyond the control of the asylum managers. In the Burntwood asylum, for instance, the meat allowance had been reduced from 1 kg per head weekly in 1916 to 0·64 kg in 1918; heating also was almost certainly inadequate as the cost of coal escalated. During 1914 there had been 108 deaths within the asylum, but in 1918 there were 256, and these figures cannot be explained by an outbreak of influenza which accounted for only a few deaths. The strong implication is that malnutrition occurred. Nor was the Burntwood asylum unique: in the Worcestershire asylum there were 134 deaths during the final quarter of 1917, whereas during the whole of 1916 there had been 148 deaths.

Thus, if Lomax found "... poor nutrition ... and a high death rate ...", then this finding is in harmony with what was happening elsewhere and is a reflection of the harsh conditions which prevailed at that time. Of course, there can be no excuse for brutality and perhaps Harding's comment that "Prestwich

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