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Correspondence

Admission Rates and Lithium Therapy

SIR: Dickson & Kendell (1986) have reported their observations in Edinburgh. From 1970 to 1981, while long-term lithium therapy was increasingly employed for affective illness, the admission rates for affective disorders rose substantially. The authors find the explanation of these changes uncertain, yet they conclude that the findings cast doubt on the efficacy of lithium prophylaxis under ordinary conditions.

The question of lithium efficacy in clinical practice is very important. The authors' observations are interesting, and their discussion scholarly, but it remains unclear how their failure to explain the rise in admission rates could lead them to question the efficacy of lithium treatment in practice. As the discussion part in their paper leaves several important issues open to further questioning, connecting directly the two increases they observed appears unfounded and rather arbitrary.

The possibility of a shift in diagnostic practice may serve as one example of an unanswered question. Their data shows that while the admission rates for mania and depression rose steadily, the admission rates for schizophrenia fell. As the total number of admissions remained approximately the same, a shift in diagnostic practice would be one of the possible explanations. In many other hospitals the diagnosis of manic-depressive illness increased substantially in the 1960s and 1970s, probably because of changed therapeutic expectations (Baldessarini, 1970; Symonds & Williams, 1981). Thus, this issue is obviously critical for any conclusions about the efficacy of lithium. The authors therefore compared a sample of 20 new admissions and 20 readmissions, from both the first 3 years and the last 3 years, as to the proportion of patients fulfilling the RDS criteria for the main diagnostic categories, and they felt that these had not changed. However, this does not fully dispose of the concern; and a shift in diagnostic practice remains a strong possibility. For one thing, one would require a considerably larger patient sample to detect a statistically significant shift in diagnostic proportions. Secondly, the number of diagnosed manics has increased approximately three times, a change similar in type to the diagnostic shift described elsewhere (Baldessarini, 1970). Finally, although the data present a ten-fold increase in lithium use in Edinburgh, the authors did not find any such change in the sub-samples selected, which certainly casts doubt on how representative the sub-samples are and how relevant they are for interpretation.

In attempting to explain their findings, Dickson & Kendell had to face difficulties common to most retrospective studies. As they address an important question, one can easily identify with their predicament, as well as share their view that the explanation of the findings is uncertain. There are always major risks involved in attempting to make inferences about therapeutic efficacy from epidemiological data (Shepherd, 1961). To wit, in Hamilton, Ontario, we have observed admission changes similar to those reported by Dickson & Kendell, yet the data suggest a different interpretation. From 1970 to 1981 – a period of time identical with that of the Edinburgh study – in Hamilton Psychiatric Hospital there was a similar increase in admission rate of patients with the diagnosis of major affective disorders: from 111 to 190. This increase was paralleled by a rise in admission of these patients in all hospitals in Ontario, from 1036 to 2105 during the same time period. As in Edinburgh, the admissions of schizophrenics to Hamilton Psychiatric Hospital dropped from 520 in 1970 to 435 in 1981. The use of lithium in the area also increased dramatically, reaching in the early 1980s a rate of 2 per 1000 population; over 700 patients were on lithium in the Hamilton area, a similar figure to that in Edinburgh. Yet, despite these striking similarities with the Edinburgh trends, a cohort of 240 lithium treated patients at Hamilton Psychiatric Hospital exhibited concurrently a profound drop in expected recurrences, described in our earlier studies.

The important question of lithium efficacy in clinical practice could perhaps be approached more productively by following admissions of a clinical

practice cohort of patients who actually are on lithium.

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References

BALDESSARINI, R. J. (1970) Frequency of diagnoses of schizophrenia versus affective disorders from 1944 to 1968. *American Journal of Psychiatry*, **127**, 759–763.

DICKSON, W. E. & KENDELL, R. E. (1986) Does maintenance lithium therapy prevent recurrences of mania under ordinary clinical conditions? *Psychological Medicine*, **16**, 521–530.

SHEPHERD, M., GOODMAN, N. & WATT, D. C. (1961) The application of hospital statistics in the evaluation of pharmacotherapy in a psychiatric population. *Comprehensive Psychiatry*, **2**, 11–19.

SYMONDS, R. L. & WILLIAMS, P. (1981) Lithium and the changing incidence of mania. *Psychological Medicine*, **11**, 193–196.

SIR: We agree with Schou (*Journal*, December 1986, **149**, 798–799) and Grof (above) that the findings we reported in *Psychological Medicine* last year (**16**, 521–530) do not by any means prove that prophylactic lithium therapy is ineffective, even in our own city. We ourselves emphasised that we could not exclude a number of possible explanations, including changing diagnostic criteria, for the threefold rise in the admission rate for mania that occurred between 1970 and 1981. On the other hand, we failed to find any evidence to support any of these alternative explanations. The samples of case notes we compared (40 from 1970–72 and 40 from 1979–81) yielded no hint either that diagnostic criteria had changed or that the threshold for admission had fallen between these two time periods. We are aware, of course, that diagnostic criteria for mania changed very dramatically in North America in the course of the 1970s. But Baldessarini's comments on the American scene cannot be extrapolated to Scotland. In many parts of the USA a diagnosis of mania was a rarity in the 1960s but this was never so in the UK. For example, in the comparison of admissions to mental hospitals in New York and London carried out by the US/UK Diagnostic Project in 1968 only 0.5% of the New York patients had a hospital diagnosis of mania compared with 6.9% of the London patients (Cooper *et al*, 1972). What is more, Eagles & Whalley (1985) found no significant increase in the first admission rate for mania to Scottish mental hospitals between 1969 and 1978 and it is difficult to see how any major change in Scottish criteria for a diagnosis of mania could have occurred without affecting that rate.

We do not pretend to understand why the admission rate for mania should have increased so much during a time period when the use of lithium was steadily increasing, but we are impressed by the evidence, which neither Schou nor Grof refers to, that lithium withdrawal, deliberate or inadvertent, may result in a temporarily *increased* risk of a manic episode. There are at least four reports in the literature of patients relapsing within a fortnight of their normal lithium tablets being replaced by placebo, and it is not far-fetched to suggest that patients may, for a variety of reasons, end up taking lithium intermittently more frequently under the conditions of ordinary clinical practice than in the context of a closely supervised clinical trial.

We published our findings not to deter others from putting their patients on prophylactic lithium but in the hope that they would provoke them to ask questions about mania and about lithium which they had not asked previously, and to design new studies to answer those questions. In the meantime we cannot do better than repeat the last sentence of our paper – "whatever the true explanation, there is no comfort in these findings for those, including ourselves, who have believed for the last 15 years that maintenance lithium provides an effective prophylactic treatment for at least a substantial minority of patients with recurrent affective disorders".

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References

COOPER, J. E., KENDELL, R. E., GURLAND, B. J., SHARPE, L., COPELAND, J. R. M. & SIMON, R. (1972) *Psychiatric Diagnosis in New York and London*. Maudsley Monograph No. 20. Oxford: Oxford University Press (p. 100).

EAGLES, J. M. & WHALLEY, L. J. (1985) Decline in the diagnosis of schizophrenia among first admissions to Scottish mental hospitals from 1969–1978. *British Journal of Psychiatry*, **146**, 151–154.

Panic Attacks: New Approaches to an Old Problem

SIR: Gelder's paper (*Journal*, September 1986, **149**, 346–352) should not be given more weight than it claims, as a somewhat ephemeral expression of his picture of the subject and reflecting his well-known interest in behavioural psychotherapy. However, there is the danger that some readers might mistake it for a serious appraisal of the subject, placing new ideas in relation to a review of the old ones. In particular, since the paper begins and ends with approving references to Freud, some readers might not realise the almost total omission of everything that Freud thought important on the subject. There is a case for expunging his rather dotty theories of 1895 but it