practice cohort of patients who actually are on lithium.

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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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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

seems a pity to try and understand panic attacks without any recourse whatsoever to the insights available from 90 years of psychoanalysis.

Gelder reminds us that Freud coined the term 'anxiety neurosis' in a very early (1895) paper entitled "The justification for detaching from neurasthenia a particular syndrome: the anxiety neurosis." Gelder summarises some biochemical and neurophysiological hypotheses and a theory involving hyperventilation. Turning to "psychological mechanisms", he does say that they may be equally complicated but "for the present discussion it will be enough to consider one important psychological component: the cognitive changes." This is the only hint that there might be any more to it, any more in the way of human experience relevant to the problem. In particular, there is no glimmering of what Freud's paper was about. The fact is that he was on the verge of discovering the unconscious, the interpretation of dreams and inventing psychoanalysis. In the particular paper quoted, he thought he had discovered something rather different, a theory of "actual neuroses", syndromes due to adverse sexual practices. Almost the entire paper to which Gelder refers is devoted to an exploration of the idea that "anxiety neurosis" is caused by frustrating sexual habits and by coitus interruptus in particular. He thought that neurasthenia, his other "actual neurosis", was caused especially by excessive and compulsive masturbation. These seemingly naive ideas may have made more sense in 1895 but were soon to give way to the larger insights into the workings of the unconscious that changed the shape of the planet for every literate person since. The early stumblings of a genius are fascinating to follow and Freud's *obiter dicta* had a way of getting into everyone else's language afterwards.

After all this it is rather astonishing that Gelder, asking whether the psychological component of neuroticism in panic attacks can be identified more precisely than cognitive theory permits, recalls only that "Freud (1895) stressed the anxious patient's preoccupation with fears of dying, a stroke, or loss of sanity." He adds that Beck *et al* (1974) added fears of heart disease and fainting, which sounds like an observation of almost sublime triviality – the list of expressed fears must be almost endless. Surely we could grasp Freud's point in first describing "anxiety neurosis": far from stressing the presenting symptoms, so oppressive to the patient, a psychiatrist does well to look deeper. It is a weakness of cognitive therapy that it tends to make psychology thoughtless and mindless. Of course, cognitive theory can and should take account of fantasy life and unconscious thought but too often it does not.

I find it piquant that in these very early papers Freud's approach is closer to that of the behaviour therapist than latter-day behaviour therapists seem to notice. He was then very interested in behavioural modification as a two-way interaction with thought modification and he certainly wasn't above telling people what to do. He must, after all, have been one of the astutest clinical observers and most powerful intuitive therapists ever, even though he did warn us of the dangers of *furor therapeuticum*. Furthermore, being originally a celebrated neurologist, he remained all his life deeply preoccupied with the mind–body interrelation, always wondering about the physical basis of mental processes. Perhaps we may look forward to Gelder's re-appraisal of Freud, the behaviour therapist.

Finally, Gelder gives the reference to Freud's paper as it appears in the *Collected Papers* (reprinted 1940). We probably share a sentimental attachment to those old editions but younger readers would be better referred to the Standard Edition of Freud, published jointly by the Hogarth Press and the Institute of Psychoanalysis. Strachey's translation has been criticised but remains a definitive master-work. The editorial introductions to every item are indispensable for anybody liking to place the text in its context and time.

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Psychotherapy and Placebo

SIR: Michael DeMowbray (*Journal*, November 1986, **149**, 666) returns to our debate concerning psychotherapy and placebo treatment by suggesting that "the allotment of the various therapeutic factors to the categories 'specific' and 'non-specific' . . . is purely arbitrary and relative to the theoretical standpoint of the investigator concerned". This surely is a complete mis-statement of the position taken by psychotherapists in general. Whether they embrace the principles of psychoanalysis or gestalt, or whatever, they start with a theory concerning the origins and nature of neurosis, and derive methods of psychotherapy from these principles. It is common knowledge that these principles vary widely from one school to another, and the fact that all are equally successful (or unsuccessful) and do not do better than placebo treatment surely demonstrates once and for all that the theories involved are erroneous, and that the treatment effects are in that sense non-specific. Similarly, the fact that behaviour therapy is significantly more successful suggests that its effects are specifically derived from,