criminals. (We could still, as laymen, criticize them on humanitarian or political grounds, but not as doctors on medical grounds.) There would also be no answer to Szasz's thesis, other than the general social argument that madness is undesirable and that doctors are better equipped to deal with it than other people. Perhaps none of these things worry Professor Jenner; but they worry me.

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PARENTS OF BATTERED CHILDREN

DEAR SIR,

The well-disciplined study by Selwyn Smith and Ruth Hanson (*Journal*, December 1975, 127, pp 513–25) shows some important statistical differences between the child-rearing behaviour of battering parents as compared with controls.

There are two unrecognized tendencies which both work towards submerging the observed differences between abusive parents and controls.

1. Battering parents *attenuate* accounts of accustomed rearing practices and battering incidents, whether or not they give direct admissions of guilt.

Such parents have responded to subtle cues which betray the attitudes of others. Unlike the 'control' parents, they have had a lifelong experience of doing just this, having themselves usually been victims in childhood. Subsequent accounts either of the battering incidents or of rearing practices are modified accordingly. 'I couldn't stand his crying, and shook him until he went limp' may be the culmination of incompetent rearing, or using the baby as an emotional prop for an inadequate mother, rather than a single incident.

2. Battering parents have an inaccurate or no yardstick of normality. Thus, an item such as 'Severe in training methods', or 'obedience demanded', or 'allows to cry unless something obviously wrong', will mean something quite different to an abusive parent from what the same phrase would mean to a control parent. The same applies to the 'frequent use of smacking . . . withholds love . . . rarely deprives, rarely praises', etc.

Without these two tendencies Smith's and Hanson's findings would have been even more significant, and further items of marginal significance might have been shown to have been important.

No one will now be able to take refuge in anodyne beliefs such as, on the one hand, 'Any parent is a potential batterer', or on the other 'People who batter children must be mental'. The reality is more complex.

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DIURNAL VARIATION AND ENDOGENOUS COMPONENT OF DEPRESSION

DEAR SIR,

We wish to report a research in which we examined the classical psychiatric opinion that endogenous depressives tend to improve towards evening. The limited research upon this concept has not established it as a fact (Kiloh and Garside, 1963; Rosenthal and Klerman, 1966; Stallone *et al*, 1973). In our research we employed well constructed scales for assessing both variables.

Subjects were 20 heterogenous depressives not suspected of being schizophrenic, mentally retarded, or organic. The Depression Category-Type Scale (DCTS) of Sandifer *et al* (1966) was used for determining the degree to which depression was endogenous. The Diurnal Variation Rating Scale (DVRS) was used for what its name implies.

The DCTS product-moment correlation for the 13 patients interviewed the day of admission by both H.K. and A.E. was ·87; that for the 17 interviewed by both H.K. and D.T. ·80; that for the 16 interviewed by both A.E. and D.T. ·87 (all $ps < \cdot 01$). The DCTS mean of the two or three interviewers was used for each of the 20 patients. The DVRS, for which clinical impression is practically nil, was administered at 5 pm on the next three consecutive days. The correlation between first and second DVRS score is ·82; that between first and third ·72; that between second and third ·79 (all $ps < -\cdot 01$). Mean DVRS score for the three days was used.

The correlation between DCTS (upon which a higher score indicates a greater endogenous component) and DVRS (upon which a higher score indicates improvement towards evening) is $-\cdot 01$ (NS). However, this does not necessarily imply that a relationship between the two variables never exists. The period in the course of a depression could be relevant, as suggested by Waldman (1972), who maintained that diurnal variation ceases at the depth of endogenous depression and reappears as it improves. DVRS scores indicated improvement as the day progressed for 17 of our 20 patients. This is

https://doi.org/10.1192/bjp.128.5.509a Published online by Cambridge University Press

consistent with Hamp's (1961) work with normal individuals who displayed evening mood elevation. Perhaps such diurnal variation is more widespread than an occurrence in endogenous depressives.

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CONTINUED NEED FOR PLACEBOS

DEAR SIR,

The superiority of imipramine over placebo has been clearly demonstrated by Drs Rogers and Clay (*Journal*, December 1975, 127, pp 599–603) though there are at least two simpler methods of drawing the same conclusion about efficacy: via the overall response rates and via the trends of individual trials.

In their summary, however, the authors state that further drug-placebo trials in non-institutionalized patients with endogenous depression are not justified. This is not so. It is more ethical to use placebo as a control than imipramine.

Consider a new drug which has gone through an exhaustive series of uncontrolled trials from which it could reasonably be expected that 70 per cent of such patients will show a 'greatly or moderately improved' response. The therapeutic benefit is not yet confirmed; there is reasonable doubt and some sort of quantitative evaluation is desirable and justified. A controlled trial is needed. But which control—imipramine or placebo?

Sixty-five per cent of the imipramine patients can be expected to respond adequately (the authors have shown), contrasted with 70 per cent of patients on the new drug. To reach a statistically significant result at the 0·05 level will require a trial with 1,000 patients if the original premise is correct. Five hundred patients will receive imipramine, of whom 65 per cent can be expected to respond and 35 per cent not to do so. Thus 175 patients on the control treatment can be expected to show an inadequate response.

If, on the other hand, a placebo control were to be used with a response rate of say 30 per cent (the first 14 trials in their Table I gave a response rate of ·32 with a standard deviation of ·19) then the controlled trial with the new drug will require about 30 patients. Of these, 15 will be on placebo and 10 of these can be expected to show an unsatisfactory response.

I have no difficulty in justifying the continued use of placebo, even though the value of imipramine is beyond dispute.

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SCHIZOPHRENICS' FAMILIES

DEAR SIR,

Fowler and Tsuang (*Journal*, January 1976, pp 100–1) take issue with our finding (*Journal*, August 1975, 127, pp 97–108) of more personality disorder in the families of our schizophrenic probands than in the controls. In fact there does not appear to be any substantial point of disagreement between our data and those of Fowler and Tsuang, but rather a difference of terminology. Those relatives whom we regarded as suffering from the kinds of personality disorder which our analyses suggested were biologically akin to schizophrenia they would have called cases of 'suspected schizophrenia'. Those illnesses which we did not think were biologically related to schizophrenia they found in their families to be 'transmitted independently of schizophrenia'. In our study this applied to affective disorders, neurotic reactions (except possibly in females) and neurotic personality disorders, subnormality and suicidal behaviour, while Fowler and Tsuang mention particularly affective disorder and alcoholism. As regards the latter, some but not all of our cases were thought to have arisen on the basis of personality disorder of the kind related to schizophrenia, while Fowler and Tsuang emphasize that 'alcoholism and some personality disorders in the families of