for making this distinction, and I believe that collaborative study interviewers did not add additional questions to their interview to cover this distinction. If not, it is then impossible for the authors of this article to convert their SADS interview into a DSM-III diagnosis or to derive a sub-type characterized by pervasive anhedonia.

I submit that perhaps they have identified one group which has pervasive loss of interest without the traditional vegetative signs of depression while another group has pervasive loss of pleasure plus the neuro-vegetative signs, as has already been hypothesized by Klein.

It seems logically impossible for a patient to meet DSM-III criteria for melancholia without meeting at least probable criteria for RDC endogenous depression. Thus, it appears to be a mistake to report 14 patients with DSM-III melancholia who did not also have at least probable RDC endogenous depression.

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Reliability of the SRQ-20

SIR: We were interested to read the paper by Drs Mari & Williams (*Journal*, January, 1986, **148**, 23–26). We used the SRQ-20 as a screening instrument among in-patients of a general medicine ward at the AIIMS over a six-month period. Using 11 as the cut-off point, we examined (using the ninth edition of the Present State Examination and a detailed psychiatric history) all those scoring 11 or above as well as a systematic sub-sample of those scoring below 10.

Out of a total of 326 administered the SRQ-20, 158 were examined and diagnosis was made on the basis of ICD-9. At the end of the study, specificity and sensitivity were determined at each possible cut-off point from 1 to 20, based on the 42 psychiatric cases and 116 non-cases. A correction factor was applied so as to generalise the results to the whole sample using the method suggested by Goldberg (1978).

We found great divergence between specificity and sensitivity. The original cut-off score of 11 had a sensitivity of 46.6%, and specificity of 74.1%. At 7/8, these were, respectively, 77.4% and 66.6% (sensitivity) and 51.1% and 54.4% (specificity). We found 9 the ideal cut-off, where sensitivity was 62.9% and specificity 62%. Females, and those educated at school level (matric) or less, scored significantly higher ($P<0.05$ and $P<0.01$ respectively, using the chi-squared test). The mean score for the total population was also high—7.78 (SD $\pm$ 4.95).

We feel, therefore, that the SRQ-20 may need considerable modification for use in known physically ill samples. Weighting certain questions, skewing questions toward the more "psychological", changing "false positive" questions, and defining the duration or severity of symptoms, may all have to be considered.

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Monoamine Oxidase Inhibitors

SIR: Professor Blackwell's warning (*Journal*, February, 1986, **143**, 216–217) about the danger of minimising the risk of MAO inhibition is welcome, if only as the basis for informed discussion. However, as he will no doubt recall (Samuel & Blackwell, 1968), he himself was not always on the side of the angels.

At Bethlem Royal Hospital, the reaction to Rowe's initial observation, shared by Professor Blackwell, was one of ridicule, while my suggestion that Rowe's observation be taken seriously was treated with contempt. My persistence in pursuing the matter was encouraged only by Professor Linford Rees. It is to the credit of neither of us that this encouragement has never been recorded by Blackwell in any of his many publications, nor in the publication of which I was a joint author (Samuel & Blackwell, 1968). This letter will be the first time in the 23 years since this not altogether unimportant discovery was made, that Professor Rees' essential contribution will have been acknowledged in print. It was my pleasure, in 1983, finally to speak to Rowe. To my astonishment he was totally ignorant of the

sequel to his observation and did not know of his mention in any publication nor in Ayd and Blackwell's (1970) book, *Discoveries in Biological Psychiatry*.

I suspect that Professor Blackwell is once again suffering from 'cryptoamnesia', this time in his reference to "manufacturers' myopic files". In the 1970s, such files were part of his responsibility, when working for a "manufacturer", and if 'myopic' who should be blamed? However, such a situation is not true in the 1980s, here in the UK. In 1963, medical advisers in the pharmaceutical industry felt it was their job to push reports of adverse reactions aside ("it would be premature and unscientific to warn patients not to eat cheese": Weber, 1963). Today, all serious side-effects have to be recorded within 48 hours of notification to the company. We no longer shoot the mesenger.

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On Serious Violence During Sleep-Walking

SIR: Ian Oswald and John Evans in their article (*Journal*, 1985, **147**, 688–691) added important information to our knowledge about violence during sleep. Other sleep disorders, in addition to somnambulism (Podolsky, 1958) can also generate violent behaviour (Raschka, 1984). The term "sleep-related dangerousness' introduced by Moldofsky appears useful. Such behaviour can include car accidents related to narcolepsy and assaultive behaviour related to sleep drunkeness. The criminological and forensic implications of the latter were discussed by Bonkalo (1974). Sleep disorders generating sleep-related dangerousness are often influenced by drug intake including alcohol. The resulting behaviour is often preventable.

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INDEX TO VOLUME 148

We apologise for the delay in publication of the **Index to Volume 148 (January to June 1986)** which is now enclosed.

A HUNDRED YEARS AGO

"It might seem a painful thing to deprive of liberty persons who appear so little ailing, were it not that a repeated experience has shown it to be really a vain folly, if not a positive cruelty, to send them forth into the trials of life, when they are utterly unable to encounter them."

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