to consolidate the response. An analysis of the relapse rates on placebo of first-time depressives who would not normally be expected to develop a new episode of depression for some time shows a very similar relapse rate compared with that seen in recurrent depressives (Mindham *et al*, 1973). This strongly suggests that for those who stop antidepressants immediately after response, relapse in the subsequent six months is a return of the symptoms of the inadequately treated depression. It is necessary to continue treatment for at least four months following response to ensure that the episode of depression is properly treated. This simple advice is often ignored, sometimes by patients but mainly, we believe, by doctors, who seem as yet to be unaware of the importance of this. The average duration of treatment for a course of antidepressants in the UK is currently estimated to be six weeks.

The study of fluoxetine (Montgomery *et al*, 1988), which Professor Fahy praises did not address the issue of maintenance treatment to consolidate the response. All patients who consented to the study were treated for a six-month period with the antidepressant to ensure a persistent response. This study was attempting to address a different issue: whether long-term treatment in recurrent unipolar depression reduced the chances of the development of further new episodes of depression. This is not as Professor Fahy states, mere hair-splitting. The use of a drug to treat an episode of depression is quite different from the use of a drug to prevent a new episode of depression arising, a distinction emphasised by Montgomery (1989). There are very few good studies for secondary prophylaxis although a case can be made for fluoxetine, imipramine and possibly lithium. With amitriptyline, design flaws in the two small studies conducted leave room for doubt. For most antidepressants, we do not know whether they are effective in prophylaxis or not.

The point where we take issue with Professor Fahy is his assumption that any antidepressant will do as they all, he implies, have efficacy in maintenance and prophylactic use. He appears to find it ethical to offer long-term treatment with an antidepressant although there are doubts about long-term efficacy. We would prefer to be open with our patients and tell them that some like Professor Fahy believe that any antidepressant works while others like ourselves have doubts. We would invite the patients to participate in a carefully-controlled study to help establish this important point. We would spell out the risks and benefits in order to help them make an informed decision. This seems to be the most ethical course to adopt.

Much of Professor Fahy's belief that any drug will do is based on his understanding that no well controlled investigation of continuation or prophylaxis has failed to show efficacy. This approach is risky. Lithium was found to be no better than placebo in recurrent unipolar depression by Prien *et al* (1984). More recently, Georgotas *et al* (1989) reported that nortriptyline was ineffective compared with placebo whereas phenelzine was effective in a one year study of prophylactic treatment. Results like these emphasise our need for caution and suggest that we should require a convincing demonstration of long-term efficacy of an antidepressant, preferably against placebo, to avoid putting patients at risk.

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Prison suicides

SIR: Dooley (*Journal*, January 1990, **156**, 40–45) draws attention to the important problem of suicide in prison, but makes some inferences from the data which are questionable, and comes to conclusions which are disappointing in making so few recommendations for change.

Firstly, is it true that suicide in prison is increasing? The rates quoted by Dr Dooley for 1976–1987 are remarkably similar to those given by Topp (1979) for 1958–1971. It may be that in the years 1972–1975 there were relatively few suicides and that the rate has simply returned to baseline. The general characteristics of prison suicide described by the two authors are very similar, suggesting a stable rather than a changing phenomenon.

Secondly, how good is the evidence that prison suicides are a consequence of mental disorder? The

prevalence of past psychiatric history quoted is 33%, again very similar to that of Topp (38%) and Backett (39%) (1987). These rates are no higher than have been found among general hospital patients admitted after episodes of non-fatal deliberate self-harm, the majority of whom are socially deprived and distressed rather than mentally ill. They are lower than those found among people who kill themselves outside prison, the majority of whom may be mentally ill. In this respect the sentenced prisoners described by Gunn *et al* (1978) are not an adequate control group, since half of Dr Dooley's sample were on remand or unsentenced.

Common sense might suggest that people who kill themselves in prison do so *because* they are in prison. The data tend to support that view, since suicide has been such a persistent risk of imprisonment across the years and is not overwhelmingly associated with mental illness. Again, surely the explanation for an increase in prison suicides in the summer months is that it is during those long hot days that loss of liberty becomes hardest to bear?

Such an interpretation leads to the conclusion that it is institutional reforms which are needed if the suicide rate in prison is to be reduced, and two spring readily to mind. Firstly, fewer people should be held in prison on remand. The proportion of non-sentenced prisoners in Topp's cohort was 37%; in Dr Dooley's it was 47%. Among the 68 people who killed themselves in prison in 1987 and 1988, 74% were on remand (*Hansard*, 1989). These figures confirm that remand prisons are unsuitable places in which to assess and respond to the psychological and social needs of vulnerable and distressed people (Howard League, 1986, 1989). Secondly, we should make far less use of custodial sentencing for non-violent crimes (Blom-Cooper, 1988). Half of Dr Dooley's sample killed themselves while imprisoned for crimes against property. We no longer hang people for murder; it is a great irony that they should hang themselves while in prison for lesser offences. Conditions in British prisons are a national scandal: they are overcrowded, disorganised and insanitary. If they were emptied of trivial offenders it would be for the general good. It would also increase the opportunities for a reformed prison medical service to respond to the needs of the long-term prisoner whose suicidal despair is (as Dooley points out) so often tragically unidentified under the present system.

In the 1890s the authorities responded to the problem of suicide in prison by putting up safety netting between landings. In the 1990s, should they respond by redesigning cells so that desperate prisoners cannot garrotte themselves on their bars? We must do better than that. The suicide rate in prisons raises important ethical questions about our social and penal policies. Psychiatrists (both individually and through the College) should press for reforms in the prison system with a vigour equal to that which they show in proposing reforms in the practice of forensic psychiatry in other countries. To do less is simply hypocritical.

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Outcome in unipolar affective disorder after stereotactic tractotomy

SIR: The report by Lovett *et al* (*Journal*, October 1989, **155**, 547–550) confirms our finding that a better outcome, especially in unipolar affective disorder, can be achieved by simpler surgical techniques such as bimedial frontal leucotomy (Hussain *et al*, 1988).

In our series, there were 15 cases of unipolar affective disorder. Of the 13 who had a non-stereotactic operation, 11 had a good outcome at follow-up, and of these, eight achieved a complete recovery. Neither of the two stereotactic cases however, had a good outcome. Our study was similar to Dr Lovett's in, for instance, the long histories of depressive disturbances in mood (either recurrent or continuous), the resistance to all forms of treatment available, and the assessment approach. However, the length of follow-up was longer in our study – a mean length of 108 months, compared with 69. It would seem that the stereotactic tractotomy lesion in the lower medial quadrant of the frontal lobe is unable to give a better result than the simpler modified bimedial approach, which not only produces a lesion in the lower medial quadrant but also severs the connection from the frontal lobe to the cingulate gyrus – the fronto-limbic northern pathway (Kelly, 1976).