One of the tenets of cognitive therapy is the Socratic method of questioning which assumes that no issue is sacrosanct and that the answer to many of life's conflicts can be discovered through the application of reason. This method soon flounders in non-Western patients when it is discovered that many of the emotionally significant areas are taboo to such a questioning process.

It is interesting that the authors have suggested an analogy between cultural development and individual cognitive development as described by Piaget.

This may be an attractive model to borrow but it seems to me that there are risks in doing this. Piaget's model of cognitive development assumes a *progression* through a number of stages where each stage is superior to the stage that precedes it. If such a concept is applied to the social context, the underlying assumption would be that certain societies are superior to others; a sort of 'march of progress' or social Darwinian view of human societies, so that, rather than accepting the differences in psychological make up of humans in different cultures as part of the phenomena of human cultural diversity that exists in our world, a value judgement will be implied of a culture's place in an assumed hierarchy. This would be a retrograde step of dubious scientific merit.

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Safety of 5-HT reuptake inhibitors

SIR: The letter from Waite (*Journal*, December 1991, **159,** 885) suggests that Healy (*Journal*, June 1991, **158,** 737–742) was over zealous in his recommendations of this new group of drugs. He quotes data from the Committee on Safety of Medicines (CSM) for fluoxetine up to July 1990 and it would appear he regards the numbers of adverse effects and deaths as excessive.

Up to September 1991 there were 16 recorded deaths on fluoxetine (CSM data): ten from cardiovascular events, one from liver disorder, and five from suicide (method not specified). Causality, however, cannot necessarily be implied and, interestingly, several of the cardiovascular deaths occurred in patients in their 90s.

It is almost impossible to compare CSM reports between drugs. Pinder's paper (1988) quoted by Dr Waite makes a number of interesting points in this regard. Firstly, reporting rates rose sharply in the 1970s (CSM, 1985) and most reports are made in the first few years of marketing a new drug. Secondly, higher antidepressant reaction reporting rates may not necessarily reflect a higher incidence of actual reactions. Thirdly, newer antidepressants may be selectively prescribed in patients in an 'at risk' population such as the elderly and those with cardiac disease. The older tricyclics have been available for so many years, and their side effects – such as cardiac toxicity, cognitive impairment and toxicity in overdose – are so well known that under-reporting is bound to occur.

Cassidy & Henry's (1987) work on fatal toxicity indices highlights the mortality associated with older tricyclics, and figures quoted from coroners' data on overdose deaths are far in excess of total deaths in the CSM figures. We should, therefore, be cautious in interpreting CSM figures in isolation.

The 5-HT reuptake inhibitors are relatively safe in overdose – for fluoxetine on an estimated patient base of five million worldwide, reports of death attributed to overdosage of fluoxetine alone have been extremely rare. Pharmaceutical companies who produce selective 5-HT reuptake inhibitors may welcome Dr Pinder's request in the last paragraph of his paper for an inclusion of overdosage risk in any considerations leading to recommendations for approval, renewal, restriction or withdrawal of product licences for antidepressants. The older drugs, however, may find that such regulatory changes will render them moribund.

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Serotonin, eating disorders, and HIV infection

SIR: We read with interest Ramsay's article (*Journal*, March 1992, **160,** 404–407). We would like to comment on the exacerbation of symptoms of the eating disorder during the development of HIV disease.

Serotonin is one of the neurotransmitters which is involved in the control of food intake in physiological and/or pathological situations such as anorexia nervosa and bulimia nervosa (e.g. Blundell, 1984) in which plasma tryptophan and CSF 5-hydroxy indoleacetic acid concentrations are decreased (Coppen *et al*, 1976; Kaye *et al*, 1984).

In patients with HIV infection, serum tryptophan is also low (Werner *et al*, 1988; Larsson *et al*, 1989), blood serotonin is very low (Larsson *et al*, 1989) and neopterin and kynurenine levels are elevated (Werner *et al*, 1988). These changes are thought to be due to the induction of indoleamine 2,3-dioxygenase enzyme by cytokines (Werner *et al*, 1989). Further reductions of tryptophan, serotonin and increased kynurenine levels may therefore contribute to explain the exacerbation of symptoms of the eating disorder during the development of HIV disease.

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Suicide prevention

SIR: The west country district mentioned in Professor Morgan's paper (*Journal*, February 1992, **160**, 149–153) was one of three serviced by two mental hospitals which were among the first in the country to close (1986, 1987). We have examined rates of suicide and open verdicts for the three districts between 1983 and 1990. The following figures are expressed as standardised mortality ratios (mortality ratios corrected for age) with 95% lower confidence limits in parentheses. The overall male suicide rate was 140 (127) which, when open verdicts were taken into account, reduced to 116 (106); for females, the respective figures were 130 (112) and 107 (94). Analysis of the rates over time did not reveal any significant differences from the national average.

These figures indicate that the high male suicide rate was present even before the hospital closures, and that the move to community care has had no demonstrable effect on suicide rates.

The author makes useful points about suicide prevention. One of the districts is aiming to reduce the level of attempted suicide by 20% over ten years (Gentle, 1990). Achieving this goal will be beyond the remit of health authorities necessitating changes in not only health but also social, economic, cultural and political areas.

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SIR: The whole issue of in-patient suicides is very interesting and very emotive. This was exemplified by the recent study of in-patient schizophrenic suicides (Modestin *et al*, *Journal*, March 1992, **160**, 398–401). The authors cited a number of studies indicating the importance of schizophrenia for in-patient suicides. In a study of violent behaviour, over 15 months, our group (James *et al*, 1990) observed 14 episodes of attempted suicide among in-patients in our locked ward, all by schizophrenics ($\chi^2 = 9.48$, $P < 0.005$). Despite the supportive and supervised environment of the ward, schizophrenics are vulnerable to attempted suicides or suicides, suggesting such acts are difficult to predict among this group.

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SIR: Professor Morgan's report (*Journal*, February 1992, **160**, 149–153) provoked the following thoughts.

We psychiatrists know about the number of suicides we could not prevent. But do we have any information about the number of suicides we have probably averted? How reliable are our suicide predictions? How many of those whom we consider highly suicidal would commit suicide if not intervened?

Have the suicide rates fallen with the advances in psychiatric diagnosis and treatment? Have we