thought relaxation was as credible as exposure.

With respect to Marks' comments about likely active ingredients in our cognitive therapy programme, it would appear that he has not read the article carefully. We make no claims about the relative potency of cognitive and behavioural procedures and explicitly state (p. 224) that the study design did not allow us to determine which of the many cognitive and behavioural procedures that distinguished cognitive therapy from behavioural stress management were responsible for the former's superiority. Marks also appears not to have noticed that assessment of the session tapes detected "no instances of either in-session or homework exposure to avoided illness-related situations (hospitals, television programmes, etc.)" and that "reassurance . . . was not often detected and the two treatments did not differ" in this respect (p. 220). Finally, we did not cite any controlled studies demonstrating that exposure alone or verbal disputation alone are specific treatments for hypochondriasis because none exists. After acceptance of our paper, data that these procedures are better than no treatment were produced by a Dutch group but there is no evidence that they have a specific effect (i.e. are better than an attention placebo condition).

Like Marks we are very interested in the question of which cognitive/cognitive–behavioural procedures are most effective. However, we differ in our views on the best ways to answer this question. Marks *et al* (1998) favour large-scale component analysis treatment trials. Because of their failure to deal with dose response issues, and other logical and variance control problems inherent in their design, we consider such trials insensitive instruments for detecting additive effects of cognitive and behavioural procedures. For this reason, we favour much tighter, single-session experiments (see Salkovskis *et al*, 1999, for a successful example of this methodology).

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Antipsychotic polypharmacy and early death

Sir: The article by Waddington *et al* (1998) is an example of careful audit over a prolonged period. For this, the authors are to be thanked, especially since such 'captive' populations and hence the possibility of such studies, is fast disappearing. However, critical comments are necessary. First, the major conclusions are presented in terms of statistical significance and one of these, the finding that 'polypharmacy' is a contributory cause of early death, is so alarming that it may well have been taken up by a vituperative press seeking to vilify psychiatry and all its works. However, the statistical information presented is so weak as not to be regarded as having significance. It is a curious and contradictory observation that both polypharmacy and withdrawal from medication both contributed, in the same direction, to demise. The article would have been more helpful if actual numbers, or at least median values, had been presented. Means and standard deviations, even with the addition of ranges, provide no clear information. For instance, apparently medication had been stopped in some patients but in how many and for what reason is not stated. I should like to believe that the series of investigations to which I contributed (Andrews *et al*, 1976), which demonstrated that continued medication in such a chronic population was of no value, had had some influence. Then the absence of information regarding clozapine (with its recognised lethal potential and for which careful monitoring is *de rigueur*), is a defect of the study; it is possible that earlier demise occurred in just such a context. Finally, it is a pity that all causes of death were lumped together; the opportunity has been missed to contribute to knowledge as to whether or not high-dose antipsychotic medication is verified as a cause of cardiac disease and death.

Andrews, P., Hall, J. N. & Snaith, R. P. (1976) A controlled trial of phenothiazine withdrawal in chronic schizophrenic patients. *British Journal of Psychiatry*, **128**, 451–455.

Waddington, J. L., Youssef, H. A. & Kinsella, A. (1998) Mortality in schizophrenia. Antipsychotic polypharmacy and absence of adjunctive anticholinergics over the course of a 10-year prospective study. *British Journal of Psychiatry*, **173**, 325–329.

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Authors' reply: We appreciate the controversial nature and possible unpalatability of some of the associations that we report, but are disinclined to accept a number of Dr Snaith's strictures. Regarding statistical issues, our major findings are *not* presented in terms of significances but, rather, in terms of relative risks with 95% confidence intervals, in accordance with the 'statistics' section of the *Journal*'s 'Instructions to Authors'. We do not find that antipsychotic polypharmacy is a *contributory cause* of early death; that is one of several interpretations of our finding of an *association* between antipsychotic polypharmacy and early death. Our statistical approach and data presentation are conventional (Altman & Bland, 1998), with Cox proportional hazards modelling accepted as a method of choice for examining a set of variables for independent predictors of survival. There is no contradiction in both antipsychotic polypharmacy and time since final withdrawal of antipsychotics predicting reduced survival. As stated in our article, the index of polypharmacy is the maximum number of antipsychotics given concurrently, to cover instances where this occurred prior to the index evaluation such as when antipsychotics had been withdrawn; both are identified by Cox modelling as independent predictors of reduced survival (i.e. each variable is associated with reduced survival after controlling for the influence of the other). It was not always straightforward to specify on an individual basis the reason(s) for antipsychotic withdrawal (of which there were 20 instances); we accept Dr Snaith's point that a lack of perceived value in continuing antipsychotic treatment may have contributed to its withdrawal in some patients, in addition to our own speculation in terms of terminal physical illness replacing psychiatric disorder as the primary focus of medical care.

Regarding clozapine, our article states that all patients received classical neuroleptics (10-year follow-up concluded in 1993; by then, five patients were receiving sulpiride). Causes of death were not "lumped together" but, rather, aggregated by ICD–9 categories in accordance with general population statistics issued by the Department of Health; individual details on any of the above are available from us on request. Our data indicate that it is not increasing dose that is associated independently with reduced survival but, rather, increasing number of antipsychotics given concurrently; there were insufficient data to explore individual causes of death in relation to medication.

Altman, D. G. & Bland, J. M. (1998) Statistical notes: time to event (survival) data. *British Medical Journal*, **317**, 468–469.

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Shotguns and blunderbusses: suicide in farmers

Sir: I read with interest the Oxford study on the methods used by farmers to commit suicide over a 13-year period (Hawton *et al*, 1998). The authors demonstrated that the method of choice used by farmers in England and Wales was that of firearms, followed by hanging and carbon monoxide poisoning. The authors stated that "the ownership of firearms by farmers should be questioned". The paper, originating from the city of dreaming spires, demonstrates a lack of understanding of rural issues, and is disingenuous in its conclusions. Farmers unquestionably require firearms to control vermin, including rabbit, mink, crow and other infestations.

Working in a remote area of rural Scotland, I do not have access to detailed population statistics; however, a rule of thumb will suffice for this purpose. Assume that the population of England and Wales is approximately 50 million; that 1 in 75 of us die each year; then over 13 years (the period of the study), there will have been 8.7 million deaths. The authors imply that banning the ownership of firearms to farmers might prevent up to 285 deaths. This would have reduced the number of deaths in England and Wales by a factor of 0.003%. This does not appear to be a very impressive public health measure, even if it worked and if farmers did not choose to use alternative and rather more conventional methods such as analgesic or antidepressant overdosage. The rate of death due to firearms in this group has in any case been declining throughout this period (and not just since 1989, the date of firearms legislation, as the authors suggested).

Of course, if the authors were to suggest that in addition to being prevented from owning firearms, that farmers were also prevented from owning ropes, baler-twine and washing-lines (risk of hanging), and cars and agricultural vehicles and machinery (risk of carbon monoxide poisoning), then 613 deaths might be prevented, reducing the mortality in England and Wales by a whole factor of 0.007%! Perhaps Hawton *et al* would prefer to see analgesics and antidepressants banned for townsfolk. By a similar reckoning, this would have a much greater impact in reducing deaths.

Hawton, K., Fagg, J., Simkin, S., *et al* (1998) Methods used for suicide by farmers in England and Wales. The contribution of availability and its relevance to prevention. *British Journal of Psychiatry*, **173**, 320–324.

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Author's reply: Dr Collacott's letter contains some surprising reasoning. First, to calculate the impact of suicide prevention in terms of the proportion of the overall number of deaths from all causes in the general population trivialises suicide prevention as a public health measure. Second, prevention of suicides in an individual group is surely worth pursuing, especially when that group has an elevated risk, as is the case with farmers (Charlton *et al*, 1993; Kelly & Bunting, 1998). Third, reducing the availability of means for self-inflicted death is recognised as being an important component of suicide prevention. We were not suggesting that *all* farmers should be prevented from owning firearms, but that access to lethal weapons should be restricted for farmers known to be at risk of suicide. It is surely important to restrict access to firearms when a farmer (or indeed any other individual) is known to be at risk, such as during a severe depressive episode. One well-established fact is that unavailability of one method does not mean that a suicidal individual will automatically turn to another method. Also, some survivors of firearm suicide attempts report similar impulsivity in their actions to that often found in patients presenting with overdoses (De Moore *et al*, 1994).

The recent publication describing the results of our research team's work on suicide in farmers indicates several potential strategies for suicide prevention (Hawton *et al*, 1998). However, restricting access to means will always be one important strategy.

Charlton, J., Kelly, S., Dunnell, K., *et al* (1993) Suicide deaths in England and Wales: trends in factors associated with suicide deaths. *Population Trends*, **71**, 34–42.

Hawton, K., Simkin, S., Malmberg, A., *et al* (1998) *Suicide and Stress in Farmers*. London: The Stationery Office.

Kelly, S. & Bunting, J. (1998) Trends in suicide in England and Wales, 1982–96. *Population Trends*, **92**, 29–41.

De Moore, G. M., Plew, J. D., Bray, K. M., *et al* (1994) Survivors of self-inflicted firearm injury. A liaison psychiatry perspective. *Medical Journal of Australia*, **160**, 421–425.

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Satisfaction of carers at home

Sir: Szmukler *et al* (1998) wondered whether the greater satisfaction of the carers of patients treated at home compared with hospital in the study of Marks *et al* (1994) might reflect the fact that the patients were being considered for admission at the time and had "enthusiastic experimental teams engaging in an exciting new form of care".

Two facts make the above explanations unlikely. First, the relatives' satisfaction with home care did not become significantly superior to satisfaction with standard hospital care until fully 11 months after patients had entered the study, well after admission had ceased to be an issue. Second, the relatives' significantly superior satisfaction with home rather than standard care continued when the patients were stable in the fourth year of the study, despite the experimental team by then having long been demoralised and without enthusiasm.

The relatives' superior satisfaction with home over standard care may more likely have reflected a preference for treatment

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