for 30–40% of the variance in child antisocial behaviour (Patterson *et al*, 1989).

It seems likely that parenting practices would not be adequately controlled for through the use of a socio-economic covariate owing to the fact that, although parenting practices are influenced by social and cultural factors such as class (Hoff *et al*, 2002), one of the most extensive epidemiological studies of childhood psychiatric disorders showed that social class was a poor predictor of child adjustment (Rutter *et al*, 1975).

It seems likely that parenting exerts an independent effect on child outcomes such as emotional and behavioural adjustment. The ALSPAC data contain a number of measures of parenting, including, for example, a standardised instrument measuring parenting practices during toddlerhood. It would be useful if further analysis of this data-set were undertaken to establish whether these important findings are maintained when parenting is included in the model.

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Author's reply: A recent study from our group indicated that antenatal anxiety is associated with a significantly increased risk of behavioural/emotional problems in young children (O'Connor *et al*, 2002). The study was based on the ALSPAC cohort, a prospective, longitudinal study of women followed since pregnancy. Analyses indicated that antenatal anxiety at 32 weeks' gestation was associated with an approximately 2-fold increase in behavioural/emotional problems in boys and girls at age 4 years; these associations were observed after accounting for key antenatal, obstetric and psychosocial risks, and postnatal anxiety and depression. The findings are important in providing the strongest evidence to date that the substantial evidence for long-term effects of antenatal stress/anxiety found in numerous animal investigations (e.g. Schneider & Moore, 2000) may extend to humans.

In our paper, the focus was on whether or not the antenatal environment had a role in the development of behavioural/emotional problems, an issue with substantial implications for our understanding of development, as well as for prevention and public health. Dr Barlow's letter helps draw attention to a separate research base linking behavioural/emotional problems in children with postnatal factors, particularly parent–child relationship quality. Although there remain some controversial matters in that field of research, especially concerning causal mechanisms (see O'Connor, 2002), parent–child relationship quality is certainly a robust predictor of children's psychological development. Given the multiple-risk nature of development and psychopathology, we would agree with Dr Barlow that there is a need to bring together findings from different lines of research and to revise our models and theories that consider multiple levels of risk. Indeed, there are a number of directions for this research to pursue, including the consideration of how postnatal experiences such as parent–child relationship quality moderate the effects of antenatal anxiety/stress and how the role of genetic factors may explain individual differences in response to antenatal anxiety/stress. Research along these lines is underway. Because it has tracked women intensively since pregnancy and has continued to collect information on a wide range of biological and psychosocial variables, the ALSPAC study is an especially important resource for studies of this kind.

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Brain weight in suicide revisited

In their excellent paper Hamilton & McMahon (2002) examined brain weight in suicide victims of all ages to see whether it was higher than in a control group. They attempted to replicate and reinterpret our findings (Salib & Tadros, 2000) reported in an elderly sample. The authors, quite rightly, looked at brain weight in cases and controls adjusted for body mass index (BMI), having collected additional data about body weight and height, data which Salib & Tadros (2000) were not able to collect and which was already accepted as a major limitation in the latter study.

Hamilton & McMahon (2002) did not find any significant difference between brain weight adjusted for BMI in cases and controls. However, brain weight was significantly higher in those dying by hanging than in those dying by overdose.

I would like to make one or two comments which may help to explain the difference in the findings of the two studies. In Hamilton & McMahon's study, the mean age is 38.5 years (for cases and controls) compared with 72 years in the study by Salib & Tadros (2000). Also, the mean brain weight for Hamilton & McMahon's control group was 1449 g compared with 1238 g in the sample reported by Salib & Tadros (2000). Hamilton & McMahon (2002) included only 6% of subjects aged over 60. The method of selection of the control group in their sample is different from that used by Salib & Tadros (2000) – the latter study included only controls who died naturally and not accidentally. Hamilton & McMahon (2002) were not able to replicate our findings in an elderly sample but were careful in their comparison of the findings by taking into account the differences in some basic parameters in the two studies.

It is interesting to note that another recently published paper (Balazic & Marušič,

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2002), which included patients of all ages, has confirmed our findings of a higher brain weight in suicide (Salib & Tadros, 2000) but only after they excluded 'outdoor' cases (where the body was found at an outdoor location away from the home) and controls. Hamilton & McMahon should be congratulated on their study; however, the negative findings in the younger age group may have been confounded by the choice of the control group, some of whom may have had a mode of death not dissimilar to suicide but had a non-suicide verdict returned by the coroner. On the other hand, Hamilton & McMahon (2002) are correct in making the assumption that our control group may have included people with pre-clinical dementia with lighter brains. This may have had the opposite effect on the findings (i.e. heavier brain weight in elderly suicide cases).

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Vascular events associated with pharmacotherapy

Further to an article on the prevalence of vascular events in association with the treatment of psychotic illness (Thomassen *et al*, 2001) and the subsequent correspondence (Curtin & Blum, 2002), we would like to add our comments to this interesting topic. On our unit we have recently had occasion to observe a patient with haematological abnormalities that we feel were directly associated with treatment with antipsychotic medication. The case described below attests to the potential danger of therapy for schizophrenia and adds to concerns regarding the use of clozapine in particular.

Ms B., a 40-year-old woman, was receiving treatment with antipsychotic medication for recurrent episodes of agitation and psychosis. There had been a relatively poor response to trials of three antipsychotic agents and her side-effect profile was such that there were concerns about developing signs of tardive dyskinesia. A trial of clozapine was commenced and beneficial effects were apparent within 4 weeks.

Three months into treatment there was a deterioration in Ms B.'s physical condition and she was troubled by abdominal pain and continuing dyspepsia. She was evaluated and a series of blood tests were ordered. These were normal except for a very high erythrocyte sedimentation rate (ESR) of 90 mm/l, considerably above the normal for a woman of her age. The extent of the elevation was such that a battery of tests were used by our medical colleagues to establish a cause for this abnormality. Despite extensive medical investigations no abnormality was found. The ESR remained persistently elevated above 85 mm/l.

After 5 months on treatment Ms B. developed prominent visual hallucinatory experiences, which were new developments. As these resembled epileptiform discharges that were distressing for the patient, it was decided to discontinue the clozapine therapy completely. Within 2 weeks her ESR had fallen to 15 mm/l and it has not been found to be outside the normal range since that time.

This case suggests that clozapine can produce changes in ESR, which is a crude marker of coagulation status. The persistent change seen in the ESR in this patient could not be explained by any disease process, and it certainly points to the possibility that the clozapine was implicated in increasing her blood viscosity. As a raised ESR is associated with hypercoagulability states such as those seen in malignancies, this must be a source of concern. We are pursuing our interest in this area further.

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Outcome measurement in mental health: the Italian experience in psychogeriatrics

We would like to comment on the interesting editorial by Holloway (2002) on outcome measurement in mental health, by reporting observations on psychogeriatric services in Italy.

Our country is going through a profound economic crisis, with consequences for health expenditure. In this framework regional governments, who have the duty to coordinate health service programmes, are induced to save money in the more 'frail' areas, such as mental health and geriatric services. One of the reasons for this attitude is the claim that evidence for the usefulness of these services is weak. Yet, at the same time, there are very few attempts to fund programmes devoted to acquiring such evidence. Only a small proportion of research grants, even from central government, are devoted to outcomes research in this area. We argue that the scarcity of health resources combined with devolution of health care from central to local governments support an urgent need for outcomes research implementation in the psychogeriatric field.

At a conservative estimate, $<20\%$ of the procedures adopted in psychogeriatrics are evidence-based and follow accepted guidelines. We agree about 'the difficulty of conducting evaluation of the complex social interventions typically deployed within mental [and, we would add, geriatric] health services' (Holloway, 2002) but, paradoxically, it is in times of scarce resources that it is of most relevance to evaluate whether the allocation of money to psychogeriatric services leads to significantly improved outcomes. Moreover, the 21 regions of Italy are undergoing a process of autonomy. One of the risks of this is that each region will adopt different means of measuring the quality of procedures and outcomes. This is particularly relevant if we consider the fact that evidence-based medicine, which might be a standard reference, covers only a small proportion of interventions.

We do not have programmes similar to the UK Department of Health's 'Mental Health Information Strategy' nor do we collect data to compile a minimum dataset. The majority of the work in psychogeriatrics is done without quality controls and it is not possible to benchmark different services against each other. Furthermore, clinicians are deprived of the possibility of measuring outcomes of their interventions, particularly in areas where the data do not allow a direct transfer of information in everyday clinical practice.

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