Children, neurological soft signs and schizophrenia

In their recent article, Leask et al (2002) reconfirm the presence of neurological soft signs as a significant childhood finding among people who later develop schizophrenia in adulthood. In some earlier work using a similar, bias-proof follow-back design we had identified developmental problems (a pragmatic equivalent of soft signs), weaknesses in speech and language and difficulties in peer relationships as the strongest childhood precursors of adult schizophrenia, indeed more easily relevant than family history of psychosis or demographic characteristics.

As interest is developing in prodromes of psychosis and its early onset, we also have a far better-defined group of children who incorporate all the above parameters and factors. In our child psychiatric clinical practice, we are seeing increasing numbers of children with soft neurological signs and disturbed peer relationships who are diagnosed with Asperger syndrome. In effect, it would appear that even though neurological signs are not a central criterion, they are universally present and in exactly the areas Leask et al identified.

Could it be that these youngsters are indeed the most primary candidates for future schizophrenia? It would logically follow; and then our notions on continuities may need revising and, perhaps more relevantly, a target population may be identified where preventive input could be crucial. I would welcome comments from readers.

A. Ambelas

Authors’ reply: Dr Ambelas raises the important relationship between the premorbid characteristics of individuals who later develop schizophrenic illnesses and the syndrome first described by Hans Asperger as ‘autistic psychopathy’ in childhood (Asperger, 1944). Asperger related his clinical picture to Bleuler’s concept of autism in schizophrenia and wrote that, ‘All but the last mentioned feature (dereistic thinking) can be found in the type of personality disorder to be described here’. But ‘While the schizophrenic patient seems to show progressive loss of contact, the children we are discussing lack contact from the start’. Investigating this association, Tantam (1988) found that 18 (21%) of 86 people with Asperger syndrome later developed some form of psychosis.

The status of Asperger disorder/syndrome (DSM-IV (American Psychiatric Association, 1994) and ICD–10 (World Health Organization, 1992)) within the class of autistic spectrum or pervasive developmental disorders (DSM–IV) has been much debated. These disorders are characterised by delays or deficits in social relatedness, reciprocation, and understanding social interactions. The term pervasive developmental disorders was first introduced in DSM–III (American Psychiatric Association, 1980), with Asperger disorder only separated from other pervasive developmental disorders in DSM–IV. Pervasive developmental disorders not otherwise specified constituted the majority of cases in the DSM–IV field trials. Further subdivisions of pervasive developmental disorders are likely in revisions of DSM resulting from empirical evidence and consensus of opinion. Thus, Ambelas’s target of a ‘primary candidate’ at this stage might be the broader class of pervasive developmental disorders, excluding autism, rather than Asperger syndrome per se.

Cohort studies such as the National Child Development Study (NCDS) cast some light on the issue. The epidemiology is arguably similar, with S+ schizophrenia having a lifetime prevalence of 8 per 10000, and in the NCDS at age 7 the gender split was 20:13 (i.e. 1.5:1). While Ehlers & Gillberg (1993) using their own criteria estimated a minimum prevalence of 3.6 per 1000 children (7–16 years of age) and a male to female ratio of 4:1, using more liberal criteria their prevalence was 7 per 1000, with a gender split of 2.3:1.

Most authors agree with Tantam that the core of Asperger syndrome consists of disabilities in communication, socialisation and non-verbal expression, with conspicuous clumsiness and special interests. Cohort studies suggest that there are indeed deficits in at least some of these areas in children who go on to develop schizophrenia in adulthood. In the NCDS, we found these children more often rated as over-anxious and hostile in their relationships with adults and other children, and this was both more marked and present earlier in boys (Done et al, 1994). At ages 7, 11 and 16, their teachers noted the children were mispronouncing words more often than the rest of the cohort. At 11, there were increased rates of speech difficulties, and at 16 they were poor on English ability. There are therefore difficulties in communication, although it is not clear that these are comparable to the ‘odd, pedantic, stereotypes’ that is described in Asperger syndrome. Interestingly, at each age they were delayed in reading ability, although such deficits are not recorded as characteristic of Asperger syndrome. At age 11, girls but not boys among those who later developed schizophrenia were rated as withdrawn (i.e. distant, cut-off from people, avoiding communication), evidence perhaps of difficulties in non-verbal communication. However, at age 7 the girls in all respects manifested normal social behaviour, suggesting that girls who, in adulthood, develop schizophrenia might display a characteristic developmental trajectory (i.e. a decline in social relatedness and reciprocation between childhood and adolescence).

Perhaps the most interesting parallel is the one to which Ambelas draws attention, between the increase in neurological soft signs that we have observed and the clumsiness and stereotypy of movement that is described in Asperger syndrome — a clue to the neurological basis or bases of the two clinical pictures. At age 7, the children who, in adulthood, developed schizophrenia were more likely to be rated as having difficulties in coordination, and at
age 16 were more likely to be described as clumsy. Delays in learning to stand, walk or speak are linearly related to later risk of schizophrenia (Isomanni et al., 2001) as also was delay in potty-training, a finding that corresponds to an increased incidence of failures of continence observed in the NCDS cohort.

Each of these findings suggests some commonality between features of pervasive developmental disorders (including Asperger syndrome) and those that precede schizophrenia, notwithstanding obvious differences in methods of data collection. There remains the question of time course to which Asperger drew attention – the features of Asperger syndrome are present early, whereas those of schizophrenia show an element of progression. This difference is exemplified by a progressive decrease in language scores preceding the onset of schizophrenia (Fuller et al., 2002).

The key question is why does this constellation of features come together? One finding from the NCDS sample points to the neurological substrate. At age 7, children who, in adulthood, develop schizophrenia were more likely to be rated as ambidextrous for handwriting by their mothers and were less lateralised on a square ticking task at age 11. There is evidence that lateralisation is a major determinant of the acquisition of words as well as of other aspects of cognitive ability (Crown et al., 1998). We propose that development of hemispheric dominance for the components of language is relevant to the similarities of the conditions described by the term Asperger syndrome and those that precede the onset of schizophrenic psychoses, as well as to differences in their time course. Thus, language – the core characteristic of the species – with its context in social interaction and its matrix in the lateralisation to the two hemispheres, is the function that varies between individuals and accounts for the similarities between the early developmental anomalies that were identified by Asperger and those that can now be seen as precursors to psychotic illness.


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Genetics of early-onset depression

We were very interested to read the recent, thought-provoking editorial by Andrews et al. (2002) on the prevention of depression in young people. However, we are concerned that they have understated the important role of genetics in early-onset depression. Contrary to their assertion that the children of parents with depression are likely to be at heightened risk for psychological rather than genetic reasons, available evidence suggests that childhood-onset depression represents a strongly genetic subtype of affective disorder (Neuman et al., 1997; Sullivan et al., 2000). Up to 50% of prepubertal children with depression eventually develop bipolar disorder (Geller et al., 2001) and recurrent, early-onset depression (defined as two or more episodes before age 2.5) is recognised as a malignant form of affective disorder characterised by high genetic loading, frequent recurrence and poor long-term outcome (Zubenko et al., 2001). Furthermore, one recent study suggests that the inheritance of depression in these families is compatible with a single major locus (Maher et al., 2002).

Preliminary findings from our own study of early-onset depression in a university population support the view that early age at onset defines a subgroup at very high genetic risk. Using the Family Interview for Genetics Studies (FIGS, National Institute of Mental Health, 1999), 76% of the subjects seen so far (36 out of 47) report at least one first-degree relative with affective disorder, with 87% (41 out of 47) reporting either a first- or second-degree relative affected. The mean age at onset in this group is 15.6 years (s.d. = 2.6).

Population-based interventions are unlikely to reduce the prevalence of depression in young people as long as we have an incomplete understanding of how genetic and non-genetic risk factors interact to bring about the depressive phenotype. Interventions such as the cognitive therapy programme described by Clarke and colleagues (Clarke et al., 2001) might be cost-effective strategies if they can be targeted to high-risk groups. Unfortunately, we are not yet in a position to reliably identify those individuals at high risk.


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**Authors’ reply:** Smith et al worry that we have underestimated the role of genetics in early-onset depression. They draw our attention to the published evidence for the importance of genetic factors in prepubertal depression, which itself is a marker for adult bipolar disorder, although not necessarily a marker for major depression (the topic of our editorial). From their own data, they report that three-quarters of 47 subjects who had a depressive episode by a mean age of 15 had a first-degree relative with an affective disorder. Strong evidence indeed of familiarity, but not necessarily of a genetic cause. Despite their certainty, many of us have problems with the precise nature of the evidence supporting genetic factors in major depression, in part because of the dimensional nature of depression, and in part because of the extensive comorbidity.

We opined that the heightened risk of depression in young people whose parents had depression was likely to be ‘more psychological than genetic’ and referred the reader to the review by Beardslee et al (1998). We provided evidence that intervention programmes for adolescents can reduce by half the probability of depression in the future. Smith et al argue that universal interventions are unlikely to be effective until we have a complete understanding of how genetic and non-genetic factors interact to bring about the depressive phenotype, and that interventions targeted to high-risk groups should be deferred until we can reliably identify those individuals at high risk.

We strongly oppose this thinking. Most interventions in medicine are introduced before there is a complete understanding of the aetiology of the disorder, and usually before there is precise information as to which individuals will respond. Simply to know that an intervention can produce a reliable and significant benefit is sufficient to warrant implementation. We believe that this is the situation in regard to the prevention of major depression in young people.

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**Depressive symptoms and cognitive decline**

In their recent paper, Paterniti et al (2002) reported that depressive symptoms predict cognitive decline over a 4-year period. This is a well-designed and well-written study that replicates a previous finding from similarly well-designed studies. Negative findings on this question, however, are also common in the literature, including a report from the same French group a few years ago (Dufouil et al, 1996). I would like to point out some relevant issues overlooked by Paterniti et al.

First, I find it unfortunate that the paper cites few negative reports, with no mention in the discussion of the many longitudinal studies that have reported no association between depressive states and subsequent cognitive decline (Dufouil et al, 1996; Prince et al, 1996; Cervilla et al, 2000). It is particularly surprising that Paterniti et al quote the study by Chen et al (1999) as reporting that ‘depressive symptoms are predictive of cognitive decline’, when in fact they found that dementia predicted the onset of depressive symptoms but not the other way round.

Second, it is regrettable that Paterniti et al overlooked the only study to date addressing the very same question but for a considerably longer follow-up period (Cervilla et al, 2000). Longer follow-up periods could help to distinguish between psychopathology shared by depression and dementia (e.g. difficulties with memory and concentration, or apathy), as pointed out by a previous study by some co-authors of Paterniti’s paper (Dufouil et al, 1996). Indeed it could be argued that if depressive symptoms have a real capacity to predict cognitive decline, the latter should be expected to become more apparent as the study’s follow-up period lengthens. This, in fact, has not happened in our cohort (Prince et al, 1996; Cervilla et al, 2000) and I believe this adds potentially unique information to Paterniti et al’s discussion.

Finally, it is also regrettable that Paterniti’s group did not explore the repeatedly reported interaction with gender in considering whether depressive symptoms predict cognitive decline (Prince et al, 1996; Cervilla et al, 2000). Indeed, the latter studies have reported that if an association exists between depressive symptoms and cognitive decline, this seems to be the case in men only (Cervilla et al, 2000), or in men of above-median premorbid IQ (Prince et al, 1996).

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**Preventing suicide**

In his editorial, De Leo (2002) cites important papers of the past 8 years. He does not mention that over 100 years ago the great sociologist, Emile Durkheim (1897), stated that the suicide rate reflected patterns of social relationships within communities and that individual mental disorder had little bearing on this behaviour. His view has never been effectively refuted.

De Leo does, however, observe that ‘socio-economic events’ such as wars and economic fluctuations may ‘provoke effects’ that ‘would be incomparably bigger than any well-targeted anti-suicide initiative’. He recognised that in most Western countries, there is currently a ‘remarkable decline’ in youth suicide, which cannot be attributed to suicide prevention activities. Over the past 50 years, there have been synchronous, international trends in suicide (La Vecchia et al, 1994). All of these events are probably due to sociocultural influences rather than fluctuations in the prevalence of mental disorders, and substantiate Durkheim’s view.

De Leo states that suicidal behaviour attracts little interest among contemporary psychiatrists, as judged by the low number of contributions to suicidology journals. But this would seem to be the wrong yardstick. If Durkheim’s view is accepted, the most profitable approach to the prevention of suicide would be the creation of full employment and supportive environments, and the reduction of family breakdown.
and drug misuse. Such an approach would call for increased attention from sociologists, economists, clergy, educators and governments. In the defence of psychiatrists, in the psychiatric literature there is considerable interest in suicide prevention among people with mental illnesses.

De Leo sees promise for suicide prevention in antidepressants, functional neuroimaging and psychometric testing, but surely this would apply only in the clinical setting. It is important to reveal the alternative to identifying and intervening with people at high risk (which has been described as ineffective and even wasteful), that is, the public health approach, in which efforts are made to reduce the risk of suicide across the community (Rosenman, 1998).


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Author’s reply: Sociocultural factors are of great importance in suicide, and the deliberate manipulation of the sociocultural milieu (social engineering?) would evoke a meaningful change in suicide mortality. However, this concept is theoretical and, like most approaches to suicide prevention among high-risk individuals, lacks rigorous scientific evidence. It is important to point out that while Emile Durkheim’s theories have never been effectively refuted, neither have they been supported by convincing empirical evidence.

My main contention is that the prevention of suicide, like other types of preventable death, requires a multifaceted approach that should incorporate interventions specific to high-risk individuals as well as public health approaches. As far as I am aware, this principle guides all existing national strategies, including the recently launched National Plan in England (September 2002). There is little doubt that strategies exclusively targeting high-risk subjects would produce only minimal reductions in mortality rates. Dr Pridmore maintains that counteracting unemployment and drug misuse, and improving community cohesiveness, would be profitable approaches to population-based suicide-prevention tactics. Once more, although shareable on the basis of common sense, convincing evidence for the effectiveness of these interventions is non-existent. For example, I recently reported in this journal on the impact of a telephone support service on suicide mortality among the elderly (De Leo et al., 2002). The supportive environment provided by that service had a significant impact only among female clients. Elderly men, who suffer from far higher rates of suicide than women, reported very little benefit. Similarly, full employment would surely positively affect suicide attempt rates, but maybe not suicide mortality.

The multi-disciplinary approach to suicide seems to me the conditio sine qua non under which prevention of this human tragedy can be effectively pursued. Given their professional exposure to suicidal individuals, psychiatrists are often in a privileged position to positively interfere with a suicidal process. To do it more consistently and on a larger scale, they should contribute more to suicide research, particularly within multi-disciplinary teams in collaboration with psychologists and sociologists, demographers and anthropologists. Complexity of causes requires complexity of remedies; there are no short cuts.


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I read De Leo’s (2002) editorial on preventing suicide with interest. However, I would like to raise a few concerns. In spite of much development and understanding in both biological and psychological causes for suicide, the prevention of suicide remains an imperfect art. However, the comparison of suicide prevention with that of ischaemic heart disease seems inappropriate. The risk factors for ischaemic heart disease are well known, stable and quantifiable. Ideally, risk factors used for predictive purpose should be stable, whereas in suicide, clearly, most are not (Hawton, 1987). Therefore, when risk factors are not stable it will be difficult to apply the same analogy to suicide prevention.

The risk factors for suicide are different for community- and hospital-based populations. We have made progress in pharmacological interventions in hospital-based populations with lithium in bipolar disorders (Kallner et al., 2000) and clozapine in schizophrenia (Meltzer & Okayli, 1995), which have been shown to reduce suicide rates. However, the risk factors in community-based populations are different and a number of psychosocial risk factors have been reported to be significantly associated with the risk of suicide. We need to understand local perspectives and regional factors that influence suicide rates. There is a need for qualitative studies to examine these issues; the factors thus identified should then be explored in epidemiological studies.


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Author’s reply: While the ability to prevent suicide is far less advanced than the prevention of heart disease, in my editorial the analogy highlighted the need for a multifaceted approach to anti-suicide strategies. I made the point that a single preventive measure would not be effective in reducing suicide mortality, as evidenced through the prevention of other types of death such as ischaemic heart disease. In the case of suicide, for example, the worldwide optimal treatment of depression would bring only a minimal reduction in suicide rates (further details available from the author upon request). None the less, fighting depression is generally perceived as the K constant of suicide prevention in existing national
strategies. This happens despite growing evidence substantiating a much reduced life-span risk for suicide in depression than that reported in earlier investigations (Bostwick & Pankratz, 2000). Given the complexity of its pathways, the prevention of suicide, like the prevention of many types of death, requires a combination of approaches, such as public and medical education, promoting community connectedness, controlling access to means, early identification and intervention, etc.

It is certainly true that risk factors for suicide are unstable and may change over time (De Leo, 2002), but probably more important is the (mostly unexplored) interaction between risk and protective factors. This is the truly crucial issue in suicide prevention (by the way, protective conditions of course counteract also the risk of ischaemic heart disease: the Mediterranean diet and omega-3-fatty acids have already convincingly underlined the role of local differences in mortality rates). And this recalls another important point raised by Dr Ravi Shankar, which refers to the local (cultural/traditional) specificity of suicidal behaviour. In countries such as China, risk factors for suicide are not dissimilar from those of Western countries – what varies is their ranking in terms of importance and expressivity (Phillips et al, 2002). Furthermore, it is well-known that within the same country there may be contiguous areas with largely differing suicide rates and that the same risk factors may operate differently in different social contexts.

To identify the exact components of a multifaceted prevention programme, tailored to local characteristics, greater knowledge of risk and protective factors is needed for both the psychiatric and general populations. Prevention of suicide is currently based on scant evidence. Therefore, I fully agree with Dr Ravi Shankar’s view that more sound research is required. Prevention must be grounded in evidence if it is likely to have an effect on suicide mortality.


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I would like to comment on the editorial by De Leo (2002) which came to the conclusion that little is new in suicide prevention. Since nothing was mentioned about pharmacotherapeutic advances in suicide, I would like to take the opportunity to discuss recent information concerning the role of novel antipsychotics in the reduction of suicidality.

Suicide rates in schizophrenia are about 13 times greater than in the general population, and make a substantial contribution to the overall suicide statistics in the UK. Suicide rates in schizophrenia were unaffected by the advent of conventional neuroleptics. This was not because these drugs are ineffective, rather that they also come with adverse events that put patients at risk for suicide – most particularly akathisia and depression. However, there is now evidence that atypical antipsychotics – most particularly clozapine – may have antisuicidal potential. This was first hinted at by a mirror-image study by Meltzer & Okuyi (1995), which suggested an 86% reduction in suicidality. Subsequently, a large epidemiological study (Walker et al, 1997) including data on completed suicides showed that deaths from suicide in clozapine users occurred at a rate of 39 per 100 000 patient-years compared with 222 per 100 000 patient-years in former users of clozapine. Our own UK clozapine study (Munro et al, 1999) confirmed this result. There are also suggestions from pivotal studies of olanzapine that suicidality is also reduced in users of this drug (Tran et al, 1997).

All these observations have their limitations, which led Novartis, in collaboration with the US Food and Drug Administration (FDA), to embark on a randomised controlled trial of clozapine v. olanzapine in the reduction of suicidality in schizophrenia (the InterSePT study), the results of which have recently been reported (Meltzer et al, 2003). Overall there was a 25% reduction in all key measures for suicidality in favour of clozapine. This has recently led the Psychopharmacology Advisory Committee to the FDA to recommend that this body approves suicidality in schizophrenia (not restricted to treatment resistance) as a new indication for clozapine. It is disappointing that the National Suicide Prevention Strategy for England and Wales has little to say about the role of new treatments in suicide prevention. However, in a recent modelling study of ours (Warner et al, 2003), which also took into account drop-out rates and treatment failure rates, we calculated that one-quarter of the target for suicide reduction in all patients in contact with mental health services could be achieved by the broader use of clozapine in treatment resistance. If clozapine were to be approved for suicidality, 50% of all patients with schizophrenia would be technically eligible. Again, calculating in drop-outs and failures an even more substantial proportion of the national target could be met. Much is made of the rates of thrombocytopenia and agranulocytosis with this drug. However, in comparison with overall reduction in all-cause mortality as well as the reduction in suicidality with treatment with clozapine, such caution is not supported by the epidemiological evidence for the overall advantage of this drug (Walker et al, 1997).

Declaration of interest

R.K. was the UK Principal Investigator for the InterSePT study funded by Novartis.


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Fluoxetine in relapse prevention of PTSD

Martenyi et al (2002) suggest that fluoxetine is effective and well-tolerated in the

CORRESPONDENCE
prevention of relapse of post-traumatic stress disorder (PTSD) for up to 6 months. I think that this statement needs careful consideration.

First, the authors start by randomising patients into a placebo group and a fluoxetine group; the latter is later subdivided into a fluoxetine/placebo group and a fluoxetine fluoxetine group. We see the outcome results of both the groups initially treated with fluoxetine, but those of the placebo/ placebo group are not included in the paper.

Second, the authors dismiss the issue of discontinuation-emergent adverse effects, referring to a study by Rosenbaum et al (1998). That study, also sponsored by Eli Lilly, concluded that fluoxetine had fewer adverse events than other selective serotonin reuptake inhibitors. However, fluoxetine was used up to a maximum dose of 60 mg/day with a mean dose close to 25 mg/day, whereas in the Martenyi et al study, the maximum dose was 80 mg/day and the mean close to 50 mg/day – double that in the Rosenbaum et al study. This is more significant as the results are not analysed on an intention-to-treat basis. Martenyi et al state that there were no significant differences when comparing drop-outs due to adverse events, but if we compare the total number of patients discontinuing the study, the percentages are almost double for those switched to placebo compared with those continued on fluoxetine (33.4% vs. 17.3%).

Third, the authors mention that the reason behind the failure to show significant differences in the improvement of symptoms between the two treatment groups is the result of inconsistent patient self-rating. Could it not simply be that there are no differences?

The study addresses an important area, but the interpretation of the results should have been more rigorous.

**Authors’ reply:** Dr Agell raises concerns regarding the conclusions proposed in our original article (Martenyi et al, 2002a) that the results of our study suggest that fluoxetine is effective and well-tolerated in the prevention of PTSD relapse for up to 6 months. Dr Agell’s concerns that (a) we do not discuss the results of the placebo/ placebo group; (b) we do not adequately address the study results regarding SSRI discontinuation-emergent adverse events; and (c) ‘the authors mention that the reason behind the failure to show significant differences in the improvement of symptoms between the two treatment groups is the result of inconsistent patient self-rating’, We will attempt to address each of these concerns.

First, the results presented in our original article pertain to the relapse-prevention phase of a larger study. Results of the acute treatment phase (including the acute results of the placebo/placebo group) may be found in Martenyi et al (2002b). The primary objective of the relapse-prevention phase of our study and the focus of our original article was to assess the efficacy and tolerability of fluoxetine in the prevention of PTSD relapse. It then follows that the relevant results should come from acute phase fluoxetine responders who were continued on fluoxetine in the relapse-prevention phase or switched to placebo. The efficacy results from the placebo/placebo group would not address our question regarding the efficacy of fluoxetine in the prevention of PTSD relapse and, therefore, the full relapse prevention efficacy results from the placebo/placebo group were not provided. We did, however, provide a breakdown of the reasons for discontinuation in the study for all treatment groups (Martenyi et al, 2002a, Fig. 1). Of the 31 patients in the placebo/placebo group (note that the sample size is small because the original randomisation was 3:1 fluoxetine: placebo), the discontinuation profile was quite similar to that of the fluoxetine/placebo group.

Discontinuation profiles for the fluoxetine/ placebo group vs. the placebo/placebo group, respectively, were: 66.1% vs. 61.3% completed the protocol; 0% vs. 0% discontinued because of adverse events; 16.1% vs. 16.1% discontinued because of clinical relapse; 4.8% vs. 12.9% were lost to follow-up; 3.2% vs. 0% discontinued because of patient decision; 9.7% vs. 6.5% discontinued because of non-compliance; and 0% vs. 3.2% discontinued because of lack of efficacy. These discontinuation data suggest that patients with an initial placebo response face a similar risk of recurrence of symptoms to those who had achieved an adequate pharmacological response and were then switched to placebo.

Second, it is true that approximately twice as many patients discontinued from the fluoxetine/placebo group compared with the fluoxetine placebo group. It is important, however, to note the reasons for discontinuations (Martenyi et al, 2002a, Table 2). The protocol specified that patients meeting pre-defined criteria for clinical relapse should be discontinued, which allowed the investigators to provide follow-up care at their discretion. Only one patient in the fluoxetine/placebo group discontinued because of an adverse event compared with none in the fluoxetine/placebo group, and the primary difference between the two treatment groups was the result of patient discontinuation with regard to reason for patient discontinuation was clinical relapse (5.8% vs. 16.1% for the fluoxetine placebo group), respectively. Accounting for all reasons for discontinuation with the exception of clinical relapse, 8 patients (12%) vs. 11 patients (18%) discontinued early for the fluoxetine placebo groups, respectively (Martenyi et al, 2002a, Table 2). It should also be noted that there were no statistically significant differences in the numbers of patients reporting any single adverse event. The adverse events most commonly reported by patients in the fluoxetine/placebo group were insomnia (15%), anxiety (6%) and headache (6%); those most commonly reported by patients in the placebo group were insomnia (10%), headache (5%) and pain (5%). These data provide further support that the long half-life of fluoxetine and its active metabolite, norfluoxetine, provide benefit with regard to the minimisation of the risk of discontinuation-emergent signs and symptoms.

Third, statistically significant differences were detected between treatment groups for the a priori defined primary analysis (time to relapse, P=0.027; Martenyi et al, 2002a, Fig. 2). In addition, using repeated-measures analysis of variance (Martenyi et al, 2002a, Fig. 3), we can see that those patients in the fluoxetine/ fluoxetine group continued to improve over time, with a statistically significant difference between groups occurring from week 28 to the study end-point (week 36), based on our primary efficacy measure, and

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Declaration of interest

This work was sponsored by Eli Lilly and Company. E.B.B., A.P. and C.M.M. are employees of Eli Lilly and Company.


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The Edinburgh Postnatal Depression Scale

The Edinburgh Postnatal Depression Scale (EPDS; Cox et al, 1987) is one of the most widely used self-report instruments to screen for depression in the post-partum and antenatal periods. As with all instruments, it is important for validity that the wording of a measure remains faithful to that described in the original validation study. While checking our EPDS against the original, we noticed a difference in the wording of one of the items. We believe that the EPDS used elsewhere may also contain the same anomaly. Item 4 on the EPDS provided in the paper by Cox et al (1987) is phrased: ‘I have been anxious or worried for no good reason’. However, the version reproduced in Cox & Holden’s book (1994), which is also likely to be the source from which many centres copy their EPDS, is different: ‘I have felt worried and anxious for no very good reason’ (differences from the journal version italicised for clarity). In addition, the order of anxious and worried has been reversed. Personal communication with Professor Cox has confirmed that the wording in the journal paper is correct. That these mistakes have occurred in a book about the ‘use and misuse’ of the scale is somewhat ironic. Indeed, this makes us a little anxious and worried!

What effect might these differences have on the self-reports of women or men? It is hard to know – hopefully, none. It would not, however, be surprising if these alterations lead to differential responding and scores.

Over the many years of our involvement in this field, we have also noted usage where the EPDS preamble was omitted or altered, provenance (e.g. authors and date) was not acknowledged, and incorrect cut-off scores were inadvertently applied. We should all, therefore, be more rigorous in our use of this scale.


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Authors’ reply: We are indebted to our distinguished colleagues in Australia for pointing out this ambiguity. We will be indicating in our definitive EPDS book, soon to be published by Gaskell (Cox & Holden, 2003), that the scale from the first validation study as published in 1987 contains the correct and original wording.

The differences between ‘being’ and ‘feeling’, ‘anxious or worried’ and ‘worried and anxious’ are not only semantic. Perhaps committed EPDS advocates, like your correspondents, will test their hypothesis that these word changes may affect the total EPDS score. We doubt it, but a local grant-giving body might support an ambitious master’s student.

The EPDS is not, of course, a precise measuring-rod of feelings, but its total score has been shown to provide a remarkably accurate indication of the likelihood of clinical depression in many cultures and countries.

Our new book, Perinatal Mental Health: A Guide to the Edinburgh Postnatal Depression Scale (EPDS), is our definitive and final attempt to ensure that the EPDS is used as frequently as appropriate; and misused – never!

Declaration of interest

J.C. and J.H. developed the EPDS and are authors of Cox & Holden (2003), sales of which may generate personal royalty payments.


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One hundred years ago

Royal Asylum of Montrose (Annual Report for 1900)

Suicidal tendencies were marked in a large proportion of the patients admitted, and the inquiries of Sir John Sibbald now published for the first time show that Forfarshire and the neighbouring county of Kincardine have a larger proportion of suicides compared with the population than the rest of Scotland. The same authority states that ‘the counties of the east coast of Scotland all show higher suicidal rates than the western counties. It is curious that the city of Dundee shows a lower rate than the rest of Forfarshire. It is so far in favour of the view of those who say that Celticism and Catholicism prevent suicide, for
Dundee contains the largest proportion of Irish Catholics of any part of Forfarshire. Certain parishes in the two counties named send a very high proportion of suicidal patients to the asylum and suicides are specially frequent in these parishes. ‘A possible explanation of this,’ says Dr Havelock, ‘seems to be that of inherited predisposition for the suicidal tendency is strongly hereditary in most cases and is prone to increase unduly in districts where the population is stagnant and stationary. The whole subject is beset with problems of extreme interest and difficulty.’

**Corrigendum**

Mood stabilisers plus risperidone or placebo in the treatment of acute mania. International, double-blind, randomised controlled trial. *BJP*, 182, 141–147. The authors’ affiliations should read: LAKSHMI N. YATHAM, MB, FRCP, MRCPsych, University of British Columbia, Vancouver, British Columbia, Canada; FRED GROSSMAN, DO, Janssen Pharmaceutica, New Jersey, USA; ILSE AUGUSTYNS, PhD, Janssen Pharmaceutica, Bersee, Belgium; EDUARD VIETA, MD, PhD, Bipolar Disorders Programme, Hospital Clinic, University of Barcelona, IDIBAPS, Barcelona, Spain; ARUN RAVINDRAN, MD, Royal Ottawa Hospital, University of Ottawa, Canada.

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Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey.
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