Smaller trials for better evidence

The interesting debate between Parker and Anderson & Haddad (2003) suggests more fundamental reasons to question prevailing research paradigms and designs in respect of the efficacy of and indications for psychotropic medicines. That the clinical trial industry reveals even marginal drug effects may be seen as surprising given the virtual absence of any basis for a taxonomy of mental disorders, other than the syndromal classifications used in psychiatric practice. There is little evidence that the major syndromes align with any readily defined pathophysiological variance. Group heterogeneity in trial work, as the debaters remark, will therefore attenuate the evidence for substantial drug treatment effects, sometimes to vanishing point. Meta-analysis of such data may not be much more revealing, compounding the influence of variable sampling in individual trials and publication bias.

These side-effects of the randomised controlled trial ethos are not greatly mitigated in the field of organic mental disorders. At huge expense, multicentre trials of cholinesterase inhibitors in patients classified as probably having Alzheimer’s disease have shown only very modest (and to many observers still unconvincing) effects on cognitive and neuropsychiatric outcomes (e.g. Lanctôt et al., 2003). This is because these conditions, pace distinguished efforts at nosological definition in life, are also heterogeneous. This variability, already evident from detailed clinical and neuropsychological assessment, is further revealed by functional and structural analysis of the brain. It is these data which might best inform sampling for therapeutic trials. Studies on a smaller scale, therefore, targeting the more readily therapeutic trials. Studies on a smaller scale, therefore, targeting the more readily

nature and the modalities of efficacy using cholinesterase inhibitors. In the same way, studies of smaller groups of patients receiving treatment for depression may reveal correlations between clinical features and treatment responses that are more likely to guide the selection of therapy for individual patients (Mayberg, 2003).

Large randomised controlled trials, by submerging variation in the interest of marginal statistical significance, seem to offer limited hope of significantly improving the evidence that guides clinical practice. Studies of cognitive and pharmacological interventions might best be carried out with smaller patient groups for whom there has been detailed assessment of relevant pathophysiological and cognitive variance, as well as the manifest clinical symptoms.

Declararion of interest

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Pendulum management in secure services

Tilt (2003) defends himself clearly against the criticisms of Drs Exworthy & Gunn (2003). However, he does not emphasise the extent to which they have misrepresented aspects of the Tilt Report (Tilt et al., 2000). Specifically, Exworthy & Gunn state, following their quote from the Report concerning the relationship between security and therapy, ‘one should go further because in high secure hospitals therapy in its widest sense is an integral part of security’. This virtually paraphrases the Report itself: ‘Security is the responsibility of all personnel in a high security hospital and . . . good security facilitates good therapy and vice versa’ (paragraph 8.2).

There also appears to be a marked absence from this debate of both historical and organisational perspectives. Rapoport (1960) suggested, in considering the institutional dynamics of therapeutic institutions, that ‘disturbances were partly a function of cycles of abdication of authority, in the name of permissiveness, followed by authoritarianism to restore order’. The consequences of the report on the Ashworth Hospital inquiry (Blom-Cooper et al, 1992) (Ashworth at that time being an abusive, authoritarian institution) were clearly thought by Fallon et al (1999) to relate to a breakdown of security (permissiveness), leading to the Tilt Report (which has been perceived by many in secure services as authoritarian).

Perhaps attempting to understand this cycle more, and how it may relate to the complex (and potentially contradictory) tasks facing secure psychiatric services, might reduce the likelihood of yet more scandals, inquiries and reports in the future. Scott (1975) suggested that ‘detaining custodial institutions have two aims, one therapeutic, the other custodial. These can and should be complementary, but there is a tendency for these functions to polarise out and eventually split like a living cell into two separate institutions’. The debate between Exworthy & Gunn and Tilt illustrates the recurring nature of this phenomenon.
Sertraline and exposure therapy in social phobia

I read with interest the article by Haug et al (2003), but was puzzled by the conclusion they drew from their data.

After a 24-week study comparing sertraline, sertraline plus exposure, exposure plus placebo, and placebo in patients with social anxiety disorder (Blomhoff et al, 2001), patients were followed up at week 52. In the summary the authors conclude that ‘Exposure therapy alone yielded a more effective improvement than when given in combination with sertraline’. The opposite is the case: it takes 1 year for the exposure patients to reach the level of improvement that the sertraline and the combination patients have already reached after half a year. Perhaps the patients treated with exposure only showed further improvement during follow-up, whereas exposure therapy combined with sertraline and sertraline alone showed a tendency towards deterioration after the completion of treatment’. This seems to be a misleading interpretation of their data.

Haug and colleagues did not mention the primary efficacy measures of their study in their paper. Reading the original paper by Blomhoff et al, I find that the primary efficacy measures were numbers of responders or partial responders on the Clinical Global Impression – Social Phobia (CGI–SP) and the Social Phobia Scale (SPS). In the first study, treatment with sertraline was superior to placebo, but exposure was not. For example, 45.5% of the patients treated with sertraline plus exposure were responders compared with 33.0% of the patients treated with exposure plus placebo. I wonder why it was not mentioned in the second paper whether the three active groups differed from placebo and from each other on the primary efficacy measures.

Instead, Haug et al report only relative changes in mean scores without adjusting for the large absolute differences at termination of the acute study (week 24). After 52 weeks, exposure patients only caught up to the already better scores of the sertraline groups. From both papers, I calculated the following total mean changes for weeks 0–52 by adding the mean changes for weeks 0 to 24 and the ones for weeks 24 to 52 and found: 1.68 for placebo, 2.02 for sertraline plus exposure, 1.92 for sertraline, and 1.88 for exposure plus placebo on the CGI–SP overall severity. For the SPS, I found the following mean changes: 12.09 for placebo, 15.56 for sertraline plus exposure, 14.12 for sertraline, and 15.91 for exposure plus placebo. These scores may change a little bit after correction for participants who withdrew from the trial. I doubt that any of these scores differs significantly from each other or from placebo.

By no means is it true that ‘Exposure therapy given alone is more effective in the long term than when given in combination with sertraline’. The opposite is the case: it takes 1 year for the exposure patients to reach the level of improvement that the sertraline and the combination patients have already reached after half a year. Perhaps the patients treated with exposure only showed further improvement during follow-up, whereas exposure therapy combined with sertraline and sertraline alone showed a tendency towards deterioration after the completion of treatment. This seems to be a misleading interpretation of their data.

Author’s reply: The primary efficacy measures from our paper about treatment effect at week 24 (Blomhoff et al, 2001) are reported in the method section of the paper about the follow-up study (Haug et al, 2003). In the pairwise comparisons, combined sertraline and exposure and sertraline alone were significantly superior to placebo, while a non-significant trend towards increased efficacy of exposure alone compared with placebo was reported.

The four study groups had a significant reduction in scores on social phobia scales from baseline to follow-up. Furthermore, there was no significant difference in scores on primary efficacy measures between the active treatment groups in any of the time-point analyses between week 0 and week 24. In the follow-up analyses we were therefore mainly interested in the changes after cessation of treatment. For the exposure group and the placebo group there was a further improvement in scores on social phobia from week 24 to week 52 and the changes on several of the subscales were highly significant. On SF–36, which demonstrates changes in a more global functioning, there was a significant improvement for the exposure alone and the placebo groups, while there was a significant deterioration in both the sertraline-treated groups. Changes in scores on other social phobia scales for the sertraline-treated groups were non-significant, but there was a tendency towards deterioration (Tables 1 and 2, pp. 314–315). We agree that the changes in sertraline-treated groups during the follow-up period were marginal. However,
contrasting these minimal changes with the significant improvement in the exposure-treated group, we find it appropriate to conclude that exposure therapy given alone seems to be more beneficial in the long term. Longer follow-up could have added valuable information to this issue. In all groups about 20% of the patients were treated with sertraline during the follow-up period so this could not explain the differences in scores between the groups at week 52.

Declaration of interest
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Premature conclusions about depression prevention programmes
In my opinion, the meta-analysis by Jane-Llopis et al (2003) suffers from some methodological flaws that misguided the authors to draw premature conclusions on predictors of prevention in depression prevention programmes.

First, many of the selected studies did not target the prevention of depression but examined therapeutic or preventive strategies for other primary disorders and used depression scores as secondary outcome measures. For example, Bisson et al (1997) studied the efficacy of psychological debriefing on the development of post-traumatic stress disorder (PTSD) in victims of acute burn trauma. They showed that psychological debriefing may even worsen the long-term course of burn victims. But while psychological debriefing may have been mistakenly considered helpful for preventing PTSD in the past, no reasonable therapist or researcher has ever claimed that massive emotional confrontation would represent a promising strategy for depression or depression prevention.

Second, the coding of respective methods looks rather inconsistent, and I wonder how the authors were able to reach such a high interrater reliability across codes. For example, the psychological debriefing method used by Bisson et al (1997) was coded as ‘behavioural, cognitive and educational’ (p. 389), while the code ‘cognitive’ was missing for Seligman et al’s (1999) intervention based on cognitive therapy. Similarly, four research groups using similar variants of the Coping with Depression Course by Lewinsohn et al (1984) were coded differently (e.g. ‘cognitive and competence’, ‘behavioural, cognitive, educational and social support’, ‘cognitive’, and ‘behavioural, cognitive, competence and educational’ (pp. 386–391)). Finally, the coding category ‘behavioural methods’ incorporates very heterogeneous strategies. For example, behavioural strategies found to be helpful in cognitive–behavioural therapy for depression focus on increasing pleasant activities and social skills training (Lewinsohn et al, 1984), whereas the delivery of peer support telephone dyads by lay persons, as used in the studies by Heller et al (1991), may be regarded as a very specific behavioural strategy which has so far not been recommended as a helpful intervention by the research community. In Jane-Llopis et al’s meta-analysis, respective interventions from the studies by Heller et al (1991) had negative effect sizes and therefore may have substantially accounted for the missing or even negative effect of the ‘behavioural’ component of preventive measures.


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Homicide data
I am writing to query the homicide statistics quoted by Dr Salib (2003). The figures he quotes for total annual homicides suggest a fall in homicide between 1979 and 2001. The source for his figures is quoted as the Office for National Statistics (ONS).

Homicide statistics are easily available through the website of the ONS and from various other sources, including Home Office statistical bulletins and the House of Commons Library. For example, Richards (1999) describes homicide trends between 1945 and 1997, demonstrating the dramatic rise in rates of offences initially recorded as homicide seen over that time from around 300 or 400 a year in the 1950s to more than 700 a year in the late 1990s. The recent Home Office Statistical Bulletin (Simmons & Dodd, 2003) shows a continuing rise in this trend with 1048 deaths initially attributed to homicide in 2002/2003, although these figures are based on date of notification and thus can include deaths that actually took place in earlier years.

Dr Salib’s paper appears to use data on death registrations from the ONS where there has been a conviction for murder or for manslaughter. However, the ONS assigns a temporary ICD–9 code for cause of death for deaths where death was violent, unnatural or suspicious or pending the outcome of inquests and legal proceedings, which are of course often prolonged. The ONS site itself states that it is difficult to present accurate statistics on number of homicides using death registrations, which is what Dr Salib has seemingly attempted to do.

As psychiatry is faced with a Government currently determined to medicalise as far as possible the growing problem of violence in our society, it is essential that psychiatric journals present statistics on this subject in a meaningful fashion. Dr Salib’s paper, although not specifically about trends in homicide over time, presents misleading data on this subject, which are neither helpful nor informative to the wider debate on violence in society.


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Author’s reply: Dr Rowlands raises an important question, triggered by homicide data in my recent paper on the effect of
Mental health and psychiatric research in Brazil

Saxena et al (2003) have shown the under-representation of low and middle-income countries on the editorial boards of ten leading psychiatric journals, based on a World Health Organization report. Horton (2003), Editor of The Lancet, has presented some evidence of publication bias against diseases of poverty studied in developing countries. Wilkinson (2003), formerly Editor of the British Journal of Psychiatry, has suggested that the absence of representation on the Editorial Board does not necessarily bias an editor’s decision-making. However, Catapano & Castle (2003) have shown that research papers from developing countries represent a very small proportion of the publications (<2%) in important psychiatric journals, which has remained the same for 10 years. We argue that Brazil, a middle-income country, is progressively improving its scientific production and reaching the standards of high-income countries.

We have assessed the mental health scientific production of Brazilian postgraduate programmes between 1998 and 2002 using a Brazilian Ministry of Education database. The eight doctoral programmes in psychiatry and psychobiology, all in state institutions, have awarded 183 PhDs and this has resulted in publication of 1664 scientific articles in journals; 605 of these in journals indexed by the Institute of Scientific Information (ISI). The production of ISI-indexed papers doubled in this 5-year-period. The mean impact factor of the ISI-indexed journals where articles were published was 1.82 (range 0.01–29.51); 64% were published in journals with an impact factor >1. The number of Brazilian articles in psychiatry and psychology (442) published between 1998 and 2003 corresponds to 10% of France’s (4129) production, but the impact factors are very similar: 4.48 and 4.83, respectively (data from ISI, reported on http://in-cities.com/countries).

Although health problems in developing countries account for over 90% of the world’s potential life-years lost, only 5% of global health research funds are devoted to these problems (Mari et al, 1997). The investment channelled to postgraduate and human resource educational programmes in Brazil has assured the country a modest but continuous contribution to the worldwide production of knowledge in health. It is expected that the quality of the scientific production of countries such as Brazil will influence editors’ decision-making and overcome eventual ‘institutional racism’ (Horton, 2003).

Declaration of interest

J.J.M. and E.C.M. are Editors and R.A.B. is an Associate Editor of Revista Brasileira de Psiquiatria.


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One hundred years ago

The attitude of the legal profession towards the medical profession

In a letter published in The Lancet of Feb. 27th, p. 611, Dr. E. Magennis, writing of the conduct of barristers in Ireland, deplored the disappearance of that courteous treatment of the medical witness which once characterised cross-examination but which at the same time did not prevent the most vigorous investigation of the facts, and he drew attention to the unwarranted impertinence, frequently amounting to positive insult, which appears to arise from the assumption that the medical witness must not only be prejudiced but ready to give perjured evidence on behalf of the party employing him. There are many who agree with Dr. Magennis, and who will add that the discourteous treatment of the medical witness is not altogether
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C. Kuehner
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