Correspondence
EDITED BY KIRIAKOS XENITIDIS and KHALIDA ISMAIL

Contents  • Combating editorial racism  • Prevention of psychosis  • Guided self-change for bulimia  • What is a traumatic event?  • Potentially preventable suicide  • Transcranial direct current stimulation in developing countries  • Drug combinations for rapid tranquillisation  • Limitations of rapid tranquillisation trial  • Who pays the piper?  • Begetting drunkards

Combating editorial racism

I was delighted to see the editorial on ‘Combating editorial racism in psychiatric publications’ (Tyrer, 2005). You liken this to those from the world’s poorest countries always playing uphill and into a howling gale. How about taking away their football boots!

I applaud Professor Tyrer for addressing this problem and want to reciprocate with this contribution. Black and minority ethnic groups within the wealthy 10% of the world’s population have a responsibility to engage and link with those in the remaining 90% to ensure that knowledge and research are disseminated as widely as possible and, importantly, to ensure that this has an effect on those receiving psychiatric services. An example of this two-way synergy is work my (White) colleague and I have completed on the emotional effects of the troubles in Northern Ireland (Kapur & Campbell, 2004). In applying a psychoanalytic model to the conflict we have attempted to highlight the emotional traumas suffered in everyday life. This suffering is universal. Archbishop Tutu kindly agreed to write the foreword to our book and here we have an example of a synergy that has facilitated ideas from psychoanalysis, which has struggled with cultural diversity (Littlewood, 1988; Littlewood & Lipsedge, 1997), being shared and validated by a nation, South Africa, which like Northern Ireland has suffered its own experiences of political oppression with the consequent infliction of trauma.

The problem with playing the ‘race card’, as you note, is that it can be seen as an excuse for work that may not reach the standards of a particular journal. Or, vice versa, the work might be of too high a standard and thus ‘show up’ lesser publications of the other 10%, mainly White contributors! The problem with not protesting is that there is an institutional collusion with racism which opposes merit and excellence and only leaves feelings of injustice which we suggest (Kapur & Campbell, 2004) is a major cause of terrorism. Maybe Archbishop Tutu agreed to write the foreword because he knew, half-way across the world, that we had one particular experience in common: playing uphill against a howling gale in our football socks.

But there is hope; as long as people continue to speak out we can make good use of recent research findings which suggest that prejudice is not ‘hard wired’ in the amygdala (Wheeler & Fiske, 2005). If you change the context in which people are seen, prejudice can lessen. For example, contributors from Black and minority ethnic groups are part of a professional community first, rather than part of a particular race. I will now prepare my next paper for submission to the Journal (it has been 17 years since my last one; Kapur et al, 1988).


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Prevention of psychosis

The Editor is of course right to highlight the potential importance of the work by Morrison and colleagues (2004) and how this must be weighed against possible methodological flaws. While the authors acknowledge most of these, there are some aspects of the study which deserve further clarification. For example, the exclusion of two cases after randomisation to the cognitive therapy group owing to the fact that they had apparently been psychotic at inception could be justified. It is stated that ‘all other participants were questioned about this possibility’ – however, can we be sure that psychosis at that earliest stage was rooted out equally assiduously in both groups, cognitive therapy and ‘control/ monitoring’?

Another matter discussed is the randomisation procedure, which resulted in unequal group allocation. The authors state boldly that this was due to chance, and the methodology for randomisation (stratified for gender) as described seems to be sound. However, I am not sure I would be happy to accept a reprieve from a gloomy fate on the basis of ‘tails’ on the toss of a coin, in the knowledge that it had previously yielded ‘heads’ 37 times out of the last 60. Let’s just imagine that somehow a gremlin interfered with the randomisation process so that the patients who seemed less likely to decompenstate, the majority, were steered into the therapy group. This would produce the observed pattern. This gremlin need not even be credited with much clinical foresight since prediction of onset of psychosis in a very high-risk group of 20–21-year-olds is quite simple given one of the most robust findings in the epidemiology of schizophrenia, namely the later age of onset in females. So, as long as more females find their way into the intervention group, a better short-term outcome is virtually assured. Morrison et al ended up with 40% females in the cognitive therapy group v. 17% in the control group. It may all be due to chance and adjustable in the logistic regression analysis, but given the impossibility of delivering a psychological intervention blindly, the integrity of the randomisation procedure must be beyond question.


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Author's reply: Professor David highlights important points in relation to the trial of cognitive therapy for the prevention of psychosis. He asks for clarification regarding the exclusion of two participants for having been psychotic at inception, but only reporting this on second contact with an assessor. This was certainly what happened, and following this the research assistants were instructed to ask all participants about this. This was not in the original protocol for the study, as such an event was unexpected (although, with hindsight, maybe it should not have been). It did seem reasonable to exclude these participants, especially given that the study is the first of its kind (clearly future studies should address this issue in the protocol).

He also raises the issue of randomisation. The procedure for randomisation is very clearly outlined within the original paper and the difference in gender rates was due to chance. It is true that this method resulted in more of the treatment group being female, which is an indicator of better outcome for such a population; however, the method also resulted in the treatment group having a higher proportion of people who were unemployed and a significantly higher level of baseline positive symptoms, both of which would predict poorer outcomes for the treatment group. It is also worth noting that gender was utilised as a covariate in all analyses regarding transition that were reported.

Professor David has identified two important issues that can only be clarified by replication of the results of this study with a more rigorous protocol and an alternative method of randomisation; we would agree that such research is required to determine whether the preventive effects of cognitive therapy with people at ultra-high risk of developing psychosis are generalisable and robust.

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Guided self-change for bulimia

Bower & Gilbody (2005) raised questions about stepped care in psychological therapies to which I would like to provide some answers. Our treatment evaluation study of guided self-change for bulimia nervosa incorporated a self-care manual and investigated acceptability, drop-out rate, ‘extra treatment’ and longer-term outcome (Thiels et al., 1998a, 2003). We did not dare to offer the manual only as a first step to Germans spelt by a healthcare system with excellent provision of long-term psychotherapy. Instead we compared 8 fortnightly sessions plus a self-care manual (guided self-change) with 16 weekly individual sessions of cognitive-behavioural therapy (CBT). There were no significant differences between therapies regarding drop-out rate, general satisfaction with treatment and views regarding the usefulness of the therapies.

A journalist who wanted to write about our study in the weekly newspaper Die Zeit met with strong resistance. Although we published the 6-month follow-up results in the American Journal of Psychiatry (Thiels et al., 1999b, quoted by Bower & Gilbody, 2005) the head of the section of Die Zeit did not believe that such low therapist input could work. Some psychiatrists in the hospital where the journalist’s husband worked feared for their jobs.

Bower & Gilbody (2005) state that ‘research on the patient acceptability assumption would need to access the views of a number of different populations…’. We invited family physicians, psychiatrists, gynaecologists and various counselling services to refer those who complained of symptoms suggestive of bulimia nervosa (the clinical picture of which was briefly described) (Thiels et al., 1999b). An article about the service in a local newspaper led to several self-referrals. Unfortunately, we did not compare acceptability according to the source of referral and thus can only report the overall acceptability.

Follow-up by personal interview using expert and self-rated instruments was carried out a mean of 54.2 months (s.d. = 5.8) after the end of therapy. Significant improvements were achieved or maintained in both groups on the main outcome measures. These included eating disorder symptoms from expert ratings (Eating Disorder Examination sub-scores: over-eating, vomiting, dietary restraint, shape and weight concern), self-report (Bulimic Investigatory Test, Edinburgh) and a global five-point severity scale. Improvement was also seen on the subsidiary outcome variables Beck Depression Inventory, Self-Concept Questionnaire and knowledge of nutrition, weight and shape. During the week before follow-up 66.7% of the guided self-change group and 61.5% of the CBT group had not binged, vomited or misused laxatives.

A considerable proportion in both groups had extensive further psychological treatment, mainly for their eating disorder. The majority of these had not done well during initial treatment. An analysis of covariance showed a significant interaction between treatment group and additional treatment between the 6-month and the 4-year follow-up. Cross-tabulation showed that this difference was due to the fact that three of the five in the CBT group with additional treatment between the two follow-up assessments improved more than the eight in the CBT group without additional therapy ($\chi^2 = 6.24$, d.f. = 1; $P < 0.035$). However, the six in the guided self-change group who received additional treatment made as little progress as the seven who did not ($\chi^2 = 0.26$, d.f. = 1; $P > 1.00$). Four out of 12 individuals received additional treatment from their study therapist, the other eight received additional treatment elsewhere. The question is whether those who were allocated to guided self-change would not have recovered in the course of 4 years with any therapy or whether they might have done better with more therapist contact from the beginning of treatment.


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What is a traumatic event?

Olff et al. (2005) sampled farmers during a foot and mouth epidemic, concluding that half whose livestock were culled had 'post-traumatic stress at levels requiring professional help', and 'the high prevalence of post-traumatic stress symptoms is an understimation of the real levels of psychopathology' (p. 166). What was the 'traumatic event' implicated in these
'post-traumatic' reactions? According to DSM–IV–TR criteria for post-traumatic stress disorder (PTSD) (American Psychiatric Association, 2001), a traumatic event requires that 'the person experienced, witnessed, or was confronted with an event or events that involved actual or threatened death or serious injury, or a threat to the physical integrity of self or others' (further requiring that 'others must be persons, not animals') and that 'the person's response involved intense fear, helplessness, or horror' (p. 467). We seriously question livestock loss as a traumatic event.

Loosening criteria for a traumatic event represents a progressive 'conceptual bracket creep' in defining trauma (McNally, 2003). Will the next study examine PTSD in children 'traumatised' by their pet hamster’s death, or from watching Bambi die in the famous Disney movie? What about being exposed to offensive remarks by others (Avina & O'Donohue, 2002)? With the current trajectory all negative experiences will be synonymous with traumatic events, trivialising the experiences of real trauma victims. We ask where will researchers finally draw the line in what is considered traumatic? Continued disregard for the criteria will lead to anyone being considered trauma-exposed and eligible for a PTSD diagnosis. With healthcare resource limitations, truly trauma-exposed and symptomatic patients could consequently be denied care (at a minimum subjected to extensive waiting lists), and our courts would be crippled with unnecessary PTSD litigation.

Olff et al (2005) claim that 'Although the foot and mouth crisis is not a traumatic event in the usual sense, the consequences do resemble features of PTSD' (p. 166). This statement minimises (without justifying) the authors’ disregard for trauma criteria, and poses a circular argument in contending that the presence of PTSD symptoms confirms trauma exposure. However, trauma exposure must be distinguished from PTSD, since minor stressors (e.g., taking a nightshift job) can result in symptoms (e.g., difficulties in sleeping, problems concentrating) that are aetologically distinct from PTSD.


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Authors’ reply: Post-traumatic stress disorder is unusual among DSM disorders in that the diagnostic criteria specify an aetiological event: exposure to a traumatic stressor. In their letter Elhai et al cite examples that do not meet the stressor criterion, the symptom criteria for PTSD, or the criteria of distress or impairment. The DSM–IV symptoms (re-experiencing, avoidance/numbing and hyperarousal) are defined in terms of their connection with a traumatic event. The ‘conceptual bracket creep’ (McNally, 2003) refers to the broadening of the stressor criterion in DSM–IV, especially to the inclusion of ‘second-hand exposure’, such as learning about the unexpected death of a close friend/relative or watching atrocities on television (see Rosenbaum, 2004). This seems to increase the eligible events by about 20% (Breslau & Kessler, 2001). However, more important is the question addressed in the DSM–IV guidebook ‘whether or not to include reactions to the numerous stressors that are upsetting, but not life threatening (Frances et al, 1995: p. 259)’ to eliminate the stressor criterion altogether. The fear that more inclusive definitions will vastly increase the frequency of the diagnosis seems to be unrealistic. More minor stressors simply will not result in the other diagnostic criteria for PTSD.

McNally (2003) makes an important point in stating that with the inclusion of such diverse events it will be difficult to identify common psychobiological mechanisms underlying symptomatic expression. In our opinion, to develop PTSD the stressor – often associated with severe sadness – should be intense enough to evoke a psychobiological dysregulation of the fear system, which results in the event being re-experienced, avoided and leading to a state of hyperarousal where the person feels that danger could strike again at any moment. This psychobiological stress response is dependent on subjective appraisal of the event and not on objective criteria of stressor severity (Olff et al, 2005). This would suggest that ‘second-hand exposure’, non-typical traumatic stressors or even life events might in some instances evoke an intense psychobiological dysregulation leading to ‘PTSD’ symptoms. Apparently, this was the case for the farmers who witnessed (saw, heard, smelled) all their animals being destroyed, an event that was beyond their control and is certainly ‘outside the range of their normal experience’.

Mental healthcare should be available to those with significant mental health problems, even if these are considered sub-threshold for PTSD. By conducting a large epidemiological survey in The Netherlands we hope to determine what kind of stressors (including life events) evoke what kind of 'post-traumatic' symptoms, as well as the implications for mental healthcare.


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Potentially preventable suicide We read the short report by Bennewith et al (2005) with interest. The authors attempted to address one of the objectives of the National Suicide Prevention Strategy for England, restricting access to means of suicide (Department of Health, 2002).

The authors found 10 cases (6%) of ‘potentially preventable’ suicide by hanging in controlled environments such as hospitals and prisons, among 162 cases of a
randomly selected sample of suicide by hanging from a wide geographical area in England.

We would like to make the following comments. The report made no reference to the proportion of older victims in the randomly selected sample. The mean age given in the report (41 years) is almost the same as that for all people over the age of 16 years in England and Wales who hanged themselves in the same year as the study (2001) and over the past 23 years. These cases include, on average, 16% over the age of 64 years. This means that the study sample of 162 contained at least 25 victims over the age of 64, a sizeable older element that was not referred to in the report.

This is important and ought to have been clarified particularly in relation to the deaths that occurred in hospital when the victims were found seated (4.7%), kneeling (7.4%), lying (8.7%) or partially suspended (3.4%) and to individuals who were found alive (4.3%). However, this does not apply to hanging in prison where victims had an estimated mean age of 28 years (Shaw et al., 2004).

We calculate the expected annual rate of ‘potentially preventable suicides by hanging’ within institutions (controlled environment) in England and Wales to be about 110 cases of a total of 1300 expected annual suicides by hanging: 86 in prison (Shaw et al., 2004) and 24 (about a third of 71 hanging incidents by psychiatric inpatients) in hospital (Department of Health, 1999). ‘Potentially preventable suicide by hanging’ in controlled environments involving prisoners represents 5% of all suicide by hanging in England and Wales and 2% in the case of psychiatric in-patients. This is remarkably similar to the 6% in the report of Bennewith et al.

It would be of real interest, and certainly of practical value, if future studies specifically investigated suicide by hanging within controlled environments such as hospitals and prisons using an appropriately selected sample over a period of time (e.g. 220 incidents expected over 2 years in England and Wales, based on current figures). This would provide a study with acceptable power and some inferential value compared with the modest 10 cases reported by Bennewith et al.

Authors’ reply: The aim of our research was to undertake a detailed assessment of a sample of all (community and institutional) suicides by hanging in a defined geographical area over a 6-month period, focusing on potentially preventable aspects of these deaths. The context for the research was the rise in suicides by hanging in England and Wales (Gunnell et al., 2005) and the National Suicide Prevention Strategy for England (Department of Health, 2002). Of note, rates of suicide by hanging have not increased among men or women aged 65 years and over (Gunnell et al., 2005). Although generally Office for National Statistics figures for England and Wales from the 1970s onwards show that rates per 100,000 for deaths by hanging were higher in those aged 65 and over compared with rates in other age groups, this is not the case from 2000 onward when rates for death by hanging increased in the 15- to 44-year age group and decreased among those aged 65 and over (Gunnell et al., 2005).

The Editor decided that our paper should be resubmitted as a short report. The limited space did not enable us to give a full breakdown of the distribution of age, gender, race, social class, etc. of all of our sample. In response to the concern of Drs Salib and Theophanous we can confirm that 13 (8.0%) of the 162 cases in our study were aged over 65 years. Furthermore, 19 (11.7%) were aged under 25 years and 139 (85.8%) were male.

For those interested in a more detailed account of suicides in psychiatric hospitals and prisons we suggest the following sources: Dooley (1990), Shaw et al. (2003), Shaw et al. (2004) and Gunnell et al. (2005).

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Transcranial direct current stimulation in developing countries

The suggestion by Fregni et al. (2005) that transcranial direct current stimulation (tDCS) might be an inexpensive solution to the lack of resources for the treatment of depression in developing countries is well meaning but does not take into account the real reasons for the poor uptake of psychiatric treatments. If, as the authors state, the uptake is only 34% in a resource-rich country such as the USA with its high educational levels and awareness campaigns, a rate of 17% in Brazil is not surprising and is most likely not due to the lack of affordable drugs (Chisholm et al., 2004). Cheap and effective, if not the latest, antidepressant drugs are usually available in most countries. In making their suggestion, the authors also ignore the expert opinion regarding the first-line management of depression around the world (Crawford, 2004). Most commentators would agree that this should be pharmacotherapy and not direct magnetic or electrical stimulation of the brain. The lack of primary healthcare facilities in many countries makes the suggestion of tDCS as a primary intervention impractical.

My major concern, however, is not that the authors recommend tDCS as a first-line intervention but that they recommend it as an intervention at all. By basing their


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recommendation on just one unpublished modern study, these well-respected scientists appear to have gone beyond the available evidence. Transcranial direct current stimulation is not a new intervention for depression, with a number of studies published in the 1960s and '70s (Bindman et al, 1964; Lippold & Redfearn, 1964; Lolas, 1977). However, the results were not uniformly positive and certainly not persuasive enough for this intervention to have been adopted by clinicians. Although I acknowledge that our knowledge of the brain has improved, Fregni et al do not present evidence to show how modern tDCS is superior to that used four decades ago. We need to know a lot more about tDCS before it can be accepted as an effective treatment, and must await the results of many ongoing trials. In the meantime, those with depression in the developing world should be dissuaded from unplugging their car batteries and clamping the leads on to their foreheads.

**Authors’ reply:** We thank Professor Sachdev for his letter and we certainly agree that further studies on the antidepressant effects of tDCS are needed and that the standards of application of a given therapy in any part of the world should be matched. It is certainly not acceptable that inferior treatments are used in developing countries. However, although antidepressants are often available in developing countries, problems with distribution and management of these medications often preclude regular and effective clinical treatment. For instance, in São Paulo, a relatively rich city in Brazil, shortage of antidepressants is common (Brazilian Ministry of Health website, http://portal.saude.gov.br/saude/). Those with depression are regularly faced with the choice between stopping antidepressant treatment or paying for it with their own money. Poor patients often have to interrupt their treatment, risking worsening or relapse of their depression. The situation is even worse in poorer countries. Furthermore, it is well established that higher prevalence rates of depression are found among poor, illiterate and urban migrants (Almeida-Filho et al, 2004). Therefore, those most in need are less able to afford regular antidepressant treatment.

We agree that medications should be the first line of treatment for those with newly diagnosed depression. However, we cannot ignore the fact that many in poor areas are not being treated for depression at all. Therefore, our intention is to simulate the search for new, inexpensive approaches for the treatment of depression. Our suggestion of tDCS is based on several well-conducted studies showing its modulatory effects on brain activity (Nitsche et al, 2003), past positive trials of this technique in depression (Lolas, 1977) and our preliminary data showing a significant antidepressant effect (Fregni et al, 2005). The main differences between the current tDCS protocols and those used in the 1960s and '70s derive from recent knowledge of stimulation to optimise cortical modulation and therefore clinical effects (Nitsche et al, 2003). Furthermore, substantial evidence from studies of transcranial magnetic stimulation and electroconvulsive therapy suggests that electrical stimulation is a powerful treatment for depression (George et al, 2002).

Our message is simple: a large number of those with depression are suffering because they cannot afford medicine, therefore new solutions should be offered. Transcranial direct current stimulation might represent such a solution and should be investigated further.

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**References**


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**Drug combinations for rapid tranquillisation**

It is important to develop cost-effective and efficient methods of treatment in emergency psychiatry, especially where resources are poor. Alexander et al (2004) in their paper comparing two methods of rapid tranquillisation concluded that the injectable haloperidol–promazine mix is as effective as lorazepam and suggested that in India the former is more cost-effective. We acknowledge the findings of their study but would like to make some observations regarding cost-effectiveness and methodology.

The preferred combination for rapid tranquillisation at the two largest psychiatric centres in India (the National Institute of Mental Health and Neurosciences, Bangalore, and the Central Institute of Psychiatry, Ranchi) (combined monthly out-patient attendance of >9000) is haloperidol with lorazepam rather than haloperidol with promethazine. This is guided by the literature as well as existing practice (McAllister-Williams & Nicoll Ferrier, 2002; Hughes & Kleespies, 2003). This combination is about 25% cheaper than the haloperidol–promazine mix (CIMS, 2004). Since promethazine has both alpha-1 and dopaminergic antagonism its combination with haloperidol is more likely to produce hypotension and neuroleptic malignant syndrome in agitated patients, who are often dehydrated and have electrolyte imbalance. On the other hand lorazepam decreases the required dose of haloperidol. Hence we feel that the
combined out-patient attendance of the three centres is also greater than 9000.

Our wider survey of drug formularies, including the source of Ranjan and Chandra, and local pharmacies reveals that the price of injectable haloperidol (5 mg/ampoule) ranges between Rs 4.00 and Rs 5.50; that of promethazine (50 mg/ampoule) between Rs 3.00 and Rs 7.00; and that of lorazepam (4 mg/ampoule) between Rs 7.00 and Rs 15.00. We therefore reiterate our contention that the haloperidol–promethazine mix is cheaper than (even reduced doses of) haloperidol and lorazepam.

We agree that the Overt Assessment Scale would have generated more specific results. However, the outcomes for this pragmatic trial were not chosen to generate specific results; they were chosen by the doctors and nurses of the emergency rooms to be of clinical utility. From the reaction we have already had to this study these outcomes do seem acceptable and welcome to others.

We acknowledge that there were nine more people with mania, six more misusing substances and five more already on benzodiazepines in the lorazepam arm than in the comparison arm. There is no indication, however, that the integrity of the randomisation procedure was compromised, as such chance imbalances could occur in the absence of stratification. It is unlikely that these imbalances account for the findings, as the difference in the numbers of people ‘clinically improved’ between the two interventions at 15, 30, 60 and 120 min were 31, 25, 20 and 14, respectively, and in numbers ‘asleep’ 40, 47, 35 and 14.

Although recommended by important review articles and guidelines, we have found only four randomised studies in which a total of 80 people received the combination of haloperidol and lorazepam (Aren et al, 1986; Battaglia et al, 1997; Bienenk et al, 1998; Subramaney et al, 1998). None of these studies reports useful data on time to tranquillisation/sleeping; most report scale-derived data that are difficult to interpret clinically. For such limited data to direct practice at the two largest psychiatric centres in India, as well as many other places, would seem imprudent. The effects of haloperidol plus promethazine, we would still suggest, are better proven than other prevalent approaches. Recent influential guidelines in the UK have noted this and the sister study (TREC Collaborative Group, 2003) to be the only large trials of high methodological quality in this area (National Collaborating Centre for Nursing and Supportive Care et al, 2004).

Certainly the study and others like it need to be repeated so that the evidence upon which we treat people at this vulnerable time is robust. Practice on lesser evidence is surely unethical.


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Limitations of rapid tranquillisation trial

In their excellent paper Alexander et al (2004) systematically conducted a comparison trial of intramuscular lorazepam and haloperidol–promethazine in violent or agitated patients. The authors utilised a prospective follow-up design and used proper diagnostic assessment measures, thus taking care of most of the
methodological pitfalls that have plagued research in this area.

However, a few concerns about the study persist. Ideally, there is a need for a viable placebo arm to compare the efficacy of both interventions. A comparison of a new agent with a drug previously shown to be active without a placebo comparator is uninterpretable unless one agent is superior to the others. Concluding that a drug is efficacious without a placebo comparator can lead to an incorrect assumption of efficacy if neither the investigational drug nor the active drug was, in that trial, any better than placebo would have been if included. Introducing a drug into therapeutic use on the basis of such a trial would expose patients to a compound with no greater benefit than placebo (Temple & Ellenberg, 2000). A placebo is also important in the assessment of the safety profile, as it provides a base for determining which adverse events are truly related to the investigational drug. For these reasons, placebo-controlled trials are almost universally demanded by regulatory bodies to demonstrate efficacy for any pharmacological intervention.

The authors have not described any investigations carried out to exclude toxic states, epilepsy and other organic conditions. They failed to comment on vital parameters during and after administration of both interventions. They could have assessed the level of satisfaction of the treatment team with the intervention (Petrack et al., 1996). They could also have applied any scale for aggression, agitation, alertness and psychopathology (Battaglia et al., 1997).

Certain issues merit consideration before accepting the authors’ conclusion. The better outcome of the haloperidol-promazine group compared with the lorazepam group could be because the combination group had more patients with mania than the lorazepam group and the combination group had more moderately ill and less severely ill patients than the lorazepam group. In addition, details of additional medications were not mentioned. It remains possible that some improvement was due to additional medications in both groups.

The authors commented that 23 patients failed to sleep at all during the 4-h follow-up compared with only 8 in the combination group, which is difficult to understand from Table 2. There were some inconsistent findings in the paper: sleep outcome in the combination group at 120 min were 69% and 88% in Table 2 and Table 5, respectively. Similarly, there was a discrepancy in the number of patients in the combination group who were never tranquil (Results and Table 2).

Nevertheless, we feel that the authors have taken a useful step in this relatively neglected area. Further studies are required on the effectiveness of these interventions in the hope that better understanding can lead to better treatment of violent patients.


AUTHORS’ REPLY

We thank Dr Jhirwal et al for their interest in our pragmatic trial. In this reply we shall address only those issues that have not already been covered in response to earlier comments.

The first concern regarded the omission of a placebo arm. A placebo group was initially considered but abandoned as clinicians felt this was unethical, difficult to justify and likely to pose practical difficulties in implementation. This was a pragmatic trial and the design was driven by what questions clinicians wanted answered and what interventions they (and the institution’s ethics committee) would permit. Moreover, systematic reviews reveal no evidence that placebo interventions in general have clinically important effects, and the role of placebos in clinical trials, apart from helping to minimise bias, is questionable (Hróbjartsson & Gøtzsche, 2004). Our pragmatic trial utilised adequate allocation concealment and masking of primary outcome assessors, two crucial features of trial design that significantly affect the internal validity of a randomised controlled trial (Juni et al., 2001).

Those with toxic states, epilepsy or other organic conditions were invariably excluded from the study as treating clinicians were uncomfortable about their inclusion in a randomised trial with sedative agents. Investigation results are rarely available before the intervention is instituted for violent patients under normal conditions of clinical practice.

All those subjected to tranquillisation received standard levels of care that included monitoring of vital signs and intensive nursing support. Any adverse events with regard to autonomic instability were promptly reported. Only two patients on lorazepam reported any adverse events and this is described in the discussion (paragraph 4, page 63; a printing error in Table 2 ascribes this to the combination group instead of lorazepam).

Table 2 records that equal numbers in both groups were given additional medication (always a single dose of 100 mg chlorpromazine) and this contradicts the speculation that differences in additional medication could have influenced improvement in favour of any particular group. The proportions that failed to sleep or were never tranquil reported in paragraph 1 on page 65 are correct. Table 2 reports the numbers who were tranquil/asleep and asleep at the times when outcomes were recorded. People who were tranquil or asleep at one assessment did not invariably remain so at other assessments, hence explaining the apparent discrepancy. We acknowledge the error in Table 5 where the proportion asleep at 120 min should be 69% and not 88% for TREC–India.

We hope the interest aroused by this paper will prompt greater use of trials of pragmatic design, free of industry sponsorship and aimed at answering clinical questions of relevance to real world clinical practice.


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Who pays the piper?

Given the high profile currently afforded the debate about the influence of the pharmaceutical industry in psychiatry (Moncrieff et al, 2005), Craddock & Owen’s confident prediction of the imminent usurping of the Kraepelinian dichotomy serves as a timely reminder of what is at stake here (Craddock & Owen, 2005). Both authors of the latter paper are consultants to GlaxoSmithKline and both have received honoraria for academic talks from Eli Lilly, Astra-Zeneca and GlaxoSmithKline.

This is the context within which their prophetic discussion concerning the failings of the Kraepelinian model takes place. This context is important because at the heart of the nosological edifice they erect after tearing down Babylon is the assertion that ‘...this research agenda will be best served by adopting broader inclusion criteria for the functional psychoses ...’ (p. 365). Note that they are primarily concerned with what is of value to schizophrenia researchers here.

The authors then confidently delineate how clinical practice will change with a resounding reaffirmation of the medical model ‘...psychiatrists are likely to have at their disposal simple and inexpensive tests to help identify the pathways involved in an individual’s illness ...’ (p. 365), before concluding with a suggested title for the new shibboleth: ‘psychosis-spectrum illness’ (p. 366).

The subtext here is that by broadening the inclusion criteria for the functional psychoses the mandate for the use of antipsychotic medication will follow suit, thereby permitting the pharmaceutical industry to ply its wares to many more people than before. Ironically, the much criticised Kraepelinian dichotomy means that at present at least some people (perhaps those with psychosis but otherwise specified) do not have to receive medication. Within Craddock & Owen’s proposed nosology one wonders whether anyone will escape the reach of the drug industry.

Having just won the College’s Research Prize and Bronze Medal, sponsored by Organon, with a piece of research that could be perceived as critical of both psychiatry and the drug industry, I now find myself in the position of having to debate whether or not to accept the prize. Am I a hypocrite and what would you do?


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Begetting drunkards

In his editorial, David Ball (2004) suggests that Plutarch invites an environmental explanation for the intergenerational transmission of drunkenness in his work The Training of Children in AD 110.

Actually, the full quotation from this work is:

‘The advice which I am, in the next place, about to give, is, indeed, no other than what has been given by those who have undertaken this argument before me. You will ask me what is that? It is this: that no man keep company with his wife for issue’s sake but when he is sober, having drunk either no wine, or at least not such a quantity as to distemper him; for they usually prove wine-bibbers and drunkards, whose parents beget them when they were drunk. Wherefore Diogenes said to a stripling somewhat crack-brained and half-witted: Surely, young man, your father beget you when he was drunk. Let this suffice to be spoken concerning the procreation of children; and let us pass thence to their education.’

This would seem to suggest that Plutarch, and others of that time, were concerned more about an adverse biological effect of alcohol on conception, rather than an environmental process of copying (or otherwise being influenced by) parental drinking behaviour.


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Author’s reply: I would like to thank the Reverend Professor Cook for his comments and the opportunity to provide the full quote from Plutarch, which would not be possible in a brief review of the subject. I agree with his interpretation that it reflects concern regarding the adverse biological effect of exposure to alcohol during conception and as such represents an environmental interpretation as indicated in the editorial – albeit one operating at a very early stage in development!

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One hundred years ago

The after-care of patients discharged from asylums

DR. GOODALL, the Medical Superintendent of the Joint Counties Asylum, Carmarthen, draws attention, in his annual report, to the fact that the prevalent view amongst the community at large is that insanity is an incurable state, and that whilst people who have suffered from many bodily diseases – such as gout, rheumatic fever, or disorders of the air passages – will, as a rule, take precautionary measures to avoid a recurrence of the malady, their pessimistic views regarding insanity (and, we may add, the too-frequent idea that insanity is in the nature of a miraculous visitation, unconditioned by the ordinary
environment of the sufferers) militate disastrously in many cases against prophylaxis as regards mental relapses. He relates the case of one patient who had had an attack of acute mania and returned to her home from the asylum to nurse four or five young children in scarlet fever, and at the same time had to attend to her ordinary household duties, whilst none too well fed. The result was what might have been foreseen. He describes three other cases from this year’s readmissions in which the risk of relapse might and should have been avoided. Now, amongst the better-informed and educated classes of society, the risks of relapse are doubtless reduced to a minimum; but with regard to the insane poor, almost nothing is attempted by way of educating the patients or their guardians as to the conditions likely to bring about these unfortunate results. We remember visiting a private asylum, in North Germany, whose medical director limited the number of his patients to twenty-five. These took up, he said, the whole of his time, as he had not only to study their individual characters and to discover the appropriate treatment of their individual mental or emotional idiosyncrasies, but before their discharge as convalescent he had to visit their homes, to study their mode of family life and to prescribe to the patient’s relatives rules to guide them in their conduct towards the returning member of their family. This ideal is of course impossible of attainment in the case of any but small private establishments, but what might be done, in the case of the insane poor, is the formation of charitable societies with branches all over the country, to do all that is possible, by way of publications and otherwise, to enlighten returned patients and their families as to the nature of insanity, and, without anything in the nature of meddlesome interference, by personal visitation and instruction to remedy any bad condition which seems likely to cause a return of the affection in predisposed persons. There are already societies for the after-care of friendless patients; their extension in the new direction indicated would undoubtedly be of immense benefit to both the patients and the general community.

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