Correspondence

EDITED BY KHALIDA ISMAIL

Contents

- Cognitive—behavioural therapy for schizophrenia
- Cognitive impairment in bipolar disorder
- Antenatal anxiety, parenting and behavioural/emotional problems in children
- Brain weight in suicide
- Vascular events associated with pharmacotherapy
- Outcome measurement in mental health: the Italian experience in psychogeriatrics
- Recruitment in old age psychiatry

Cognitive—behavioural therapy for schizophrenia

The Insight into Schizophrenia Research Group (Turkington et al, 2002) should be congratulated in reporting results of a large study of cognitive—behavioural therapy (CBT) plus educational handouts usual care in patients with post-acute schizophrenia. Perhaps because of space restrictions, the article leaves several important questions unanswered which it may be useful to consider here.

Regarding primary outcomes, the authors state that 25% change would represent 'good clinical improvement' and hence clinical significance but it is notable (from their Table 2: p. 525) that no outcome changed by more than 15%. We assume that the negative change score under burden of care is an error as it does not lie within the standard deviation range. Nevertheless, the authors state that the change in insight was clinically significant in the CBT group with a number needed to treat (NNT) of 10. As it is not stated in the article, perhaps the authors could clarify what thresholds for improvement in the primary outcome measures were chosen in order to calculate the NNTs. Without these, their comparison with the smaller studies by Kuipers et al (1997) and Tarrier et al (1998) (whose main NNTs were both 5) is not possible.

Concerning carer participation, it is not clear from the article whether the proportion of patients treated together with a carer was the same in both groups. As satisfaction was rated, how did the 75% rate of satisfaction in the CBT group compare with that in the treatment-as-usual (TAU) group? This is important because it will affect burden of care ratings and possibly other outcome measures, particularly over the follow-up period.

Perhaps the biggest obstacle in interpreting the study is the question of the equivalence of the arms of the study. This applies not only to the length of time given by the therapists, which should have been controlled, but also the expectation of the patients, and perhaps the educational material given out. Normally, in a randomised controlled trial the patient is blind to the potentially superior intervention. However, in this study 'all TAU patients were told that they would receive CBT intervention at the end of the follow-up period', thus indicating to patients and carers that they were in the control arm of the study.

Despite the large sample size, the study may have still been underpowered to detect the effect of a brief and relatively weak intervention delivered by non-expert therapists. This is best illustrated by re-running the power calculation. Using the authors' own figures, attempting to find an intervention that improves symptomatology by 13.5% rather than 25% would require 1326 participants not 540. The effects of weak allocation concealment, potentially unequal carer participation and other factors noted by the authors will no doubt impact upon follow-up data but it will be of interest to find out whether the modest effects detected at 5 months will be translated into clinically meaningful changes in readmission rates or functioning at 9 months and beyond (Sederer et al, 1996).


R. Ruddy, A. Mitchell University of Leeds School of Medicine, Academic Unit of Psychiatry & Behavioural Sciences in Relation to Medicine, 15 Hyde Terrace, Leeds LS2 9LT, UK.

Author's reply: We are grateful to Drs Ruddy and Mitchell for the opportunity to clarify the methodology in this study. In relation to the primary outcomes of the study, baseline means are given with change scores and confidence intervals and not standard deviations. The NNT of 10 for a 25% improvement in insight with CBT was calculated from the following results: 92 out of 257 CBT patients achieved a good clinical improvement compared with 43 out of 165 in the TAU limb. Similarly, a good outcome for overall symptoms was achieved in 112 out of 257 v. 59 out of 165 and for depression in 114 out of 257 v. 55 out of 165, respectively. The NNTs given can therefore be compared directly with those in the smaller studies listed above. The good outcomes in TAU are interesting and presumably relate to increased use of atypical antipsychotics and the effects of assertive outreach teams. The negative score given for burden of care in the CBT group should indeed be positive, as pointed out in the above correspondence. The proportion of patients treated with a carer was the same in both limbs of the study. However, as TAU patients and carers were not receiving an active comparable intervention to control for therapist time, satisfaction was not rated in that group.

This was the first pragmatic field study designed specifically to answer the question of translation as posed in the Cochrane review (Jones et al, 1999). We were aiming to discover whether any effect would accrue in a community setting with non-expert therapists using CBT. Having shown a clear effect to be present the study should now be replicated with a control psychological treatment to control for non-specific factors (Sensky et al, 2000). It will be important that this study be run from a position of clinical equipoise as indicated above. The study was adequately powered to detect the symptomatic improvement at the 25% level as calculated from our pilot study (Turkington & Kingdom, 2000). The follow-up study will deal with the issue of whether or not these clinically meaningful outcomes are durable and will also include data on relapse.


Declaration of interest
The study in question was funded by Pfizer. D.T. has undertaken consultancy work for Pfizer and has received honoraria and hospitality from Pfizer, Janssen and Lilly in relation to conference presentations on the subject of CBT in schizophrenia.

D. Turkington University of Newcastle, School of Neurosciences and Psychiatry, Department of Psychiatry, Royal Victoria Infirmary, Queen Victoria Road, Newcastle upon Tyne NE1 4LP, UK

Cognitive impairment in bipolar disorder
We write to correct some misconceptions evident in the recent editorial by Ferrier & Thompson (2002). Previously, we reported impairment in accuracy measures on recognition memory tasks and increased latencies on executive tasks in patients with bipolar disorder in remission (Rubinsztein et al, 2000). Ferrier & Thompson (2002) argue that the cognitive impairment observed in our study may have been confounded by the effects of ‘residual’ symptoms. As yet there is no generally accepted ‘cut-off’ for what constitutes remission. We devised rigorous criteria to define remission based on a patient’s own view of his or her illness, that of their psychiatrist and a structured interview. We excluded patients with scores of ≥8 on both the Hamilton Rating Scale for Depression (HRSD) and Young Mania Scale (YMS). These rating scales were devised to rate symptom severity in patients with an affective disorder and not for use in normal control subjects. Our average reported score on the HRSD was 2.1 (s.e.m. = 0.5) and on the YMS it was 0.8 (s.e.m. = 0.4). Thus, very few residual symptoms were evident and these scores certainly do not support any concern that patients had residual depression or mania.

Although the rationale for using such scales in controls is dubious, for the sake of argument we have reanalysed our data reported in Rubinsztein et al (2000) using a partial correlation analysis, as in Clark et al (2002), to control for differences observed on the HRSD (we did not rate control subjects using the mania scale) on the tests that showed significant impairment by analysis of variance (ANOVA). We still find significant impairment on both the visual recognition memory tasks and on latency measures from the one-touch Tower of London planning task (see Table 1).

These findings suggest that there are trait impairments in accuracy of visual recognition memory and slower responses on a planning task in bipolar remission. Importantly, impairments of memory and learning have been consistently observed in a number of other recent studies where rigorous diagnostic criteria for remission were applied (e.g. Van Gorp et al, 1998; Krabbendam et al, 2000; Cavanagh et al, 2002) as well as in a recent unpublished study (L. Clark, personal communication, 2002) that showed that verbal recall was still impaired following partial correlation for residual symptoms. The presence of significant impairments on executive tasks in bipolar remission has been more variable and may depend on clinical factors or the specific neuropsychological test paradigm employed. The precise functional significance of the cognitive impairment in bipolar remission needs to be examined further but may well impact on response to psychological and drug treatments. Cognitive symptoms could in fact be among the most sensitive indicators of incomplete remission.


Cognitive symptoms could in fact be among the most sensitive indicators of incomplete remission.


J. S. Rubinsztein, B. J. Sahakian Department of Psychiatry, University of Cambridge, Box 189, Addenbrooke’s Hospital, Hills Road, Cambridge CB2 2QQ, UK

Antenatal anxiety, parenting and behavioural/emotional problems in children
O’Connor et al (2002) report the effects of antenatal anxiety on behavioural/emotional problems in 4-year-old children. Their analysis of the Avon Longitudinal Study of Parents and Children (ALSPAC), a longitudinal, prospective study of women, their partners and an index child (Golding et al, 2001) takes into account a number of important covariates, including postnatal anxiety, gestational age, birth weight, and socio-economic status. They have not, however, included any measures of parenting. This is of concern because there is now a substantial body of evidence to indicate a clear association between parenting and child emotional and behavioural problems. For example, there are now a number of empirically validated models depicting the developmental progression for conduct and behaviour problems. These show a clear association between parenting practices characterised by harsh and inconsistent discipline, little positive parental involvement with the child, poor monitoring and supervision, and behaviour and conduct problems in early childhood (Patterson et al, 1989). Indeed, work using structural equation models showed that parenting and family interaction variables accounted

Table 1 Results of partial correlation analysis on tests in which ANOVAs were significant

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Partial correlation coefficients</th>
<th>P</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pattern recognition memory</td>
<td>Proportion correct</td>
<td>0.41</td>
</tr>
<tr>
<td>Spatial recognition memory</td>
<td>Proportion correct</td>
<td>0.31</td>
</tr>
<tr>
<td>Delayed matching to sample</td>
<td>Proportion correct</td>
<td>0.35</td>
</tr>
<tr>
<td>One-touch Tower of London</td>
<td>Response time</td>
<td>−0.42</td>
</tr>
<tr>
<td>Affective shifting task</td>
<td>Response time</td>
<td>0.04</td>
</tr>
</tbody>
</table>
for 30–40% of the variance in child antisocial behaviour (Patterson et al., 1989).

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

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


J. Barlow Health Services Research Unit, Department of Public Health, University of Oxford, Institute of Health Sciences, Old Road, Headington OX3 7LF, UK

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

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


T. G. O’Connor Departments of Child & Adolescent Psychiatry and Social, Genetic and Developmental Psychiatry, Institute of Psychiatry. Box number PO 80, III Denmark Hill, London SE5 8AF, UK

Brain weight in suicide revisited

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

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

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

It is interesting to note that another recently published paper (Balazic & Marusič,
Vascular events associated with pharmacotherapy

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

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

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

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

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


P. Murphy, A. Roche, A. Byrne Department of Psychiatry, Naas General Hospital, Co. Kildare, Ireland

Outcome measurement in mental health: the Italian experience in psychogeriatrics

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

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

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

We do not have programmes similar to the UK Department of Health’s ‘Mental Health Information Strategy’ nor do we collect data to compile a minimum dataset. The majority of the work in psychogeriatrics is done without quality controls and it is not possible to benchmark different services against each other. Furthermore, clinicians are deprived of the possibility of measuring outcomes of their interventions, particularly in areas where the data do not allow a direct transfer of information in everyday clinical practice.
Currently, the future of special care units (SCUs) in nursing homes in the Lombardia region is widely debated. In this region a network of 60 SCUs has been active for the past 8 years to treat patients with dementia with severe behavioural disturbances. The units are funded by the regional health system with 15 euros/patient/day more than regular nursing homes. Since the regional government has decided to optimise expenditures for geriatric and psychogeriatric services, it has been asked whether clinical results obtained in the SCU are worth the extra money. Unfortunately, the international literature analyses markedly different models, and Italian SCU researchers apparently have not performed adequate studies to measure outcomes. The only meaningful piece of evidence is an observational controlled study of 18 SCUs and 25 traditional nursing homes funded by the European Commission, which demonstrates that patients admitted to SCUs had behavioural disturbances of severity similar to patients cared for in traditional nursing home wards, but with significantly less physical restraints (Frisoni et al, 1999). Consequently, as physicians, we are unprepared to dispute the decisions of the government and cannot affect the future of SCUs.

This experience further supports the need to implement in our country a system of outcomes research. Although the promises are probably higher than the obtainable results, it is essential to start this process if we hope to improve the diffusion and the quality of psychogeriatric services in Italy.


R. Rozzini Geriatric Research Group, via Romanno I, 25122 Brescia, Italy
G. B. Frisoni Laboratory of Epidemiology & Neuromaging, IRCCS S. Giovanni di Dio, Brescia, Italy
M. Trabucco Geriatric Research Group, Brescia, Italy

Recruitment in old age psychiatry
I feel obliged to counter the assertion by O’Gara & Sauer (2002) that decrupt wards are a major contributor to poor recruitment into psychiatry. If true, it need not be so.

As an old age psychiatrist in two Black Country towns (Dudley and Wolverhampton – the latter having recently been dedicated a Black Country city) I have had several undergraduates attached to my teams. We are not a ward-based speciality, our work is done with older people in their own homes. Some of these homes may be decrepit but it is the people within that matter. They have grown old in their homes and all have interesting stories to tell. They are good people who have experienced adversity scarcely imaginable to today’s cossetted youth and are the more fascinating for it.

Our students have told us (and I have no reason to doubt their sincerity) that they have felt enriched by the experience of helping these important people in their homes. The students have learned from us and we, particularly if they have been local people, have learned a lot from them. All this is done with little recourse to the great god ‘resources’.


G. Spencer Bushey Fields Hospital, Dudley, West Midlands DY1 2LZ, UK

One hundred years ago

The verdict of ‘suicide while insane’

At an inquest held recently upon the body of a medical man in London evidence was given that he had purchased prussic acid, that the bottle containing it had been found nearly empty by his bedside, and that he had died from prussic acid poisoning. There was also evidence that there was nothing in his circumstances or his life to cause him distress and there was no evidence of any motive that might be said to have induced him to take his life. The jury found a verdict of ‘suicide’ and a discussion took place between them and the coroner as to whether the usual addition should be made to the effect that the deceased at the time of committing the act was of unsound mind, the coroner suggesting that over-study might be considered as a possible cause of mental derangement, while the fact of suicide might be treated as evidence that mental derangement existed. The jury, however, refused to accept these suggestions and the coroner in recording their verdict made the observation that it did not now involve the consequences that used to follow a finding of felo de se. The penalties attaching to self-murder constituted bygone days the effort of the law to punish one whose act had withdrawn his person from its reach. In the words of Blackstone the suicide is guilty of a double offence; one spiritual in evading the prerogative of the Almighty, the other temporal against the King who has an interest in the preservation of all his subjects. The law in Blackstone’s day, being unable to punish the dead man, used to act upon what he left behind, his reputation and fortune – on the former by an ignominious burial in the highway with a stake driven through his body, on the latter by forfeiture of all his goods and chattels to the King, hoping that his care for either his own reputation or the welfare of his family would be some motive to restrain him from so desperate and wicked an act. More recently forfeiture for felony has been abolished (in 1870) and since 1882 burial with ignominy has been forbidden and the coroner has now to give directions for the interment of the remains in a churchyard or other burial ground without any right to the celebration of a burial service but not necessarily without the celebration of any such service. The returning of verdicts of ‘suicide while of unsound mind’ without any evidence of such unsoundness is no doubt to a large extent a survival from the days of

443
post-mortem penalties, while to some extent it has its motive still in the desire to shield the dead against the charge of impiety alluded to above. Blackstone protests against the practice and against any such doctrine as that the act of suicide is itself evidence of insanity. In a very large number of cases the person who kills himself is not in a condition of mind to estimate or to control his action, but at present it often occurs that a jury finds a verdict that a person was of unsound mind when he committed a certain act upon no other ground than because he committed the act in question or upon evidence upon which no such verdict would be returned in any other case than that of suicide. This perhaps salves the feelings of surviving relatives by formally acquitting the deceased upon the charge of self-murder, but at the same time to some extent it constitutes a record of insanity in a family founded upon altogether insufficient evidence. No reasonable person would regard such a verdict as conclusive, but whatever weight it carries for the one purpose it must carry for the other. It is easy, at all events, to understand that intelligent and conscientious men upon a jury who have sworn to find a verdict according to the evidence may object to doing so as to a matter of which there is no evidence at all. It may also be suggested that a verdict to the effect that the deceased caused his own death but that the condition of his mind when he did it had not been proved to the jury’s satisfaction would inflict no unnecessary pain upon survivors without asserting as a fact that for which there is no foundation.

**REFERENCE**


Researchers by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey
Cognitive—behavioural therapy for schizophrenia
R. Ruddy and A. Mitchell
Access the most recent version at DOI: 10.1192/bjp.181.5.439

References
This article cites 5 articles, 4 of which you can access for free at:
http://bjp.rcpsych.org/content/181/5/439#BIBL

Reprints/permissions
To obtain reprints or permission to reproduce material from this paper, please write to permissions@rcpsych.ac.uk

You can respond to this article at
/letters/submit/bjprcpsych;181/5/439

Downloaded from
http://bjp.rcpsych.org/ on November 4, 2016
Published by The Royal College of Psychiatrists

To subscribe to The British Journal of Psychiatry go to:
http://bjp.rcpsych.org/site/subscriptions/