

far more regularly than this (daily, if necessary). While this might seem excessive, it appears that it is crucial to maintaining such patients in the community, and is ultimately cost-effective. Indeed, Preston & Fazio (2000) showed that for our ICM service, with a capped case-load per case worker of around 10 patients, and a mean number of annual community contacts of 164 (s.d.=20) *v.* 56 (s.d.=100) for non-intensive patients, in-patient bed-days fell dramatically (from a mean of 118 days (s.d.=113) per year before ICM, to 57 days (s.d.=91) in the second year of the ICM intervention). The control group showed no such reduction in bed-days, and the overall cost saving (factoring in the increased out-patient costs for the ICM group) at the end of the 2 years was AU\$801 475 for 65 patients ($P < 0.001$).

Thus, it is important that the precise nature of the intervention is examined before dismissing ICM as a cost-effective model of service delivery.

Preston, N. J. & Fazio, S. (2000) Establishing the efficacy and cost effectiveness of community intensive case management of long-term mentally ill: a matched control group study. *Australian and New Zealand Journal of Psychiatry*, **34**, 114–121.

UK700 Group (2000) Cost-effectiveness of intensive *v.* standard case management for severe psychotic illness. UK700 case management trial. *British Journal of Psychiatry*, **176**, 537–543.

D. J. Castle University of Western Australia and Fremantle Hospital & Health Service, Alma Street, Fremantle 6160, Western Australia

We read with interest the paper regarding the cost-effectiveness of intensive *v.* standard case management for severe psychotic illness (UK700 Group, 2000).

We feel very strongly that more comment should have been made on the topic of training (or lack of training) of the case managers involved. The findings of this large, well-designed trial are very similar to those of Muijen *et al* (1994), who found no difference in outcome between the use of community psychiatric nurses (CPNs) configured into case management teams *v.* CPNs working in a generic way. The main implication of this study was that merely reconfiguring services into different working arrangements provides no additional benefits to patients. However, it could be argued that benefits may accrue if training in research-based interventions is provided. Indeed, such training for CPNs and other health care professionals has

been developed in the Thorn and similar programmes, which focus specifically on providing skills in assertive community treatment, family interventions, psychological interventions etc. One could argue that these groups of skills, which comprise what is loosely known as psychosocial interventions, are essential to effective case management.

In the UK700 study, we note that the case managers received a 2-day induction course in case management and an unspecified amount of instruction in outreach practice given by a team leader in the assertive community treatment service from Boulder, Colorado. It seems to us that such training input is insufficient to provide the skills necessary to deliver truly effective psychosocial care. (The Thorn programme comprises 36 days of direct training plus the equivalent of 50 days of further study and project work.) We are therefore not surprised that the case managers with smaller case-load sizes could not improve on the outcomes attained by those working with the more average size case-loads.

Surely studies of training *per se* are now needed, with random allocation of case managers to training in research-based interventions or to standard practice, and measurement of outcomes for both the trainees (in terms of skills acquisition and knowledge gain) and their patients (in terms of clinical outcomes).

We are at present spending enormous amounts of money on training throughout the National Health Service and yet the vast majority of this training remains completely unevaluated. Although randomised controlled trials of training interventions will be costly, the price of not knowing whether training makes a difference is much greater.

Muijen, M., Cooney, M., Strathdee, G., et al (1994) Community psychiatric nurse teams: intensive support versus generic care. *British Journal of Psychiatry*, **165**, 211–217.

UK700 Group (2000) Cost-effectiveness of intensive *v.* standard case management for severe psychotic illness. UK700 case management trial. *British Journal of Psychiatry*, **176**, 537–543.

K. Gournay, G. Thornicroft Department of Health Services Research, Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF

Lithium and mortality

In their study of mortality in patients with affective disorder commenced on lithium, Brodersen *et al* (2000) paint an unfairly negative picture of the efficacy of lithium.

They compared mortality in patients with affective disorders who were started on lithium, irrespective of their compliance with treatment, with that of the general population. This gives a false impression that lithium could increase mortality. To assess the efficacy of lithium, they should ideally have compared those who were compliant with the treatment with those who were not and also with the general population, as Kallner *et al* (2000) did. The latter study clearly demonstrates that even though affective disorder patients have an increased mortality compared with the general population, lithium has a definite antisuicidal effect. Moreover, in unipolar depression, suicide rates increased only after patients discontinued lithium. These two studies also show how the methodology can affect the findings.

Brodersen, A., Licht, R. W., Vestergaard, P., et al (2000) Sixteen-year mortality in patients with affective disorder commenced on lithium. *British Journal of Psychiatry*, **176**, 429–433.

Kallner, G., Lindelius, R., Petterson, U., et al (2000) Mortality in 497 patients with affective disorders attending a lithium clinic or after having left it. *Pharmacopsychiatry*, **33**, 8–13.

K. S. Gracious Medway Hospital, Windmill Road, Gillingham, Kent ME7 5NY

F. Falodun West Suffolk Hospital, Hardwick Lane, Bury St Edmunds IP33 2QZ

Authors' reply: Gracious & Falodun find that our study of mortality in affective disorder patients commenced on lithium (Brodersen *et al*, 2000) paints an unfairly negative picture of lithium's efficacy. They object to our intention-to-treat analysis of all patients commenced on lithium irrespective of compliance, which showed a significantly elevated standardised mortality ratio (SMR) of 2.5. They suggest that we should have compared compliant with non-compliant patients and with the general population, as did Kallner *et al* (2000).

Kallner *et al* actually reported – even in patients compliant with lithium – that mortality in general (SMR=1.6) and suicide in particular (SMR=14.0) were significantly elevated. They also found that mortality was even higher in non-compliant patients, a result which may very well be valid. However, comparison of compliant with non-compliant patients introduces a considerable selection bias, since patients are not randomly allocated to the two groups. Rather, patients with comorbidity, such as

drug and alcohol misuse and other predictors of negative outcome (Vestergaard *et al*, 1998) select themselves to the non-compliant patient group. Therefore, a finding that non-compliant patients fare worse than compliant patients may testify only to the existence of negative predictor variables among patients who were non-compliant, instead of supporting the efficacy of lithium treatment. Neither our study nor Kallner *et al*'s allow conclusions as to whether or not lithium has specific antisuicidal effects exceeding what can be inferred from its ability to prevent recurrent illness episodes in affective disorder patients.

The efficacy of long-term prophylactic treatment with lithium has been questioned frequently (Moncrieff, 1995). We believe, as apparently do Gracious & Falodun, that despite its shortcomings lithium is a very helpful tool in the psychiatric armamentarium. Arguments that support the efficacy (or inefficacy) of long-term lithium treatment should, however, rest on sound scientific evidence.

Brodersen, A., Licht, R. W., Vestergaard, P., et al (2000) Sixteen-year mortality in patients with affective disorder commenced on lithium. *British Journal of Psychiatry*, **176**, 429–433.

Kallner, G., Lindellius, R., Petterson, U., et al (2000) Mortality in 497 patients with affective disorders attending a lithium clinic or after having left it. *Pharmacopsychiatry*, **33**, 8–13.

Moncrieff, J. (1995) Lithium revisited. A re-examination of the placebo-controlled trials of lithium prophylaxis in manic–depressive disorder. *British Journal of Psychiatry*, **167**, 569–574.

Vestergaard, P., Licht, R. W., Brodersen, A., et al (1998) Outcome of lithium prophylaxis: a prospective follow-up of affective disorder patients assigned to high and low serum lithium levels. *Acta Psychiatrica Scandinavica*, **98**, 310–315.

P. Vestergaard, R. W. Licht Mood Disorders Research Unit, Aarhus University Hospital, Skovagervej 2, DK-8240 Risskov, Denmark

Finding the evidence in forensic rehabilitation

Cure & Adams (2000) suggest that we managed to overlook 22 000 potential references including 2000 which apparently contain data relevant to our inquiries. Contrary to our belief, they also claim that the randomised trial is the preferred research methodology in forensic psychiatric rehabilitation.

These criticisms are, in our view, based on a poor understanding of the process of

rehabilitating mentally disordered offenders, and reveal a blinkered approach to novel research strategies which may be of value in such atypical settings.

Cure & Adams cited three examples of the many quality studies they allege we overlooked in our review. All were published after the final submission of our paper, but are presumably presented as examples of the treatment and rehabilitation of mentally disordered offenders. Two of the cited reviews examine anti-psychotic treatment (in people with learning disabilities and with acute schizophrenia) and the other is a review of sex offender treatment. These studies are, without doubt, most relevant to clinical forensic psychiatric practice. They do not, however, target the process of rehabilitation in a more general sense, as outlined in our paper. There is more to forensic work than drugs and specific programmes for certain offender groups.

Apparently, Cure & Adams fail to appreciate the difference between psychiatric work among forensic and non-forensic populations. That difference is the rationale for our remark that a randomised trial is not the method of choice in evaluating the outcome of forensic psychiatric rehabilitation. The crucial point is that allocation to forensic psychiatric treatment is not controlled by medical professionals but by legal authorities, refractory to the systematic and premeditated manipulation that some research requires. Although mentally disordered offenders, delivered by the courts to the hospitals, can be diverted into different treatment schemes, it is not feasible to maintain a predetermined course of rehabilitation. Important factors such as the length of incarceration, number and duration of leaves as well as external support by non-forensic caregivers, are not possible to randomise and control.

Randomised trials do not provide the only source of data on treatment efficacy, although where these trials are possible, valid and important data may be presented. Our paper did not pretend to review all articles related to the field of forensic psychiatric practice. Such magnificent and ambitious endeavours can only be embarked upon by the privileged few who are provided with considerable support from national funding institutions. Their reports may prove invaluable in guiding clinicians, assuming that the issues are correctly presented – a considerable responsibility. One obvious risk of the rapid

growth of evidence-based medicine is its inhibiting effect on the advancement of the theory of clinical practice and its potentially discouraging effects on active contributors and reviewers of articles to medical journals.

Cure, S. J. & Adams, C. E. (2000) Forensic trials inform the present and future (letter). *British Journal of Psychiatry*, **177**, 182.

Lindqvist, P. & Skipworth, J. (2000) Evidence-based rehabilitation in forensic psychiatry. *British Journal of Psychiatry*, **176**, 320–323.

P. Lindqvist Örebro Forensic Psychiatry Service, Eken, S-70185 Örebro, Sweden

J. Skipworth Regional Forensic Psychiatry Service, Auckland, New Zealand

Arachnophobia: a practical management device

While not wishing to endorse a particular product or brand, I would like to report the effectiveness of a cheap and readily available device in the management of insect and spider phobia (the 'Bug Katcha', from Betterware). The device consists of a clear Perspex box with a sliding door mounted on a long handle, allowing the offending insect to be entrapped from a distance and released without manual contact.

Having in jest presented a severely spider-phobic psychiatrist friend with such an item, I was pleased to hear that its use had provided effective exposure *in vivo* and led to a marked reduction in symptoms of anxiety. She became able to talk about and to be in a room with spiders without displaying visible signs of arousal. As many non-arachnophobes prefer not to handle spiders directly, her functioning seems to have been restored to an acceptable level.

This device may provide a practical and cost-effective way to reduce the manifestations of simple insect and spider phobias.

S. Smith Department of Psychiatry, Kidderminster General Hospital, Bewdley Road, Kidderminster DY11 6RJ

Thrombocytosis due to clozapine treatment: working towards an early marker for clozapine-induced agranulocytosis

Recently, Hampson (2000) reported thrombocytosis with clozapine, and serious consideration must be given to reports that