

The implications of the recent study of CBT for the prevention of psychosis (Morrison *et al*, 2004) need to be realistically interpreted with this background.

First, two people were excluded from the cognitive therapy arm after the trial had begun, which would have led to a non-significant result. This should have been acknowledged in the abstract, as an abstract has the most impact with service planners.

Second, after 6 months of cognitive therapy, there was a decrease in the development of psychosis compared with the control arm; however, there was similar distress for both groups. Cognitive therapy for psychosis has an aim of decreasing the distress of psychosis as well as the formulation of an explanatory model for that psychosis. It may be that a reframed and normalised explanatory language was taught to the individuals at high risk, and this led to the decreased identification of symptoms at 12 months and the masking of a psychotic episode. This would not ultimately lead to a decrease in distressing psychosis, but to a later identification of psychosis and a possible delay in pharmacological treatment.

The possible risk of harm or hazard was ignored, with a clear bias against the use of medication expressed by the authors in the discussion. Furthermore, the editorial comment alluded to the possibility of premature publication (Tyrer, 2004), but it is the implication of harm which needs to be explicitly stated.

Marshal, M. & Lockwood, A. (2004) Early intervention for psychosis (Cochrane review). Cochrane Library, Issue 3. Oxford: Update Software.

Morrison, A. P., French, P., Walford, L., et al (2004) Cognitive therapy for the prevention of psychosis in people at ultra-high risk: randomised controlled trial. *British Journal of Psychiatry*, **185**, 291–297.

Tyrer, P. (2004) From the Editor's desk. *British Journal of Psychiatry*, **185**, 360.

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Author's reply: We welcome Dr Marlowe's comments on our paper and would like to respond to the issues that he identified. The Cochrane review to which he refers examined more traditional approaches to early intervention (i.e. from first episode onwards) rather than a preventive

approach in people at high risk, so we are unsure of the relevance of this. Within the manuscript we clearly acknowledge that there were several methodological limitations, including the exclusion of two participants, but we were unable to incorporate these in the abstract as he suggests because of limitations of abstract length imposed by the *Journal* (indeed, we were asked to further reduce the abstract at proof stage).

We agree that cognitive therapy for psychosis (and the prevention of psychosis) has an aim of decreasing the distress of psychotic experiences as well as the formulation of an explanatory model for a person's difficulties. We also agree that a reframed and normalised explanatory language may be developed by the service users; however, it is unlikely that this would lead to a masking of a psychotic episode. Rather, it is intended to reduce the potential for catastrophic appraisals of psychotic experiences, which are very clearly implicated in the experience of distress (Chadwick & Birchwood, 1994), and the development of normalising appraisals is at the heart of cognitive therapy for established psychosis (Morrison *et al*, 2003) and the prevention of psychosis alike (French & Morrison, 2004). Even if such a masking were to occur, the assumption that this could cause harm clearly demonstrates a bias against the use of psychosocial interventions, as it suggests that only pharmacological treatments can reduce the potential harm that may result from an untreated psychotic episode, when there is evidence that psychological treatment is also important in this respect (de Haan *et al*, 2003).

We are accused of being biased against using antipsychotic medication; we certainly are against medication in a population who are yet to develop a psychotic disorder, for the ethical reasons outlined within our paper and elsewhere (Bentall & Morrison, 2002). Finally, it is suggested that we avoid explicitly stating the possibility of harm arising from such an intervention; however, we clearly highlight the possibility of harm resulting from stigmatisation.

Bentall, R. P. & Morrison, A. P. (2002) More harm than good: the case against using antipsychotic drugs to prevent severe mental illness. *Journal of Mental Health*, **11**, 351–365.

Chadwick, P. & Birchwood, M. (1994) The omnipotence of voices: a cognitive approach to auditory hallucinations. *British Journal of Psychiatry*, **164**, 190–201.

de Haan, L., Linszen, D. H., Lenior, M. E., et al (2003) Duration of untreated psychosis and outcome of schizophrenia: delay in intensive psychosocial treatment versus delay in treatment with antipsychotic medication. *Schizophrenia Bulletin*, **29**, 341–348.

French, P. & Morrison, A. P. (2004) *Cognitive Therapy for People at High-Risk of Psychosis*. London: Wiley.

Morrison, A. P., Renton, J. C., Dunn, H., et al (2003) *Cognitive Therapy for Psychosis: A Formulation-Based Approach*. London: Psychology Press.

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Integration of psychiatric and physical health

In The Netherlands the *British Journal of Psychiatry* is distributed among Dutch psychiatrists by courtesy of the pharmaceutical industry. For the October issue of the Dutch edition I was asked to write the editorial comment, to be circulated with the *Journal* as an accompanying letter. My focus is integrated psychiatry in medicine.

Reading the October issue I was struck by the lack of an integrated perspective. Current epidemiological findings underscore how the organisation of our healthcare system is epidemiologically unfair and does not take into account the frequent co-occurrence of psychiatric disturbances and physical illness (Kendell, 2001; Royal College of Physicians & Royal College of Psychiatrists, 2003). The fragmentation of care is seen as one of the major problems of current healthcare (Institute of Medicine, 2001); this applies with regard to treatment of physical disorders in mental healthcare and vice versa.

The editorial by Kingdon *et al* (2004) on the recommendations of the Council of Europe lacks such an integrated perspective. Among the recommendations the quality of physical care is not mentioned by the Council other than in relation to restraint, and this omission is not mentioned by Kingdon *et al*.

Similarly, the review by Thornicroft & Tansella (2004) opens with the fact that depression leads to more disability-adjusted life-years than cardiovascular disease and cancer, but it does not report their meaningful interrelation, for instance through compliance (DiMatteo *et al*, 2000). In the section 'Acute in-patient care' it is mentioned that patients with physical comorbidity should preferentially be seen in such facilities and not in community

care. The authors do not elaborate on how such treatment can be provided adequately. In the highest model of the three models presented for mental healthcare all kinds of subspecialist treatments become available. However, integrated clinics for people with comorbid physical and mental health problems are not mentioned.

Taking the current epidemiological and pathophysiological perspectives into account, the Editorial Board of a journal such as the *British Journal of Psychiatry* should consider inclusion of an integrated perspective in their review process. Such an approach will reduce psychiatrists' blind spot and psychiatrists' illusion (Cohen & Cohen, 1984) and will initiate an inspiration in health care comparable with that arising from the description of the previously fragmented and now integrated research institute (McGuffin & Plomin, 2004).

Declaration of interest

F.J.H. has received a fee for writing the editorial comment circulated with the Dutch edition of the *British Journal of Psychiatry*.

Cohen, P. & Cohen, J. (1984) The clinician's illusion. *Archives of General Psychiatry*, **41**, 1178–1182.

DiMatteo, M. R., Lepper, H. S. & Croghan, T. W. (2000) Depression is a risk factor for noncompliance with medical treatment: meta-analyses of the effects of anxiety and depression on patient adherence. *Archives of Internal Medicine*, **160**, 2101–2107.

Institute of Medicine (2001) *Crossing the Quality Chasm: A New Health System for the 21st Century*. Washington, DC: National Academies Press.

Kendell, R. E. (2001) The distinction between mental and physical illness. *British Journal of Psychiatry*, **178**, 490–493.

Kingdon, D., Jones, R. & Lönnqvist, J. (2004) Protecting the human rights of people with mental disorders: new recommendations emerging from the Council of Europe. *British Journal of Psychiatry*, **185**, 277–279.

McGuffin, P. & Plomin, R. (2004) A decade of the Social, Genetic and Developmental Psychiatry Centre at the Institute of Psychiatry. *British Journal of Psychiatry*, **185**, 280–282.

Royal College of Physicians & Royal College of Psychiatrists (2003) *The Psychological Care of Medical Patients: A Practical Guide*. Council Report CRI08. London: Royal College of Physicians & Royal College of Psychiatrists.

Thornicroft, G. & Tansella, M. (2004) Components of a modern mental health service: a pragmatic balance of community and hospital care. Overview of systematic evidence. *British Journal of Psychiatry*, **185**, 283–290.

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Quality of life and ECT

The first author of this study (McCall *et al*, 2004) has an apparent career, if not financial, conflict of interest in the treatment being reviewed. He is the President of the Association for Convulsive Therapy, the industry trade organisation, as well as the editor of its journal which he calls 'the voice of ECT' (McCall, 2004). This ought to have been revealed to readers directly; as it is, it reveals itself in the many flaws of research design which bias the study towards minimising the risks of electroconvulsive therapy (ECT).

The study included those who had had ECT as recently as 4 months previously, thus building into the research design the assumption that the adverse effects of ECT resolve within that time period; but there is evidence that this is not so. If it is not, then the study is simply comparing those who are still suffering the after-effects of ECT with those suffering more severe after-effects, a comparison which tells us nothing about the effects of ECT per se. The fact that those at baseline averaged a score of only 18 on the Mini-Mental State Examination suggests some type of cognitive dysfunction, perhaps due to ECT, even at that point.

The measures chosen by McCall *et al* in all areas – cognition, amnesia and, most importantly, what he calls quality of life and functioning – are the grossest possible, and cannot register the deficits known to be associated with ECT because they are simply not designed to do so. The authors must be aware of the work of the Service User Research Enterprise (SURE) group (Rose *et al*, 2003) in which patients describe a highly specific pattern of permanent memory and cognitive deficits post ECT. This was a rigorous systematic review of the literature on ECT's effects, and encompasses what most people would call quality of life and functioning. It revealed that for at least one-third of individuals ECT had deleterious, often devastating, effects on these areas which lasted more than 6 months and appeared to be permanent.

Individuals lost the ability to perform their jobs. They lost memory of up to 20 years of their lives. They were unable to handle schoolwork because of impaired memory function and concentration. They did not recognise persons previously well known to them. They waited anxiously for the promised 'return of memory' which

never came. None of this is consistent with improvement in quality of life.

Why then are McCall *et al*'s results so seemingly contradictory? Because he did not ask about these things. Instead, participants were asked, quite literally, whether they could wipe their own backsides. If they were simply able to get out of bed, feed and dress themselves, and use a bus or a telephone they were graded as functioning at the highest possible level. No one has ever reported that ECT affected their ability to use a toilet.

Finally, 4 weeks after ECT is too soon for individuals, who are unlikely to have tried to go back to work or school yet, to be able reliably to assess their altered memories and abilities.

McCall, W. V. (2004) Past, present and future of the *Journal of ECT* (and related treatments). *Journal of ECT*, **20**, 1–2.

McCall, W. V., Dunn, A. & Rosenquist, P. B. (2004) Quality of life and function after electroconvulsive therapy. *British Journal of Psychiatry*, **185**, 405–409.

Rose, D., Fleischmann, P., Wykes, T., et al (2003) Patients' perspectives on electroconvulsive therapy: systematic review. *BMJ*, **326**, 1363.

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Author's reply: We are grateful for Ms Andre's interest in our paper. She is the director of the Committee for Truth in Psychiatry (CTIP), which is a vocal anti-ECT group in the USA (see <http://www.harborside.com/~equinox/ect.htm>). As such, we feel that our work must be on target and of some importance to attract their criticism. Ms Andre has some specific complaints with our work, which we address as follows.

First, Ms Andre suggests that I have an apparent 'career, if not financial, conflict of interest' that invalidates the paper, especially as pertains to my role as President of the Association of Convulsive Therapy (ACT). I receive no financial or material support for serving as president of ACT; ACT is self-supporting through the dues of its members. The idea of a 'career conflict of interest' is not a concept endorsed by the American Medical Association Code of Ethics, per section 8.031 (Council on Ethical and Judicial Affairs, 1997). It is just as likely that she has a conflict of interest as director of CTIP in writing her letter – any information that supports the use of ECT threatens the position of CTIP. We would