

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.

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**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

welcome Ms Andre's full disclosure of her financial support from CTIP, and disclosure of the source of funding for CTIP since its website states that dues are not a requirement for membership.

Second, she claims that those in our study had an average Mini-Mental State Examination (MMSE) score of 18 at baseline. In fact, the mean baseline MMSE score was 27.4, as shown in Table 2 (McCall *et al*, 2004: p.407). The minimum MMSE score for inclusion was 18.

Third, Ms Andre takes us to task for not citing Rose *et al* (2003). The Rose *et al* paper has merit, but has no direct bearing on our work. Those authors 'aimed to... assess the debated distinction between efficacy, effectiveness, and satisfaction'; the focus of our paper is quality of life (QOL) and function, not 'satisfaction'. As reviewed by Asadi-Lari *et al* (2004) satisfaction and QOL are discrete, non-overlapping ideas.

Fourth, Ms Andre asserts that memory effects of ECT must necessarily affect QOL. Ms Andre is changing the definition of terms to suit her purposes, or else remains unfamiliar with the field. QOL research is '... widely regarded as a robust measure of outcome assessment...' and is defined as '... the patient's perspective of their own health status' (Asadi-Lari *et al*, 2004). It is a violation of the concept for anyone, including Ms Andre, to define a patient's QOL for them.

Fifth, Ms Andre belittles our work for showing that ECT is associated with significant improvement in activities of daily living and instrumental activities of daily living. She does not recognise that impairment of instrumental activity of daily living may be the deciding factor in referring patients for ECT (McCall *et al*, 1999) and that ECT is superior to medication in improving instrumental activities of daily living over 1 year of follow-up (McCall *et al*, 2001).

We do share one goal with Ms Andre – a desire for truth in psychiatry. We choose to reveal truth through the scientific method as opposed to rhetoric.

**Asadi-Lari, M., Tamburini, M., & Gray, D. (2004)** Patients' needs, satisfaction, and health related quality of life: towards a comprehensive model. *Health and Quality of Life Outcomes*, **2**, 1–15.

**Council on Ethical and Judicial Affairs (1997)** *Code of Medical Ethics*. Chicago, IL: American Medical Association.

**McCall, W.V., Cohen, W., Reboussin, B., et al (1999)** Pretreatment differences in specific symptoms and quality of life among depressed inpatients who do and do not receive electroconvulsive therapy: a hypothesis regarding why the elderly are more likely to receive ECT. *Journal of ECT*, **15**, 193–201.

**McCall, W.V., Reboussin, B. A., Cohen, W., et al (2001)** Electroconvulsive therapy is associated with superior symptomatic and functional change in depressed patients after psychiatric hospitalization. *Journal of Affective Disorders*, **63**, 17–25.

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

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

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**Author's reply:** Dr McCall responds to my letter but does not answer it. I get very tired of explaining to ECT proponents that the Committee for Truth in Psychiatry is not an 'anti-ECT' group, but no matter how many times and in how many contexts I do so, that false statement continues to be made. More about CTIP later, since I cannot leave Dr McCall's claims unrefuted. But much more important are the still unaddressed concerns about the methodology and validity of the McCall *et al* study.

My point about building assumptions about the longevity of ECT's adverse effects into the research design by including persons who had recently had ECT was not addressed.

Nor was any evidence presented to show that the rating scales chosen by McCall *et al* are relevant to the types of deficits reported by former ECT patients and illustrated so well in the SURE report. (Nor has there been evidence, which I requested privately from the author, to show that the study participants, who for some reason scored so poorly on both the MMSE and the IADL prior to this course of treatment, are representative of ECT patients as a whole.)

McCall's point that ex-patients and only ex-patients define what quality of life is and by what standard it should be measured is exactly my own: no ECT survivor or ex-patient ever has or ever would define 'quality of life' or 'functioning' in the terms Dr McCall uses. He says, 'It is a violation of the concept for anyone to define a patient's QOL for them', yet that's exactly what he has done. Had he asked patients themselves, an approach taken by the Rose *et al* group, he would have set off in a productive direction instead of down a blind alley.

His attempt to selectively redefine the work of Rose *et al* as research on

'satisfaction', not relevant to work on quality of life, is without foundation, as a reading of the actual study will show. It was he who brought up the work ongoing in Britain as relevant, by his reference in his first sentence to the National Institute for Clinical Excellence guidelines which came out concurrently with, and used some of the same evidence base as, the report of the Rose group at the SURE.

There is a wide literature on non-financial conflicts of interest, best described as 'an individual occupying dual roles which should not be performed simultaneously' (Fava, 2001). Those include treatment researcher and editor of a journal promoting the treatment under study.

If you yourself read what CTIP says, and not what others say about us, you will begin to wonder where the 'anti-ECT' claim comes from. We are an international organisation made up entirely of persons who have received ECT. We represent the spectrum of outcomes, from persons who feel ECT is beneficial and have had it more than once, to persons whose lives were ruined by it. None of us was truthfully informed of the risks of ECT before consenting to it, and no one liked being lied to. Our organisation exists for one purpose only: to advocate truthful informed consent for prospective ECT patients. Thus, it makes no sense to say that 'any information that supports the use of ECT threatens the position of CTIP'.

Whether you are of the opinion that being in favour of truthful and informed consent somehow makes you anti-ECT depends on whether you believe that patients have the right to full disclosure of ECT's risks – and the right to make a decision for themselves based on that information – or whether you believe that ECT's risks are such that full disclosure would result in patients en bloc deciding to forego the treatment. That Dr McCall and colleagues are in the latter camp speaks much more eloquently than their article as to what they really believe about ECT's effects on quality of life.

CTIP, founded in 1984, has never received funding of any kind.

**Fava, G. A. (2001)** Conflict of interest and special interest groups. *Psychotherapy and Psychosomatics*, **70**, 1–5.

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**Editor's note:** This correspondence is now closed.