The trial was generally well done and the procedures reasonably described. However, the researchers did not, from a cognitive–behavioural perspective, treat maternal depression. Cooper *et al* describe that treatment used cognitive–behavioural techniques but focused not on depression but on the management of mother–infant interactions.

Several randomised placebo-controlled trials have shown that CBT – when done properly – is an effective treatment for post-partum depression (Holden *et al*, 1989; Appleby *et al*, 1997; Chabrol *et al*, 2002) and for major depressive disorders (Hollon *et al*, 2002). There is an important relationship between post-partum depression and mother–infant interactions but it is not, by any means, the entirety or even the essence of post-partum depression. Although it is advisable to customise CBT to patients' circumstances, exclusive use of one focus, such as mother–child interactions, is not a test of the therapy. If the goal is to change depression, one should treat depression. Thus, the title is inaccurate and the discussion of the lack of effect of CBT for maternal depression is misleading.

Appleby, L., Warner, R., Whitton, A., *et al* (1997) A controlled study of fluoxetine and cognitive–behavioural counselling in the treatment of postnatal depression. *BMJ*, **314**, 932–936.

Chabrol, H., Teissedre, F., Saint-Jean, M., *et al* (2002) Prevention and treatment of post-partum depression: a controlled randomized study on women at risk. *Psychological Medicine*, **32**, 1039–1047.

Cooper, P. J., Murray, L., Wilson, A., *et al* (2003) Controlled trial of the short- and long-term effect of psychological treatment of post-partum depression. I. Impact on maternal mood. *British Journal of Psychiatry*, **182**, 412–419.

Holden, J. M., Sagovsky, R. & Cox, J. L. (1989) Counselling in a general practice setting: controlled study of health visitor intervention in treatment of postnatal depression. *BMJ*, **298**, 223–226.

Hollon, S. D., Haman, K. L. & Brown, L. L. (2002) Cognitive–behavioral treatment of depression. In *Handbook of Depression* (eds I. H. Gotlib & C. L. Hammen), pp. 383–403, New York: Guilford Press.

P. J. McGrath Psychology Department, Dalhousie University, Halifax, and IWK Health Centre, Halifax B3H 4JI, Nova Scotia, Canada
F. J. Elgar School of Social Sciences, Cardiff University, Cardiff, UK
C. Johnston Department of Psychology, University of British Columbia, Vancouver, British Columbia, Canada
D. J. A. Dozois Department of Psychology, University of Western Ontario, London, Ontario, Canada
S. Reyno Dalhousie University, Halifax, Nova Scotia, Canada

Authors' reply: There are many cognitive–behavioural therapies, with the precise form of the CBT shaped to the nature and context of the particular disorder. So, for example, CBT for panic disorder and CBT for bulimia nervosa (Hawton *et al*, 1989), although sharing a basic orientation and broad therapeutic principles, are very different from one another. The form of CBT in which we were interested had as its principal focus the mother–infant relationship and aspects of infant management. The reason for this was quite clear. It is well established that many forms of treatment for post-partum depression, including counselling (Holden *et al*, 1989), interpersonal psychotherapy (O'Hara *et al*, 2000), 'cognitive–behavioural counselling' (Appleby *et al*, 1997) and fluoxetine (Appleby *et al*, 1997), have significant antidepressant effects, but it has not been established that any of these interventions has an impact on the quality of the mother–infant relationship and child developmental progress, both known to be compromised in the context of post-partum depression. (The evidence for the efficacy of CBT in this context is, incidentally, less certain. Indeed, none of the three studies cited by Professor McGrath and colleagues in support of this form of treatment delivered an orthodox CBT; and one, in fact, was not a study of CBT at all, but of non-directive counselling.) We were interested in determining whether treatment that addressed the maternal role, as part of a wider supportive therapeutic relationship, would have wider benefits. The form of CBT we investigated was shaped by these concerns, and the discussion refers explicitly to this treatment and is, therefore, wholly apposite.

In several respects the findings of our trial were not what we had expected and were, to us, disappointing. However, the data were what they were, and it was our job to try to understand them. When the first trials comparing CBT with interpersonal psychotherapy for major depression were published in the 1980s, British clinical psychology reverberated with the chunterings of the CBT faithful whose instinctive reaction to the equivalence conclusion was to query the probity of the trial CBT therapists. With time, a more mature position was evolved. The findings of our study, along with the broad failure of the trials of preventive treatments for post-partum depression, would seem to us to be cause for pause and reflection, rather than instinctive defensiveness.

Appleby, L., Warner, R., Whitton, A., *et al* (1997) A controlled study of fluoxetine and cognitive–behavioural counselling in the treatment of postnatal depression. *BMJ*, **314**, 932–936.

Hawton, K., Salkovskis, P., Kirk, J., *et al* (1989) *Cognitive Behaviour Therapy for Psychiatric Problems: A Practical Guide*. Oxford: Oxford University Press.

Holden, J. M., Sagovsky, R. & Cox, J. L. (1989) Counselling in a general practice setting: controlled study of health visitor intervention in treatment of postnatal depression. *BMJ*, **298**, 223–226.

O'Hara, M., Stuart, S. & Gormon, L. L., *et al* (2000) Efficacy of interpersonal psychotherapy for postpartum depression. *Archives of General Psychiatry*, **57**, 1039–1045.

P. J. Cooper, L. Murray Winnicott Research Unit, Department of Psychology, University of Reading, Whiteknights, 3 Earley Gate, Reading, Berkshire RG6 6AL, UK

Cognitive–behavioural therapy as a treatment for psychosis

McKenna (2003) comments that Sensky *et al* (2000), in their trial of cognitive–behavioural therapy (CBT) *v.* befriending for the treatment of schizophrenia, found no advantage of CBT over befriending at the end of the 9-month intervention period. In his view, they were therefore not justified in making the claim that CBT is effective in treating negative as well as positive symptoms in schizophrenia. This assertion fails to recognise the different mechanisms by which CBT and drugs may benefit psychotic symptoms. While drugs are likely to produce a (relatively) immediate effect in altering neurotransmitter pathways, CBT (as is the case with other psychological therapies) is postulated to alter attachment-related memory (Gabbard, 2000) and develop an understanding of the illness. Cognitive–behavioural therapy utilises skills which, if successful, can be maintained by the patient long after therapy has ended. This would explain why Sensky *et al* (2000) witnessed a non-significant difference between the control and intervention groups at the end of the intervention period but a significant continued improvement in those receiving CBT (and not in those receiving befriending) at 9-month follow-up. It would not be expected that drugs would maintain a benefit 9 months after being stopped. Preliminary results of a 5-year follow-up of the cohort of patients in this study indicate that these gains in the CBT group have been maintained (D. Turkington, personal communication, 2001).

https://doi.org/10.1192/bjp.183.5.462 Published online by Cambridge University Press

As a result of the distrust of psychological approaches, studies of CBT (e.g. Kuipers *et al*, 1997; Sensky *et al*, 2000) have invariably recruited patients whose symptoms are 'resistant' to medication. The fact that these studies have still shown significant improvement over either a control intervention or routine care is testament to the greater benefits that might be demonstrated if the patients enrolled in research were representative of those in clinical practice targeted for psychological intervention.

In any case, surely the question is not which is more effective, but how both pharmacological and psychological approaches could be combined for greatest effect.

Declaration of interest

The author has received grants from the Wolfson Foundation and the Association of Physicians of Great Britain and Northern Ireland for research into the durability of cognitive–behavioural therapy for the treatment of schizophrenia.

Gabbard, G. O. (2000) A neurobiologically informed perspective on psychotherapy. *British Journal of Psychiatry*, **177**, 117–122.

Kuipers, E., Garety, P., Fowler, D., *et al* (1997) London–East Anglia randomised controlled trial of cognitive–behavioural therapy for psychosis: I: Effects of the treatment phase. *British Journal of Psychiatry*, **171**, 319–327.

McKenna, P. J. (2003) In debate: Is cognitive–behavioural therapy a worthwhile treatment for psychosis (against)? *British Journal of Psychiatry*, **182**, 477–479.

Sensky, T., Turkington, D., Kingdon, D., *et al* (2000) A randomized controlled trial of cognitive–behavioural therapy for persistent symptoms in schizophrenia resistant to medication. *Archives of General Psychiatry*, **57**, 165–172.

N. Samarasekera North Tyneside Hospital, Rake Lane, North Shields, Tyne and Wear NE29 8NH, UK

Thank you for the debate on CBT and schizophrenia (Turkington/McKenna, 2003). I would like to make the following points.

First, CBT is not a single treatment – it contains many complex components and skills, and therapist variables must be an essential issue for careful evaluation as with all psychological therapies.

Second, befriending fares significantly better than 'treatment as usual' in much CBT research. McKenna dismisses this as placebo or 'special treatment'. The fact of such significant improvement from befriending says something very serious about treatment as usual. Why should those suffering from psychosis *not* receive special treatment? The finding points to the need for more consideration of the (poorly termed) 'non-specific factors in psychotherapy' – factors clearly not treated as sufficiently important in basic care in psychosis (Paley & Shapiro, 2002).

Third, in the Sensky *et al* (2000) trial quoted, CBT patients maintained their (significant) clinical improvement at follow-up, whereas the befriending controls fell back towards previous levels. It seems that CBT gives the patients a thinking structure to help manage some of their symptoms in the longer term.

Fourth, many people believe that you cannot treat persons with psychosis as if they were suffering from something such as diabetes, for which a single remedy like insulin might be sufficient. McKenna's pronouncement on randomised controlled trials is, therefore, open to serious questioning. The need adapted approach is the antithesis of the randomised controlled trial method. In the former, the treatment is individualised and intentionally different (qualitatively and quantitatively) from one case to another and may well change over time. A randomised controlled trial, equally intentionally, eliminates individuality in the treatment. Because the idea of relationships can be especially disturbing to patients with psychosis, psychological therapies can be seen by patients as threatening; therefore, the therapy has to be very carefully 'administered' – individually and flexibly.

Fifth, there are other outcome measurements at least as important as psychiatric symptoms. The experience of treatment is very important, as well as quality of life measurements. Turkington emphasises the high take-up rate of CBT, far higher than uptake of medication in psychosis.

Sixth, thank goodness for CBT, just one of several ways for practitioners to re-discover some tools that enable them to relate to patients with psychosis. McCabe *et al* (2002) show how uncomfortable ordinary psychiatrists are without such tools when engaging with patients when the latter want to discuss symptoms.

Seventh, CBT and psychodynamic approaches overlap to a degree, at least as practised by Turkington (Martindale, 1998; Turkington & Siddle, 1998). Much has changed in psychodynamic therapy since the flawed studies of old. Modern psychodynamic approaches to psychosis have a much more flexible technique in engaging patients, and a greater and broader appreciation of mental mechanisms in psychosis.

Finally, relationship approaches in psychosis need encouragement, support and research. All psychiatrists need basic training in engaging with patients with psychosis. Research indicates that befriending might be a good place to start, but it is clearly not so easy – as the outcome of 'treatment as usual' indicates.

Declaration of interest

B.M. is Chair of ISPS (International Society for the Psychological Treatments of Schizophrenia) UK, a network the main objective of which is to promote psychological approaches to psychosis (treatment, education and research).

Martindale, B. (1998) Commentary (on Cognitive therapy for the treatment of delusions). *Advances in Psychiatric Treatment*, **4**, 241–242.

McCabe, R., Heath, C., Burns, T. (2002) Engagement of patients with psychosis in the consultation: conversation analytic study. *BMJ*, **325**, 1148–1151.

Paley, A. & Shapiro, A. (2002) Lessons from psychotherapy research for psychological interventions for people with schizophrenia. *Psychology and Psychotherapy: Theory, Research and Practice*, **75**, 5–17.

Sensky, T., Turkington, D., Kingdon, D., *et al* (2000) A randomized controlled trial of cognitive–behavioural therapy for persistent symptoms in schizophrenia resistant to medication. *Archives of General Psychiatry*, **57**, 165–172.

Turkington, D., Siddle, R. (1998) Cognitive therapy for the treatment of delusions. *Advances in Psychiatric Treatment*, **4**, 235–241.

—— / McKenna, P. (2003) In debate: Is cognitive–behavioural therapy a worthwhile treatment for psychosis? *British Journal of Psychiatry*, **182**, 477–479.

B. Martindale Psychotherapy Department, John Conolly Wing, West London Mental Health NHS Trust, Uxbridge Road, Southall UB1 3EU, UK

Efficacy of antidepressant medication

The debate between Parker and Anderson & Haddad (2003) neatly summarised contemporary thinking on the question of antidepressant effect. It was a pity, though, that they provided no discussion of any historical perspective. The wonderfully clear account provided by David Healy (2002), for instance, shows how the marketing tail of psychopharmaceuticals now often wags the entire dog. The process by which this came about has been gathering momentum since the early 1960s. Healy explores its various causes and corollaries

https://doi.org/10.1192/bjp.183.5.462 Published online by Cambridge University Press