Psychological Medicine, 2003, **33**, 755–758. © 2003 Cambridge University Press
DOI: 10.1017/S0033291703007670 Printed in the United Kingdom

CORRESPONDENCE

To the Editor:

In the July 2002 issue of this journal, Pilling *et al.* published a meta-analysis of family intervention studies in schizophrenia. They aimed at including studies where the control treatment might be 'standard care or other active treatments'. However, they included two studies where two different kinds of family intervention were compared, both kinds fulfilling the definition of 'family intervention' on p. 765 (McFarlane *et al.* 1995; Schooler *et al.* 1997). They characterized one of the options in each study as 'other active treatment'. This is unjustified, and these studies should not have been included in the meta-analysis.

The McFarlane study showed that the multiple-family groups had better outcome than the single-family modality at 2 years follow-up. In the Schooler study, a supportive monthly multiple-family group modality had as good outcome as a more intensive combination of single-family and multiple family-group modalities. Thus, these largest and methodologically best studies with multiple-family groups showed the opposite of what the authors contend, namely that single family treatment seemed to have a better effect on relapse and readmission rates than group family treatments (abstract p. 763 and p. 771).

There seems to be a couple of reasons for this error: (1) misinterpretation of the studies comparing family–group modalities; and (2) lumping together of group modalities that differ in at least two ways – (*a*) duration of treatment, and (*b*) whether the patient participated in the group or not.

Regarding (*a*), no study of family treatment shorter than 6 months has shown an advantage of the family treatment above individual control treatment (except the study by Goldstein *et al.* 1978). Thus, it seems more reasonable to assume that the short duration, not the group design of the study of Posner *et al.* (1992), entailed equal outcome.

Regarding (*b*), no study including only relatives in the family groups and excluding patients, has shown patient outcome to be better in the family modality than in non-family modalities. For this reason, we would not expect relative groups in the study of Buchkremer *et al.* (1995) to entail a better patient outcome than self-help groups. This failure is probably due to the exclusion of the patients, not to the group design.

On the other hand, both the McFarlane and the Schooler studies on long-term multiple-family treatment including patients in the groups, showed relapse rates (about 25% over 2 years) well below those found for individual care in other studies (about 50%). This suggests that such multiple-family groups are indeed more efficient than standard care, and at least as efficient as single-family treatment.

Thus, the authors' recommendation of single-family designs to the detriment of multiple-family designs does not seem to be supported by scientific evidence.

REFERENCES

Buchkremer, G., Holle, R., Schulze Mönking, H. & Hornung, W. P. S. (1995). The impact of therapeutic relatives' groups on the course of illness in schizophrenic patients. *European Psychiatry* **10**, 17–27.

Goldstein, M. J., Rodnick, E. H., Evans, J. R., May, P. R. A. & Steinberg, M. R. (1978). Drug and family therapy in the aftercare of acute schizophrenics. *Archives of General Psychiatry* **35**, 1169–1177.

McFarlane, W. R., Lukens, E., Link, B., Dushay, R., Deakins, S. A., Newark, M., Dunne, E. J., Horen, B. & Toran, J. (1995). Multiple-family groups and psychoeducation in the treatment of schizophrenia. *Archives of General Psychiatry* **52**, 679–687.

Pilling, S., Bebbington, P., Kuipers, E., Garety, P., Geddes, J., Orbach, G. & Morgan, C. (2002). Psychological treatments in schizophrenia: I. Meta-analysis of family intervention and cognitive behaviour therapy. *Psychological Medicine* **32**, 763–782.

Posner, C. M., Wilson, K. G., Krai, M. J., Lander, S. & McIlwraith, R. D. (1992). Family psychoeducational support groups in schizophrenia. *American Journal of Orthopsychiatry* **62**, 206–218.

Schooler, N. R., Keith, S. J., Severe, J. B., Matthews, S. M., Bellack, A. S., Glick, I. D., Hargreaves, W. A., Kane, J. M., Ninan, P. T., Frances, A., Jacobs, M., Lieberman, J. A., Mance, R., Simpson, G. M. & Woerner, M. G. (1997). Relapse and rehospitalisation during maintenance treatment of schizophrenia. *Archives of General Psychiatry* **54**, 453–463.

HÅVARD BENTSEN

Address correspondence to:
Dr H. Bentsen,
Aker University Hospital,
Division of Psychiatry,
Sognsvannsveien 21,
N-0320 Oslo,
Norway.

To the Editor:
We read with great interest the recent paper by Pilling *et al*. (**32**, 783–791) in which they reported the results of meta-analyses of randomized controlled trials of social skills training and cognitive remediation.

Cognitive remediation is a relatively new approach in the treatment of schizophrenia. It is potentially an important tool, because cognitive deficits persist despite pharmaceutical treatment and restrict the possibilities for functional recovery. Studies investigating the effect of cognitive interventions are now emerging rapidly. However, these studies differ greatly with regard to methodological quality, making it increasingly difficult to judge the merits of this new approach.

The strength of this meta-analysis is that it applies strict methodological criteria to this field with its increasing amount of empirical data. The authors were able to select five studies that met the following criteria: (*i*) randomized controlled design; (*ii*) comparison against standard care or other active interventions; and (*iii*) the intervention formed a 'programme focused on improving cognitive function using a procedure implemented with the intention of bringing about an improvement in the level of that specified cognitive function'. These five studies yielded two effect sizes for the domain of attention, four for verbal memory, two for visual memory and two for mental state. Meta-analyses of these effect sizes yielded no positive effect of intervention on any of the cognitive domains. The authors conclude that the results do not justify the incorporation of cognitive remediation into clinical practice and suggest that it might be more fruitful to focus treatment on the functional deficits associated with cognitive impairment, rather than on the direct remediation of the cognitive impairment itself.

This conclusion, however, may be too bold, given the paucity of the data on which it is based. In fact, our own meta-analysis of cognitive remediation studies yielded a mean weighted effect size of 0·45, indicative of a moderate positive effect of intervention (Krabbendam & Aleman, 2003), which is in accordance with the positive results from the recent meta-analysis by Kurtz *et al*. (2001). We were able to include 12 controlled studies, 10 of which used a randomized design, that compared the effects of the intervention either to standard care or to another active intervention. Our analysis further differed from the study by Pilling *et al*. in that we calculated one effect size for each study, thus combining the results of multiple tests into one d-value. In contrast, Pilling *et al*. investigated the effect of cognitive differences for each cognitive domain separately. This may have lead to a number of effect sizes too small to detect significant effects. Therefore, we believe that the jury is still out with respect to the possible benefits of cognitive treatment. It may be premature to consider our results a sufficient basis to implement cognitive treatment in clinical practice, but neither do the current data justify the opposite conclusion.

REFERENCES

Krabbendam, L. & Aleman, A. (2003). Cognitive rehabilitation in schizophrenia: a quantitative analysis of controlled studies. *Psychopharmacology* (in the press).

Kurtz, M. M., Moberg, P. J., Gur, R. C. & Gur, R. E. (2001). Approaches to cognitive remediation of neuropsychological deficits in schizophrenia: a review and meta-analysis. *Neuropsychology Review* **11**, 197–210.

Pilling, S., Bebbington, P., Kuipers, E., Garety, P., Geddes, J., Martindale, B., Orbach, G. & Morgan, C. (2002). Psychological treatments in schizophrenia: II. Meta-analyses of randomized controlled trials of social skills training and cognitive remediation. *Psychological Medicine* **32**, 783–791.

LYDIA KRABBENDAM AND ANDRÉ ALEMAN

Address correspondence to:
Dr L. Krabbendam,
Department of Psychiatry and Neuropsychology,
Maastricht University,
PO Box 616 (*PAR45*),
6200 MD Maastricht,
The Netherlands.

The Authors reply:
In response to our two articles of July 2002, Bentsen and Krabbendam & Aleman raise a number of questions concerning, respectively, the analysis of the studies of cognitive remediation, and the analysis of family intervention in schizophrenia.

Krabbendam & Aleman list a number of issues concerning our meta-analysis. We agree on the important problem that persistent cognitive deficits present in schizophrenia and the difficulties in drawing conclusions about the value of cognitive remediation, given the variable methodological quality of the trials so far reported. They argue that our conclusion that the current data does not support the use of cognitive remediation in clinical practice is too bold and premature. In support of this they cite two more recent meta-analysis, one by themselves (Krabbendam & Aleman, 2003), and a further recently published review by Kurtz *et al.* (2001). They report in their own meta-analysis that they were able to identify 12 controlled studies, of which 10 used a randomized design and they were able to obtain a weighted mean effect size of 0·45, which was in line with that reported by Kurtz *et al.* While we were not able to review the trials included in the paper of Krabbendam & Aleman, we were able to review those published in the Kurtz *et al.* review (2001). Most of the trials reported by Kurtz were familiar to us, and included a significant number that we had rejected from our meta-analysis on the grounds that they were not randomized controlled trials, that they presented no usable data, or that they had other methodological weaknesses which meant that they failed the criteria for inclusion within our own meta-analyses. As Krabbendam & Aleman point out, one of the merits of our meta-analyses is the strict methodological criteria that we applied. Of course further new trials may emerge, which may lead to a revised position on the effectiveness of cognitive remediation, but we have recently conducted further searches in the area and have been able to identify only one further trial which we felt merited inclusion in the meta-analyses. The inclusion of this further trial has not shifted our view that cognitive remediation should not at present form part of routine clinical practice. Of course further studies may emerge in Krabbendam & Aleman's own meta-analysis that require a revision of this position.

They also raise a further issue about the way in which we aggregated effect sizes. We are not entirely clear from their letter as to the method that they propose. However, we did not feel it appropriate in our own meta-analyses to combine effects across different outcome domains, nor to take in the contribution of the same patient groups as would inevitably be the case if outcomes and effect sizes were combined across the different domains examined in these trials.

Bentsen is concerned about our meta-analysis of family interventions. First, he challenges our classification of 'other active treatments', suggesting that we should not have included any active family intervention as a comparator for any other family intervention. The approach that we took within our meta-analysis was simply to define other active treatments as any active intervention of a psychosocial nature, including other family interventions. We accept that this is not typical of the approach that we took to the analysis of a range of other interventions included in our meta-analysis. It is nevertheless entirely consistent with the approach that we have taken.

Bentsen's critique of our paper focuses on the identification of the merits of two studies (McFarlane *et al.* 1995, and Schooler *et al.* 1997), which as he rightly points out are two of the larger studies in the area. However, we took group family treatments as our point of analysis, and there was no rationale for us to identify these two studies selectively as Bentsen does. He also chooses to highlight follow-up at 2 years, and then goes on to suggest that a separate analysis of the outcomes of these two studies at 2 years would go against our general suggestion that single family treatments seem to have a better effect on relapse. Our view on the overall effectiveness of single and group family approaches to treatment was taken on consideration not just of the two studies and outcomes that Bentsen refers to, but from an overview of their effectiveness and the acceptability of treatment. We would argue that when all factors are considered, on an intention to treat basis and taking into consideration readmission and relapse rates, together with treatment non-compliance, that our statements about the relative merits of single and family group interventions hold. We therefore think his specific comments on our misinterpreting the studies of family group modalities are not valid.

He also raises a second question regarding issues about both the duration of treatment and the participation of patients in treatment.

As stated in the original paper, it was not possible to perform an analysis with the data available that would support the view he puts forward about the impact of duration of treatment on the overall effectiveness of the studies. He is also concerned about the inclusion or otherwise of family group members. Both of these seem to us important points. Indeed in our further analysis of data (Kendal *et al*. 2002), both have been examined. We now believe that evidence is emerging to support the view that duration of treatment should be at least 9 months. Moreover, while treatments are effective without the involvement of family members, they tend to be more so if the relatives are involved.

Bentsen also quotes respective relapse rates of 25% and 50% in comparing single and group family treatments. He goes on to suggest that this supports the view that they are at least as effective. Again, our concern was to analyse group family treatments as a whole, and not those specifically developed within the McFarlane and Schooler approach. There are two problems with this. First, we decided not to focus specifically on those two studies, and *a priori* would have had no reason for doing so. Secondly, the relapse figures quoted are not based on an intention to treat analysis, which is the approach we ourselves took. We believe that if these approaches were taken, our original conclusions would stand. However, we should point out, as we did in the paper, that we do see potential advantages on outcomes other than those such as relapse and readmission which group family treatment may well have over single family interventions. We would hope that further research in this area can deal with this important point.

STEPHEN PILLING AND PAUL BEBBINGTON

CORE,
Department of Clinical Health Psychology,
University College London,
1–19 Torrington Place,
London WC1E 7HB.

https://doi.org/10.1017/S0033291703007670 Published online by Cambridge University Press