

Schooler, N. R., Keith, S. J., Severe, J. B., et al (1997)
Relapse and rehospitalisation during maintenance
treatment of schizophrenia. *Archives of General
Psychiatry*, **54**, 453–463.

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Authors' reply: We appreciate Professor Falloon's comments about our paper (Bellack *et al*, 2000), as well as his thoughts about the broader Treatment Strategies in Schizophrenia (TSS) project from which we drew the subjects of our paper. He is certainly correct that the study was not designed to systematically dismantle his behavioural family therapy (referred to as 'applied family management' in our study), and that we did not conduct a formal economic analysis of the outcomes.

Most of his comments refer to the parent TSS study, its design, its conduct and the interpretation of its results, and go substantially beyond the relatively limited questions that we addressed in our paper. The goal of our article was to examine the effects of family treatments on communication and whether changes in communication mediated patient outcomes. The TSS study compared behavioural family treatment with a less-structured family support programme. In Bellack *et al* (2000) we reported that there were no differences between the two family treatments in communication or problem-solving, and changes in communication that may have occurred did not mediate outcomes of interest, including any difference in rehospitalisation.

The TSS study was, as Professor Falloon has written (Falloon *et al*, 1996), designed to compare two family treatments based on common assumptions and common principles. Applied family treatment (AFM) was based on the behavioural family therapy developed by Falloon and "differed from Supportive Family Treatment in intensity and in the site of delivery of treatment . . . Further, AFM has a behavioral focus, with the intent of providing specific training in communication and problem solving" (Falloon *et al*, 1996, page 47). Both family treatments included parallel family support groups, and the two treatments did not differ in their orientation, as Professor Falloon now suggests. Stress management

was a feature of the two family interventions, but they were not conceptualised that narrowly. Falloon's treatment (Falloon *et al*, 1996), in particular, was viewed as a comprehensive intervention that included case management and a multi-factorial educational component designed to modify patterns of communication within the family. He reports that the problem-solving component "seldom requires more than 5 hours of teaching", yet his programme required 13 weekly sessions in the home followed by 13 bi-weekly sessions and then monthly sessions for up to an additional 6 months. Neither the data nor our own clinical observations support the thesis that most families are able to learn the targeted skills at all, let alone in 5 hours of training. In regard to cost benefit, it should be noted that there was no demonstrable benefit from this extensive training and the cost of more than a year of home visits would be prohibitive in most clinical settings: a statistical economic analysis would be redundant with this self-evident finding.

Professor Falloon makes two other assertions with which we disagree. First, he indicates that the monthly educational groups and the behavioural training were incompatible and that therapists "confused the two approaches". On the contrary, the two approaches were designed to support one another by providing a common model of the illness to families in two different forums with the added benefit of multi-family support and sharing. Moreover, Professor Falloon provided quality control for the home visits but had no ongoing role in oversight of the multi-family support groups, so it is difficult to understand how he determined the existence of this putative confusion on the part of therapists.

Second, Professor Falloon notes that there was a difference in rehospitalisation between the two family treatments over 2 years under "optimal" medication conditions. The numbers cited are selected from a large analysis that tested the effects of the family treatment comparison as well as of medication condition. There was neither a statistically significant main effect of family treatment nor an interaction of family treatment and medication. The difference he cites is thus not appropriately described as "not quite" statistically significant. The clear and overarching conclusions to be drawn from the entire data set is that Falloon's behavioural family therapy did not produce any differential benefit to family members or patients, despite its

high cost. We strongly support clinical and humanistic goals of improving the quality of interactions in families with a child suffering from schizophrenia, and of reducing stress experienced by family members and patients. However, we also believe that the TSS data provide a convincing argument that the behavioural treatment approach is not a useful or effective strategy for most families.

Bellack, A. S., Haas, G. L., Schooler, N. R., et al (2000) Effects of behavioural family management on family communication and patient outcome in schizophrenia. *British Journal of Psychiatry*, **177**, 434–439.

Falloon, I. R. H., McGill, C. W., Matthews, S. M., et al (1996) Family treatment for schizophrenia: the design and research application of therapist training models. *Journal of Psychotherapy Practice and Research*, **5**, 45–56.

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Psychological therapies in anorexia nervosa

It is heartening to see trials of therapy for anorexia nervosa, the most lethal of psychiatric illnesses and the cinderella of research. Dare *et al* (2001) have shown that over a year it is possible to effectively treat a severely ill group of young adults with poor prognostic features, and to do so on an out-patient basis.

I am surprised, though, that they feel able to conclude that "specialised psychotherapies are more effective than routine treatment". The two therapies which came out 'top' (family therapy and focal psychoanalytic psychotherapy) were given by the same three highly experienced therapists, and the next best therapy (cognitive-analytic therapy) was given by trained specialists in eating disorders, whereas 'routine treatment' was provided by junior psychiatrists on 6-month rotations who had to hand over to colleagues during the year of patient contract.

Certainly, confidence in at least one model of therapy is an important component of an experienced therapist's effectiveness, but for me the clearest implication of the study is that patients who suffer from this chronic condition do best with

continuity of care from the most experienced therapists. I am not sure we can say anything at all yet about choice of specific therapy.

Dare, C., Eisler, I., Russell, G., et al (2001)

Psychological therapies for adults with anorexia nervosa. Randomised controlled trial of out-patient treatments. *British Journal of Psychiatry*, **178**, 216–221.

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The paper by Dare *et al* (2001), on a trial of psychological treatments for anorexia nervosa, has two major shortcomings. The investigators planned for a year of weekly sessions of 50 minutes of psychoanalytic therapy; a year of weekly to 3-weekly sessions (60 to 75 minutes) of family therapy; 23 sessions (50 minutes) of cognitive-analytic therapy (CAT), and an unstated frequency of 30 minute sessions for 1 year for the 'routine treatment' group. The patients in the psychoanalytic arm ended up receiving a mean of 24.9 sessions as opposed to 12.9 for the CAT, 13.6 for the family therapy and 10.9 for the 'routine' arm. The differences in the numbers of sessions planned and those actually taking place has not been taken into account in evaluating the results. As summarised by Bergin & Garfield (1994), a large number of different studies show that more sessions are associated with greater improvements. However, the relationship is not linear and begins to taper off after 26 sessions: a figure almost reached by the patients in the psychoanalytic arm but far removed from that of the other three groups.

Not only did the 'control' group receive the fewest number of sessions, with each session lasting only 30 minutes, but as noted and implied by the authors: therapists assigned to this group had the least commitment to and experience in treating anorexia nervosa. The paper does not state how many therapists each patient 'went through' during the course of the study. All these factors would predispose to the formation of poor working alliances compared with the other groups. Thus, the poor results obtained by the 'control' group could be accounted for by a combination of fewer sessions of shorter duration and weak therapeutic alliances, rather than the superiority of specific psychological treatment models.

Bergin, A. E. & Garfield, S. (1994) *Handbook of Psychotherapy and Behaviour Change* (4th edn), pp. 58–61. New York: John Wiley & Sons.

Dare, C., Eisler, I., Russell, G., et al (2001)

Psychological therapies for adults with anorexia nervosa. Randomised controlled trial of out-patient treatments. *British Journal of Psychiatry*, **178**, 216–221.

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I would like to comment on the Maudsley trial evaluating three psychotherapies for anorexia nervosa compared with routine treatment (Dare *et al*, 2001). I congratulate the team on their efforts in this study in a research area fraught with difficulties and for their major contribution to knowledge in the eating disorders field. The authors rightly conclude that little can be drawn from the study regarding the differential impact of the therapies used. However, the paper did not make clear the differences between the conditions other than the models of therapy. The experience and qualifications of therapists were stated for focal psychoanalytic therapy and family therapy but not for cognitive-analytic therapy (CAT) and one can only conclude that the CAT therapists were not trained or qualified in CAT. Also, the total contact hours in each condition varied widely. The longer the contact hours the more impact the therapy. Perhaps the trial indicates that to treat moderately severe anorexia nervosa effectively, trained and experienced therapists and/or over 15 contact hours (over 18 × 50-minute sessions) are required. The need for experienced staff delivering therapies of adequate length is well known within the field (e.g. Palmer *et al*, 2000) but may not be fully appreciated by those commissioning or funding services. These are perhaps more important variables affecting outcome than the specific therapeutic modality used.

Dare, C., Eisler, I., Russell, G., et al (2001)

Psychological therapies for adults with anorexia nervosa. Randomised controlled trial of out-patient treatments. *British Journal of Psychiatry*, **178**, 216–221.

Palmer, R. L., Gatward, N., Black, S., et al (2000)

Anorexia nervosa: service consumption and outcome of local patients in the Leicester service. *Psychiatric Bulletin*, **24**, 298–300.

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Author's reply: We agree in part with the points made in these letters. Dr Okhai comments on the different treatment intensity between the conditions and in particular in the 'control' condition. The 'control' treatment was intended as a surrogate for placebo treatment. It is ethically difficult to have a placebo treatment for anorexia nervosa given the high morbidity of the condition and the lack of any placebo response. Our aim, therefore, was to have a 'control' condition similar to treatment as usual that would/could be offered in general adult psychiatry units. It could be argued that this therapy was better than that offered in many such positions in that regular supervision was given by an expert in eating disorders. Furthermore, the patients (2–3 per psychiatrist) were offered treatment for up to a year. We agree that in anorexia nervosa as in other conditions the therapeutic alliance is a key factor in response to therapy. We would argue that the specialist treatments have a specific focus on the therapeutic alliance. Indeed, it is perhaps noteworthy that the results of this study led to a change in the practice of cognitive-analytic therapy on the unit in that it is now preceded by a short course of motivational enhancement therapy to facilitate engagement (Treasure & Ward, 1997).

The number of sessions attended may be a sensitive marker of the therapeutic alliance in anorexia nervosa. For example, in a previous study comparing cognitive-behavioural therapy for anorexia nervosa with dietary management all patients dropped out of the dietary management group early in treatment (Serfaty, 1999).

We agree with Dr Morris that the important 'take-home message' is that specialised therapists following a specific therapeutic approach offer the best outcome in anorexia nervosa. This complements the analysis made by Nielsen *et al* (1998), in which he found that mortality was lower in regions of the country with specialised services. It is, therefore, of concern that such skills are in limited supply.

Nielsen, S., Møller-Madsen, S., Isager, T., et al (1998)

Standardized mortality in eating disorders – a quantitative summary of previously published and new evidence. *Journal of Psychosomatic Research*, **44**, 413–434.

Serfaty, M. A. (1999)

Cognitive therapy versus dietary counselling in the outpatient treatment of anorexia nervosa: effects of the treatment phase. *European Eating Disorders Review*, **7**, 334–350.