

An interesting study by Davis & Phelps (1995) showed that the concordance for schizophrenia in monozygotic twins discordant for handedness is much higher than in twins concordant for handedness (60 *v.* 32%). Again, this difference can be explained only by the presence of an environmental factor acting *in utero*, which disrupts the neurodevelopment causing schizophrenia and altering handedness. No genetic factor can explain it.

The evidence from the literature therefore directs towards an environmental factor, which acts during neurodevelopment disrupting handedness and predisposing for schizophrenia. This hypothesis is briefly considered in the article, but then dismissed because of the results of a meta-analysis. The authors compared patients suffering from schizophrenia with patients suffering from other neuropsychiatric disorders. Both groups showed an excess of left-handedness, but in the schizophrenia population the excess was significantly higher.

The authors reached the conclusion that this shows the involvement of a genetic mechanism, but this is only one of the possible explanations. Another possibility is that there are neuropsychiatric disorders that are not neurodevelopmental in origin. Another explanation is that in certain disorders the neurodevelopmental damage acts before or after the time when handedness is established.

It is therefore my opinion that the literature on handedness and schizophrenia, comprehensively reviewed by the authors, confirms that both genetic and environmental factors have to be accounted for in the aetiology of schizophrenia. The 'right shift' is still only a hypothesis and the meta-analysis by Sommer *et al* does not corroborate or refute it.

**Davis, J. O. & Phelps, J. A. (1995)** Twins with schizophrenia: genes or germs? *Schizophrenia Bulletin*, **21**, 13–18.

**Salvesen, K. A., Vatten, L. J., Eik-Nes, S. H., et al (1993)** Routine ultrasonography *in utero* and subsequent handedness and neurological development. *British Medical Journal*, **307**, 159–164.

**Sommer, L., Aleman, A., Ramsey, N., et al (2001)** Handedness, language lateralisation and anatomical asymmetry in schizophrenia. Meta-analysis. *British Journal of Psychiatry*, **178**, 344–351.

**Steinmetz, H., Herzog, A., Schlaug, G., et al (1995)** Brain (a)symmetry in monozygotic twins. *Cerebral Cortex*, **5**, 296–300.

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## Stress management and schizophrenia

Bellack *et al* (2000) have addressed the important question of delineating the core therapeutic components of carer-based stress management that account for the improved course of schizophrenic disorders found consistently when these methods have been integrated with optimal pharmacotherapy. Their conclusions deserve close scrutiny, particularly when they make strong statements about the relative cost-effectiveness of different approaches, without the benefits of any economic analysis. Their conclusion that cognitive-behavioural strategies aimed at enhancing the problem-solving of patients and their key caregivers do not contribute to the clinical benefits cannot be drawn definitively from the study they report.

This study is extremely complex and was not designed to evaluate the comparative effectiveness of the key components of cognitive-behavioural family interventions. All cases were offered 25 sessions of structured education in multi-family groups over 24 months. These educational groups aimed to assist patients and their caregivers in the management of their disorders and the stresses in their lives. However, half the sample was assigned at random to an additional 29 sessions of home-based education that employed active learning methods to enable the patient and carers to conduct weekly self-help sessions in the home. During these sessions they were expected to work on the personal problems and goals that they considered important. They were taught to use a problem-solving approach, with guide sheets to structure their discussions and to provide records of their plans. This home-based training ceased after 12 months, but the multi-family educational groups continued to 24 months.

From the outcome measures reported, there was no significant difference between the two stress management conditions in forestalling hospital admissions over the 24 months. Thus, the addition of the problem-solving training, which seldom requires more than 5 hours of teaching, was considered redundant. The method attributed to Falloon *et al* (1984) does not include the additional monthly educational groups used in this project, and it is probable that the combination of two approaches that emphasised somewhat different objectives may have proved confusing to some participants and excessive to others. We certainly

observed that at times of crisis the therapists confused the two approaches. A definitive study that aimed to compare the benefits of the educational and problem-solving strategies would need to ensure that each approach was more clearly defined, and would have to control for the time participants were exposed to the contrasting methods and the therapist's competence and enthusiasm for both methods. It may also be important to consider that maximum benefits might be reached with a lower-than-standard course of education for many cases, and that too much of a good thing may not produce the best results. It is important to note that similar multi-family group education approaches have not always proved successful (McCreadie *et al*, 1991) except where problem-solving training has been a core component (McFarlane *et al*, 1995).

Furthermore, it is interesting to refer to the earlier publication of this important study of combinations of various maintenance medication dosage strategies (Schooler *et al*, 1997). For those cases receiving the care-based stress management approaches who were also maintained on optimal doses of medication throughout the 24 months, 19% of those offered the additional problem-solving training in the first 12 months were admitted to hospital in contrast to 31% of those receiving only the education group sessions. Although this difference does not quite achieve statistical significance, the trend is clear, and this is in accord with the consistent observation of somewhat greater efficacy of the problem-solving approach when it is more clearly integrated with mental health education (Falloon *et al*, 1999).

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

**Falloon, I. R. H., Boyd, J. L. & McGill, C. W. (1984)** *Family Care of Schizophrenia: A Problem-Solving Approach to the Treatment of Mental Illness*. New York: Guilford Press.

—, **Held, T., Coverdale, J., et al (1999)** Family interventions for schizophrenia: a review of long-term benefits of international studies. *Psychiatric Rehabilitation Skills*, **3**, 268–290.

**McCreadie, R. G., Phillips, K., Harvey, J. A., et al (1991)** The Nithsdale schizophrenia surveys. VIII: Do relatives want family intervention – and does it help? *British Journal of Psychiatry*, **158**, 110–113.

**McFarlane, W. R., Lukens, E., Link, B., et al (1995)** Multiple-family groups and psychoeducation in the treatment of schizophrenia. *Archives of General Psychiatry*, **52**, 679–687.

**Schooler, N. R., Keith, S. J., Severe, J. B., et al (1997)**  
Relapse and rehospitalisation during maintenance  
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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