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. A 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.

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Treasure, J. L. & Ward, A. (1997) Cognitive analytical therapy (CAT) in eating disorders. *Clinical Psychology and Psychotherapy*, **4**, 62–71.

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Quality of evidence in meta-analysis

Thase *et al* (2001) provide some evidence that venlafaxine is superior to selective serotonin reuptake inhibitors in terms of relapse rates. Although the authors are honest about the limitations of this meta-analysis, these need further exploration.

All meta-analyses should be based on a systematic review of the literature, which should include an exhaustive search for trials including those unpublished (grey data). Failure to do this could result in publication bias, because studies showing negative results or no differences are less likely to be published than those showing positive results. Failing to identify these missing studies may skew the results of this meta-analysis towards favouring venlafaxine. Although the authors identified a further 12 trials (not included in their analysis), there is no description of the search technique and it is possible that other trials were missed.

One way to identify possible publication bias is to construct a funnel plot (Fig. 1). This is a simple technique where effect size (in this case odds ratio taken from Table 3 of the paper) is plotted against the number of subjects in each study (Table 1). The principle of a funnel plot is that small studies are less precise and the precision of a study increases, approximating to the true effect, as the sample size gets larger. This produces an inverted funnel shape. Data missing from the lower left segment of the plot suggests small negative studies have not been identified.

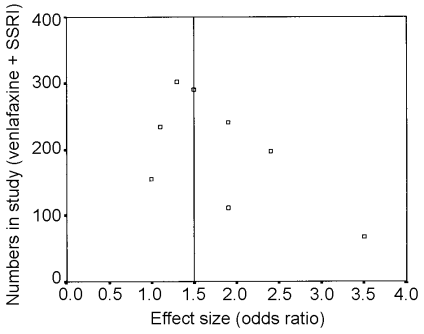

Fig. 1 Funnel plot of data from Thase *et al* (2001). The vertical line is the pooled effect size about which a symmetrical inverted funnel shape should appear.

The authors do not include the 12 other trials they identified in their paper in the meta-analysis but go on to undertake a "qualitative review" of these trials. This 'vote counting' technique can be misleading as smaller trials are given as much weight as larger ones.

There would be a tendency for some evidence-based practitioners to disregard this paper completely. I think this is to miss the point of evidence-based medicine, which is not to be reductionist about evidence. Rather, we should use our skills in evidence-based medicine to decide where on a continuum between very good and very bad a particular paper lies, and use its conclusions accordingly.

Thase, M. E., Entsuah, A. R. & Rudolph, R. L. (2001) Remission rates during treatment with venlafaxine or selective serotonin reuptake inhibitors. *British Journal of Psychiatry*, **178**, 234–241.

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Placebo response in depression

An unstated conclusion to Gavin Andrews' editorial (2001) is surely that placebo controlled trials are absolutely essential to our understanding of the true effects of antidepressants. Without placebo trials Andrews' main conclusion that the placebo effect is significant and worth potentiating would not be possible.

It is not sufficient to prove that new treatments are better than or equivalent to existing treatments because we do not know that the existing treatment is still better than 'placebo' treatments. Today's 'placebo' treatment may not be the same as that of 10 or 20 years ago when the original placebo trials were done. Further, there may be considerable differences between groups with the same diagnosis. This is all well demonstrated in the study of tricyclic antidepressants in children. Since it was thought unnecessary and unethical to do placebo trials in children and adolescents, new antidepressants were tested only against existing ones and found to be effective in 50 to 70 per cent of cases. Only after 20 or so years of such trials were placebo trials done and the 'placebo' treatment (probably the accompanying environmental, individual and family treatment) was found to be just as effective as the drug. In this time numerous children were treated unnecessarily with tricyclic antidepressants and several may have died from cardiac arrhythmia. This was not an ethical way to introduce new drugs.

Among additional reasons for placebo controlled trials are first, that far more people have to take part in a trial comparing a new treatment with an existing treatment because the difference in effect is much less than with placebo. Thus, more people will be exposed to a new treatment with unknown side-effects. Second, placebo controlled trials are the only way to get accurate knowledge of side-effects: essential information for clinicians.

Thus, the statement by Andrews that "the existence of proven treatments would normally render placebo trials unethical" is unwarranted. I believe it is unethical *not* to use placebo controlled trials even when there is a proven therapeutic method (since no method is perfect), so long as there can be no lasting harm from delaying treatment and the subjects fully understand the risks and voluntarily consent. I urge researchers and clinicians to press the World Medical Association to modify the latest version of the Declaration of Helsinki (World Medical Association, 2000), which contains this restriction on placebo controlled trials.

Andrews, G. (2001) Placebo response in depression: bane of research, boon to therapy. *British Journal of Psychiatry*, **178**, 192–194.

World Medical Association (2000) Declaration of Helsinki. Ethical principles for medical research involving human subjects. *Journal of the American Medical Association*, **284**, 3043–3045.

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Does size matter?

I commend the article by Weich *et al* (2001) examining the effects of income inequality on mental health. Given the importance of psychosocial factors in Wilkinson's (1996) thesis on inequality and health it is an important and long overdue contribution to this debate. Although this study was

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