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

J. Treasure Eating Disorder Research Unit, Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF, UK

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be based on a systematic review of the literature, which should include an exhaustive search for trials including those unpublished (grey data).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.

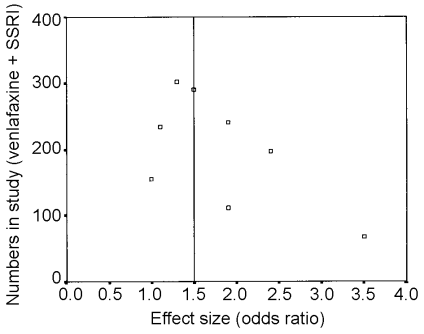

Fig. 1 Funnel plot of data from Thase *et al* (2001). The vertical line is the pooled effect size about which a symmetrical inverted funnel shape should appear.

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.

J. Warner Public Mental Health, Imperial College School of Medicine, Paterson Centre, 20 South Wharf Road, London W2 1PD, UK

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.

N. Bark Albert Einstein College of Medicine Schizophrenia Research Unit, Bronx Psychiatric Center, 1500 Waters Place, Bronx, New York 10962, USA

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

https://doi.org/10.1192/bjp.179.1.79 Published online by Cambridge University Press