

WEAVER, C.A. IIIrd. (1993) Do you need a "flash" to form a flashbulb memory? *Journal of Experimental Psychology: General*, 122, 39–46.

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Publication bias and meta-analysis

SIR: Piccinelli *et al* (1995) present a meta-analysis examining drug treatment in obsessive-compulsive disorder, which demonstrates the efficacy of anti-depressant drugs in treatment.

In the absence of large scale randomised control studies, we believe that such meta-analyses may be a valid method of obtaining useful information by pooling data from several smaller studies. However, the results need to be treated with caution since recent meta-analyses in other spheres of medicine have provided misleading results which have subsequently been refuted by large scale randomised control trials.

A recent investigation (Egger & Davey Smith, 1995) into one 'false positive' meta-analysis suggested the problem was publication bias in the studies available for pooling; small studies reporting a favourable result in treatment groups are more likely to be published. Publication bias can be demonstrated graphically by a 'funnel plot', which plots clinical effect against sample size. If there is no publication bias, then the plot resembles an inverted funnel, since the results from smaller studies are more widely but still symmetrically distributed than those of larger studies.

Piccinelli *et al* acknowledge that "Published clinical trials may be biased in favour of significant results, since trials failing to show any treatment difference may be less likely to be published". However, they do not test this possibility and since the sample sizes of the studies included were not presented, the critical reader cannot construct funnel plots from their data.

As it has been shown that such biased data can result in misleading results, we would suggest that sensitivity analyses such as funnel plots be included in all systematic reviews presented in the *BJP*. The science of systematic review remains an evolving one and if it is to retain its credibility then it should be seen to learn from its mistakes.

EGGER, M. & DAVEY, G. (1995) Misleading meta-analysis. *British Medical Journal*, 310, 752–754.

PICCINELLI, M., PINI, S., BELLANTUONO, C., *et al* (1995) Efficacy of drug treatments in obsessive-compulsive disorder; A meta-analytic review. *British Journal of Psychiatry*, 166, 424–443.

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Women's response to adversity

SIR: In their study of women's adjustment to adversity, Surtees (1995) appears to suggest that the experience of entering a women's refuge is an adverse event, comparable to the loss or threatened loss of a partner through death. They express surprise that women entering a Women's Aid refuge show a reduction in general indicators of distress unlike the other two groups who become more depressed and anxious.

The selection of women entering a refuge as a comparison group seems to completely misunderstand the nature of these women's experiences. Our recent research (Scott-Gliba *et al*, 1995) found that women in refuges have typically experienced years of violence, abuse and degradation before they take the decision to leave home. Compared with women who were not in violent relationships, they had high levels of depression, anxiety and post traumatic symptoms which decreased over time, when they were separated from their violent partner. For these women the traumatic event is not the loss of home or financial stability, but the terror that precedes it, to which the women have had to make complex behavioural and cognitive adjustments in order to survive. Although leaving a battering partner is, in itself, not without risks (violence typically escalates when faced with the prospect of abandonment) and may involve considerable material hardship, nevertheless the removal of constant threat for these women is beneficial for their emotional and mental well-being, restores their sense of self-respect and dignity and allows them to re-establish some control over their lives.

SCOTT-GLIBA, E., MINNE, C. & MEZEY, G.C. (1995) The psychological, behavioural and emotional impact of surviving an abusive relationship. *Journal of Forensic Psychiatry* (in press).

SURTEES, P.G. (1995) In the shadow of adversity: the evolution and resolution of anxiety and depressive disorder. *British Journal of Psychiatry*, 166, 583–594.

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Structured abstracts

SIR: Simpson & Baldwin (1995) describe a 71-year-old man who developed acute obsessive-compulsive disorder after a right parietal infarct. SPECT scanning revealed, in addition to the infarct, a diminution of cerebral blood flow in the right basal ganglia and temporal areas. The patient's clinical state improved on a combination of clomipramine and cognitive-behavioural psychotherapy.

While the case is of undoubted interest, it concerns me that the "Conclusions" section of the structured abstract states that: "SPECT is effective in the diagnosis of neuropsychiatric disorders such as OCD, and the pathological changes in brain metabolism detected by SPECT may be reversed by both drug therapy and psychotherapy". Clearly these conclusions, irrespective of their validity, cannot be supported by the data presented. Firstly, the description of SPECT changes in a single case of OCD hardly justifies the assertion that SPECT is "effective in the diagnosis"; OCD remains a clinical diagnosis, and whether pathognomonic blood flow changes will ever be demonstrable is highly questionable. Secondly, while it is true that the patient described improved clinically with treatment, it is unclear from a single case study whether this was because of the treatment offered (and if so, what elements were effective), or despite it. Thirdly, the authors do not report a repeat post-recovery SPECT to justify the conclusion that "changes in brain metabolism" were indeed reversed.

All this might appear pedantic, but it is imperative that conclusions are justified in terms of data presented. Given that many readers will confine themselves to a perusal of the abstract of many articles, it is doubly important that the conclusions in the abstract are accurate. The introduction of structured abstracts in the *BJP* is a welcome development, but the quality of the abstract is by no means ensured through having structure. I suggest reviewers been asked specifically to assess the coherence and validity of the structured abstracts in submitted papers.

SIMPSON, S. & BALDWIN, B. (1995) Neuropsychiatry and SPECT of an acute obsessive-compulsive syndrome patient. *British Journal of Psychiatry*, 166, 390-392.

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Psychosocial outcome of liver transplant

SIR: We read Collis *et al.*'s (1995) study with interest as we also published a study describing the psychosocial outcome of liver transplantation (Howard *et al.*, 1994). We undertook a study of similar cross-sectional design but some of our results differed appreciably from those of Collis *et al.*

Our study focused on the outcome of transplant recipients transplanted for end-stage alcoholic

cirrhosis ($n=20$). We interviewed subjects in detail extensively, but also administered several standardised questionnaires to these patients and other liver transplant patient controls ($n=54$). Our study found levels of psychiatric morbidity in both groups similar to those found by Collis *et al.* and we agree with their conclusion that the prevalence of psychiatric disorder in post-transplantation patients is comparable with that of general medical patients.

Although Collis *et al.* speculated that alcoholic patients may have a particularly high risk of psychiatric morbidity on the basis of their finding that three of four alcoholic patients were CIS (Clinical Interview Schedule) cases, we found no evidence of this. There was no significant difference between the alcoholic patients and transplant controls on any measure of psychosocial outcome except for a poorer perception of health status by the alcoholics, and only one alcoholic patient of 18 administered the CIS obtained a score compatible with psychiatric caseness. There was also no significant difference between the two groups on all measures of physical outcome. Only one other group has reported on the psychological outcome of alcoholic transplant patients (Beresford *et al.*, 1992) and they also found no difference in the prevalence of depressed mood between alcoholic patients and transplant controls (although no standardised measures were used in this study).

It is of interest that most alcoholic patients returned to regular alcohol drinking after a period of abstinence (usually of several months), but at lower levels than before the transplant.

Possible explanations for the different findings of Collis's study and ours include different selection criteria for transplantation, and differences in social support and demographic characteristics (such as social class), but we suspect that the longer period of follow-up in our study (our patients were assessed 1 to 6 years after transplantation) is most relevant. Quality of life is particularly impaired in the first year after transplantation compared with subsequent years (Lowe *et al.*, 1990).

We conclude that the evidence to date suggests that alcoholic liver transplant recipients do not have higher levels of psychiatric morbidity than other liver transplant patients but they remain vulnerable to alcohol abuse in the medium to long term. The need for specialist input from psychiatric and alcohol services should therefore be anticipated by transplant units and liaison psychiatrists if patients with alcohol problems continue to be transplanted in increasing numbers.