

## Correspondence

EDITED BY TOM FAHY

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### Duplicate publication

**Sir:** It is somewhat concerning that an article in the July issue of the *Journal* (Ghubash & Abouh-Saleh, 1997) appears to duplicate a recent publication by the same authors in *Acta Psychiatrica Scandinavica* (Abouh-Saleh & Ghubash, 1997). Both articles describe the administration of the Edinburgh Postnatal Depression Scale and the Self Report Questionnaire to 95 women admitted to the New Dubai Hospital, Dubai, for childbirth, from mid-July 1994 until the end of August 1994. The two papers report identical demographic information, methods and results; neither article cites the other.

'Redundant' or 'duplicate' publication is an issue of considerable importance in academic medicine (Shader & Greenblatt, 1996), yet is often overlooked, ignored, or dismissed as having little impact. On the contrary, as a 1995 editorial in the *New England Journal of Medicine* pointed out, "multiple reports of the same observations can overemphasize the importance of the findings, overburden busy reviewers, fill the medical literature with inconsequential material, and distort the academic reward system" (Kassirer & Angell, 1995). Such redundant publication of data may result in particularly significant inaccuracies in the psychiatric literature, since the difficulty of conducting research on our unique patient populations often leads to small sample sizes and unblinded studies. As a result, to a greater degree than in many medical specialities, we rely on the independent reproduction of research findings to assure ourselves of the validity of their conclusions.

Redundant publication is especially concerning in light of the current popularity of meta-analyses. If duplicated data are not recognised and excluded from the analysis, they will result in excessive weight being placed on the outcomes of those studies that have been repeatedly published, possi-

bly affecting the conclusion of the meta-analysis. Such a situation arose with a recent attempted meta-analysis of the efficacy of clozapine in affective disorders: several centres had each published multiple articles on overlapping data sets, and without knowing which patients participated in more than one study it proved impossible to perform even the simplest statistical analysis (Young *et al*, 1997). Such unfortunate scientific outcomes can only be avoided by the utmost integrity on the part of investigators.

**Abou-Saleh, M. T. & Ghubash, R. (1997)** The prevalence of early postpartum psychiatric morbidity in Dubai: a transcultural perspective. *Acta Psychiatrica Scandinavica*, **95**, 428-432.

**Ghubash, R. & Abou-Saleh, M. T. (1997)** Postpartum psychiatric illness in Arab culture: prevalence and psychosocial correlates. *British Journal of Psychiatry*, **171**, 65-68.

**Kassirer, J. P. & Angell, M. (1995)** Redundant publication: a reminder. *New England Journal of Medicine*, **333**, 449-450.

**Shader, R. I. & Greenblatt, D. J. (1996)** Twice may be too many: redundant publications. *Journal of Clinical Psychopharmacology*, **16**, 1-2.

**Young, C. R., Longhurst, J. G., Bowers, M. B., et al (1997)** The expanding indications for clozapine. *Journal of Clinical and Experimental Psychopharmacology*, **5**, 1-20.

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**Authors' reply:** We welcome the opportunity to reply to the concerns raised by Longhurst and Weiss. While we agree that the issue of redundant or duplicate publications is important and "often overlooked, ignored, or dismissed as having little impact", we firmly refute their allegation that this applies to our two articles. It concerns us that they should reach a summary verdict expressed in a perfunctory

statement that "the two papers report identical demographic information, methods and results". In these articles, we have reported the results of two studies on the 'blues' and postpartum depression using different instruments involving the same population with different results in the context of an extensive ongoing research programme on the epidemiology, risk factors, biology and outcome of postpartum psychiatric illness and its impact on child development.

The article published in *Acta Psychiatrica Scandinavica* reported the results of a cross-sectional study on the point prevalence and risk factors of early psychiatric morbidity 'blues' using two self-rated instruments; the Self Reporting Questionnaire (SRQ) on day 2 and the Edinburgh Postnatal Depression Scale (EPDS) on day 7 after childbirth.

The report in the *British Journal of Psychiatry*, however, was on the results of the period prevalence, incidence, diagnostic breakdown, risk factors and outcome of late postpartum psychiatric illness, predominantly depression, using the Present State Examination (PSE) at 8 and 32 weeks postpartum, in comparison with the results of a previously published community-based study using the PSE (Ghubash *et al*, 1992). The two papers were submitted at the same time and hence neither paper was cited by the other. Both papers, however, cite the validity study of the EPDS by the PSE (Ghubash *et al*, 1997).

The two studies showed different risk factors for early and late psychiatric morbidity: only two out of eight and four out of eight risk factors for early self-reported morbidity on the SRQ and EPDS, respectively, were also risk factors for late PSE-determined morbidity at week 8, and five out of nine risk factors for late morbidity were not significant for early morbidity. Moreover, for three out of four similar factors, the statistical differences were 0.1-0.01% for late morbidity versus a significant difference at the 5% level for early morbidity. Hence, the two publications reported different sets of data on postpartum morbidity using different time frames with established validity (i.e. the distinction between 'blues' and postpartum depression in relation to period of risk) and different instruments (self-rated versus observer-based). A meta-analysis by O'Hara & Swain (1996) of the results of studies of postpartum depression has clearly shown that these two factors have a significant