

and, where possible, there should be an independent assessment of eligibility and risk of bias by a second author with no conflict of interest.' Coleman did not obtain an independent assessment of the studies she authored or co-authored, nor did she acknowledge these conflicts in the review.

Coleman's conclusion that the results of the studies in her review are 'quite consistent' (p. 183) is belied by visual inspection of the Forest plots, which include non-overlapping confidence intervals. Coleman should have reported results of heterogeneity tests (chi-squared and  $I^2$ ), which probably would have shown significant heterogeneity in results across studies (presumably that is why she chose the random effects model).

Some of the commentaries on Coleman's review appear to be uninformed by current scientific standards for reviews. Comments by Fergusson *et al* are particularly misleading. Faced with variations in the methodological quality of available studies, it is essential for reviewers to weed out weaker studies. Valid conclusions can only be based on valid studies.

It is unclear how this paper got through peer review at the *Journal*. It appears that peer reviewers and the Editor ignored published standards for systematic reviews and meta-analyses. Given the serious methodological flaws contained in Coleman's review and the author's failure to report obvious conflicts of interest, we believe the article should be retracted.

- 1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.
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Priscilla Coleman's recent meta-analysis<sup>1</sup> ignores guidelines for proper scientific conduct of meta-analyses of observational data. Her results violate at least three major principles of meta-analysis: she fails to assess the underlying validity of included studies; she fails to examine statistical heterogeneity; and she illogically combines estimates for distinct outcomes. Furthermore, she accuses previous reviews of lacking 'reasonable justification' for declining to quantitatively summarise effects, when declining to do so actually reflected sound epidemiological judgement.

Coleman contends that 'Through a process of systematically combining the quantitative results from numerous studies addressing the same basic question ... far more reliable results are produced than from particular studies that are limited in size and scope'. However, expert consensus suggests that 'the likelihood that the treatment effect reported in a systematic review

approximates the truth depends on the validity of the included studies...'<sup>2</sup> Coleman fails to assess the validity of included studies and erroneously asserts that 'as a methodology wherein studies are weighted based on objective scientific criteria, meta-analysis offers a logical, more objective alternative to qualitative reviews...'. In fact, studies in meta-analyses are typically weighted by sample size, which is not always related to study quality,<sup>3</sup> and decisions on which studies to include and how to include them remain subjective. If poor-quality studies are included, as occurred in Coleman's review, a poor-quality quantitative estimate will be generated. Coleman combines statistically heterogeneous results, and illogically combines effect estimates for outcomes that vary substantially (i.e. marijuana use and suicide), thus generating a summary estimate void of meaning or utility.

Meta-analysis of observational data can be useful when carefully conducted. However, it is essential that a summary estimate be accompanied by a qualitative description of risk of bias in included studies (which Coleman's review lacked) since 'potential biases in the original studies, relative to biases in RCTs, make the calculation of a single summary estimate of effect of exposure potentially misleading'.<sup>4</sup>

Coleman ignores other essential requirements of a high-quality statistical meta-analysis.<sup>2</sup> She makes no attempt to present a replicable search strategy or article selection diagram. She attempts to justify excluding articles prior to 1995 by noting that study methodology has improved, but fails to adequately justify selected cut-off dates. Ultimately, she includes multiple methodologically weak studies, and excludes at least two older but methodologically stronger studies. She authored her review alone, despite Cochrane and PRISMA recommendations to involve multiple reviewers to reduce the possibility of investigator bias or error.<sup>2,5</sup>

Coleman makes disingenuous accusations about previous reviews. For example, she claims that our 2008 systematic review<sup>6</sup> 'overlooked' ten articles which met inclusion criteria, and 'lacked sufficient methodologically based selection criteria'. This unfounded attack is puzzling, particularly since in 2008, we directly emailed to Coleman the reasons (consistent with our methodologically based selection criteria detailed on p.437) for excluding seven of these ten articles. The remaining three (not previously enquired about) also fail to meet inclusion criteria: two had a follow-up period of less than 90 days and the other compared medical *v.* surgical termination.

Coleman continues to ignore the scientific importance of accounting for pregnancy intention in this body of literature. If women who abort (many of which are unintended pregnancies) are compared against women who deliver (many of which are intended pregnancies), effects of unintended pregnancy are difficult to disentangle from effects of abortion. Circumstances surrounding an intentional *v.* an unintentional conception or pregnancy may be related to mental health outcomes. Most aborted pregnancies in the USA were unintended.<sup>7</sup> Coleman wrongly assumes that since nearly half of pregnancies in the USA are unintended, most births are too, failing to acknowledge that almost half of unintended pregnancies end in abortion.<sup>8</sup> Thus, her assertion that 'the majority of women in the control groups in studies comparing abortion with term pregnancy actually delivered unintended pregnancies even if the variable was not directly assessed' has no empirical grounding. Similarly, her assertion that a 'no pregnancy' group may be a 'cleaner' comparison group ignores the fact that the 'no pregnancy' group would not have experienced unintended pregnancy.

The scientific validity and rigour of Priscilla Coleman's work has been questioned before.<sup>9</sup> However, we are surprised and disappointed that the multiple egregious scientific errors in her

review went undetected by the editorial or peer-review process of the *British Journal of Psychiatry*.

- 1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.
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Professor Coleman's systematic review and meta-analysis of the literature on termination of pregnancy and mental health<sup>1</sup> featured several significant omissions: an undisclosed conflict of interest; no assessment of publication bias; and no assessment of the quality of studies included. The search strategy was also inadequately reported, and the meta-analytic technique was faulty.

First, the paper states 'Declaration of interest: None'. We believe this is incorrect. It seems that Professor Coleman is an anti-abortion campaigner, who has previously expressed the view that campaigning should include work in academic journals. For example, in a Powerpoint presentation on the website of the American Association of Pro-Life Obstetricians and Gynecologists, Professor Coleman states:

'We need to develop organized research communities to continue the research, apply for grants, recruit young academics, critique data produced by pro-choice researchers, challenge politically biased professional organizations, train experts to testify, and disseminate cohesive summaries of evidence.'<sup>2</sup>

The *British Journal of Psychiatry* has committed to the International Committee of Medical Journal Editors' uniform requirements for declaration of conflict of interest. This requires the declaration of 'any relevant non-financial associations or interests (personal, professional, political, institutional, religious, or other) that a reasonable reader would want to know about in relation to the submitted work'.<sup>3</sup> As noted in a recent editorial, 'the difficult words here are "personal", "relevant" and "reasonable"'.<sup>4</sup> Given the role that campaigning has played in this issue, we believe this conflict of interest should have been declared to readers.

Second, unusually for a systematic review and meta-analysis, there was no attempt to account for the role of publication bias in the findings. We have replicated the meta-analysis by importing Coleman's data into Stata 11. After verifying that the summary odds ratios and confidence intervals produced were identical, we went on to create a funnel plot (Fig. 1) using metafunnel. This

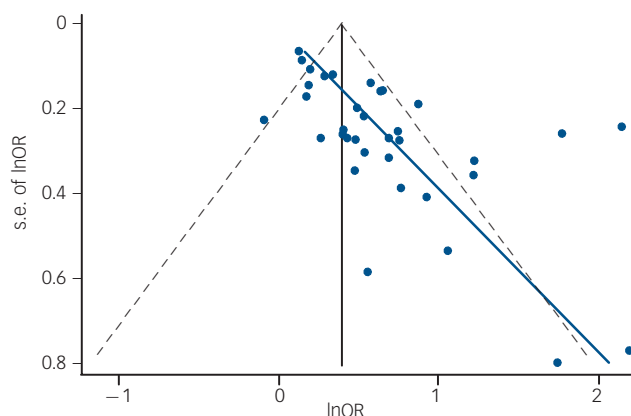


Fig. 1 Funnel plot examining publication bias data presented by Coleman.<sup>1</sup>

found evidence strongly suggestive of publication bias in the literature presented. We further used Egger's test using the metabias command in Stata 11, and again found very strong evidence suggesting publication bias ( $P < 0.0001$ ).

Third, we are concerned to note that there was no attempt to account for quality of evidence, since a previous systematic review and meta-analysis found strong evidence for a relationship between methodological rigour and study results:

'The highest quality studies had findings that were mostly neutral, suggesting few, if any, differences between women who had abortions and their respective comparison groups in terms of mental health sequelae. Conversely, studies with the most flawed methodology found negative mental health sequelae of abortion.'<sup>5</sup>

Finally, we note that there was only one assessor for the studies, and several of the included studies had more than one outcome, which were used in the meta-analysis as if they were independent observations.

We believe that as a result of these features the paper falls far short of best practice in the execution of publication-standard meta-analyses.

Declaration of interest

B.G. writes newspaper articles and books on problems in science, and has written three times previously about flaws in evidence used to campaign for changes in UK legislation to reduce access to termination of pregnancy, once online and twice in print, out of approximately 2000 pieces published. Neither author is religious, neither has a history of engaging on the issue of termination of pregnancy beyond that mentioned here.

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