

## Correspondence

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### Abortion and mental health: guidelines for proper scientific conduct ignored

We have serious concerns about the methodology of the quantitative synthesis by Coleman<sup>1</sup> and want to highlight these to prevent readers and policy makers drawing erroneous conclusions, in particular the incorrect statement that ‘nearly 10% of the incidence of mental health problems was shown to be attributable to abortion’.

This quantitative synthesis and meta-analysis did not follow the robust methodologies now generally accepted for systematic reviews.<sup>2</sup> There is no detail of the search strategy including search terms; the strategy is not comprehensive (only two databases included); other strategies to search the literature, including citation tracking, hand searching and contacting authors and experts in the field to try to minimise publication bias, were not carried out; and there was no assessment or rating of the quality of included studies, so that only those of at least reasonable quality are included in the meta-analysis. This is particularly important here as many of the primary studies included in this review have significant methodological limitations, including non-prospective design, non-standardised measures of mental disorders, lack of adjustment for pre-existing mental illness, lack of adjustment for other key confounders (e.g. social deprivation), non-comparability of exposed and non-exposed groups, and selection bias. This is especially concerning, given that previous reviews raised serious methodological concerns about some of the included studies, and came to different conclusions when these were excluded from analyses.<sup>3–5</sup> Furthermore, results from several of the included studies linking abortion to mental health problems have since been re-analysed by other researchers. These studies, using the same data, have less biased sample selection techniques and control for pre-pregnancy factors known to influence poor mental health outcomes (i.e. rape history) and have found no significant links between abortion and subsequent poor mental health.<sup>6,7</sup>

A recent population-based cohort study conducted in Denmark published in the *New England Journal of Medicine* this year confirmed this. Munk-Olsen *et al*<sup>8</sup> reported no difference between the incidence of first psychiatric contact before and after abortion. Importantly, the incidence of psychiatric contact is higher among women who underwent abortion; this is the result of a selection phenomenon and not a causal association because this relationship is evident before the abortion occurred.<sup>8</sup> This is evidence that women having induced abortions constitute a population with higher pre-existing psychiatric morbidity.

Study selection and evaluation should be carried out by two independent raters; the Coleman review was carried out by one author only. Of the 22 papers included, 11 were authored by Coleman, the author of the review. This is a conflict of interest,

and undermines the author’s ability to critically review the primary studies.

Finally, the synthesis of the data and the summary statistics are flawed. The criteria for synthesising data meant that several effect measures were included from the same study. Eleven of the included studies contributed more than one effect measure, with two studies contributing four measures each. Despite the clustering of effect measures by study, they are analysed as independent measures. This is an important limitation, since the use of several effect measures from a flawed study can magnify the bias.

Most importantly for readers of this study to know, is the erroneous conclusions drawn by the author regarding the population attributable risk (PAR). The underlying assumptions for estimating PAR include that there is a causal relationship between the risk factor (abortion) and the disease (mental ill health) and that there is independence of the considered risk factor from other factors that influence disease risk.<sup>9</sup> These assumptions are clearly not met in this review and therefore it is completely inappropriate to calculate a PAR from these data.

Abortion and mental health is a politicised issue – it is therefore essential that research in this field is methodologically robust.

## Declaration of interest

T.M.-O. was the lead author of the population-based cohort study cited in this letter.

- 1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.
- 2 Stroup DF, Berlin JA, Morton SC, Olkin I, Williamson GD, Rennie D, et al. Meta-analysis of observational studies in epidemiology: a proposal for reporting. *JAMA* 2000; **283**: 2008–12.
- 3 Major B, Appelbaum M, Beckman L, Dutton MA, Russo NF, West C. *Report of the APA Task Force on Mental Health and Abortion*. American Psychological Association, 2008.
- 4 Charles VE, Polis CB, Sridhara SK, Blum RW. Abortion and long-term mental health outcomes: a systematic review of the evidence. *Contraception* 2008; **78**: 436–50.
- 5 Robinson GE, Stotland NL, Russo NF, Lang JA, Occhiogrosso M. Is there an ‘abortion trauma syndrome’? Critiquing the evidence. *Harv Rev Psychiatry* 2009; **17**: 268–90.
- 6 Schmiede S, Russo NF. Depression and unwanted first pregnancy: longitudinal cohort study. *BMJ* 2005; **331**: 1303.
- 7 Steinberg JR, Russo NF. Abortion and anxiety: what’s the relationship? *Soc Sci Med* 2008; **67**: 238–52.
- 8 Munk-Olsen T, Laursen TM, Pedersen CB, Lidegaard Ø, Mortensen PB. Induced first-trimester abortion and risk of mental disorder. *N Engl J Med* 2011; **364**: 332–9.
- 9 Rockhill B, Newman B, Weinberg C. Use and misuse of population attributable fractions. *Am J Public Health* 1998; **88**: 15–9.

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doi: 10.1192/bjp.200.1.74

There appear to be many methodological as well as logical inconsistencies and interpretational difficulties with the report of Coleman,<sup>1</sup> which might have diminished reviewers’ enthusiasm for its conclusions. Many of these have already been addressed by previous correspondence. We believe, however, that one methodological problem that has not yet been raised – the use of the population attributable risk (PAR) measure – is very important and merits comment. This might help readers avoid misunderstanding this study, and also other studies where the PAR is used.

In Coleman's synthesis, the PAR measure has been applied inappropriately and, we believe, reported misleadingly. For example, the reported PAR for completed suicide is particularly high at 35%. For several reasons, readers should not interpret this figure as meaning that over a third of all suicides among women of reproductive age could be prevented if none of them underwent abortion. An inherent assumption in the PAR is that all other things would remain equal after the removal of a risk factor, which is clearly not true for abortion in this instance. Further, the aetiology of suicide is extremely complex, and in most cases cannot be attributed to a single adverse life event that is the one measured in a particular study. Women who die by suicide at some time following an abortion are likely to carry multiple distal and proximal risk factors as they proceed along their life course, as is true for most people of any age or gender who die by suicide, and it is fallacious to suggest that abortion can be isolated from other causal factors in these limited data-sets.

Second, in the first paragraph of the Discussion (p.183), Coleman states with apparent certainty that '...nearly 10% of the incidence of mental health problems was shown to be directly attributable to abortion.' This is about as unambiguous a statement of causality as could possibly be made, in the face of clear guidance on the potential pitfalls of drawing such conclusions when applying the PAR.<sup>2</sup> Having stated the causality of the association with such certainty, the author then appears to backtrack in her concluding remarks (pp. 185–186) by making the following ambiguous statement, clearly contradicting the view expressed at the start of her Discussion:

'Although an answer to the causal question is not readily discerned based on the data available, as more prospective studies with numerous controls are being published, indirect evidence for a causal connection is beginning to emerge.'

Following publication of just such a 'prospective study with numerous controls' in the *New England Journal of Medicine* in 2011,<sup>3</sup> it might be appropriate for Priscilla Coleman (and colleagues supportive of her views) to reconsider their conclusions. This recent study<sup>3</sup> provides the best data available from the largest unbiased sample on the association (or lack thereof) between excess risk of mental illness and abortion because that study is based on a large population sample, with measurement of mental illness both before and after the abortion event study.<sup>3</sup> That study 'does not support the hypothesis that there is an increased risk of mental disorders after a first-trimester induced abortion' (quoted from abstract).

- 1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.
- 2 Rockhill B, Newman B, Weinberg C. Use and misuse of population attributable fractions. *Am J Public Health* 1998; **88**: 15–9.
- 3 Munk-Olsen T, Laursen TM, Pedersen CB, Lidegaard Ø, Mortensen PB. Induced first-trimester abortion and risk of mental disorder. *N Engl J Med* 2011; **364**: 332–9.

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doi: 10.1192/bjp.200.1.74a

In her review of research on the mental health effects of abortion, Coleman<sup>1</sup> stated:

'In this highly politicised area of research it is imperative for researchers to apply scientifically based evaluation standards in a systematic, unbiased manner when synthesising and critiquing research findings. If not, authors open themselves up to accusations of shifting standards based on conclusions aligned with a particular

political viewpoint. Moreover, the results may be dangerously misleading and result in misinformation guiding the practice of abortion.'

However, Coleman failed to follow well-accepted scientific standards for the conduct and reporting of systematic reviews and meta-analyses. Further, Coleman's failure to state her obvious conflicts of interest in this review raises serious questions about biases in her analysis. Hence, the review is open to serious questions about the author's scientific standards, methods, political viewpoints, and potentially misleading conclusions.

Widely accepted standards for systematic reviews and meta-analyses are contained in the published AMSTAR, MARS, MOOSE, and PRISMA statements.<sup>2–5</sup> None of these standards were cited or followed by Coleman. AMSTAR is the only validated instrument for assessing the methodological quality of systematic reviews and meta-analysis. We assessed Coleman's review according to the AMSTAR statement, and found that it failed to meet any of the eleven basic requirements for systematic reviews and meta-analysis included in AMSTAR.

Following AMSTAR, specific flaws of the Coleman review are as follows:

- 1 there was no public *a priori* design
- 2 there was no duplicate study selection or duplicate data extraction
- 3 the author did not describe the search strategy in sufficient detail
- 4 the review was limited to published studies, contrary to all published standards
- 5 a list of excluded studies was not provided
- 6 the author did not provide sufficient descriptive information on included studies, including demographic characteristics of participants
- 7 the scientific quality of included studies was not documented
- 8 scientific quality of included studies was not considered in formulating conclusions
- 9 appropriate methods were not used in combining the findings of studies (Coleman clearly violated the rule for avoiding dependencies in meta-analysis, when she synthesised 36 effects from 22 studies in Fig. 1)
- 10 the likelihood of publication bias was not assessed
- 11 conflicts of interest and sources of support were not acknowledged (no financial disclosures were made and no other potential conflicts were acknowledged).

An article in the *British Journal of Psychiatry*<sup>6</sup> calls attention to the importance of non-financial conflicts of interest in the psychiatric literature. Coleman has at least two types of conflict of interest here. Among the most important of such conflicts is an agenda-driven bias, by which authors seek to influence legislation and social policy. David Reardon is a co-author with Coleman on seven articles included in the review and an author on an additional study in the review that does not involve Coleman as a co-author. Reardon is quite explicit about his agenda to instil fear of abortion as a way of facilitating passage of anti-abortion legislation.<sup>7</sup>

Coleman is the first author on 6 studies and co-author on 5 additional studies in her review; thus, she authored or co-authored fully half of the 22 studies included. According to the *Cochrane Handbook*,<sup>8</sup> this is another potential conflict of interest, since it may 'unduly influence judgements made in a review (concerning, for example, the inclusion or exclusion of studies, assessments of the risk of bias in included studies or the interpretation of results) ... This should be disclosed in the review