Coleman presents her conclusions as 'an unbiased, quantitative analysis of the best available evidence' concerning the adverse mental health consequences of abortion.[1] Huge numbers of papers by respectable researchers that have not found negative mental health consequences are ignored without comment. Not surprisingly, over 50% of the 'acceptable' studies she uses as her 'evidence' are those done by her and her colleagues Cougle and Reardon. The work of this group has been soundly critiqued not just by us[2,3] but by many others as being logically inconsistent and substantially inflated by faulty methodologies. As noted by the Royal Society of Obstetricians and Gynaecologists,[4] the authors consistently fail to differentiate between an association and a causal relationship and repeatedly fail to control for pre-existing mental health problems. We note that Coleman did not include in her articles the publication by Munk-Olsen *et al* in the January 2011 *New England Journal of Medicine*,[5] which concluded that

'the rates of a first-time psychiatric contact before and after a first-trimester induced abortion are similar. This finding does not support the hypothesis that there is an overall increased risk of mental disorders after first-trimester induced abortion'.

Indeed, the draft position statement of the Royal College of Psychiatrists concludes that when researchers control for wantedness of the pregnancy and pre-existing mental health problems, there is no increase in mental health disorders following an abortion. That same document, currently being finalised, is very critical of the methodology of the studies by Coleman and her colleagues. The 'unbiased nature' of most of the studies Coleman has used in her analysis and the Declaration of interest stated as being 'none' must be taken with a large grain of salt. Reardon, the leader of this group, has clearly expressed his rhetorical strategy as 'we can convince many of those who do not see abortion to be a "serious moral evil" that they should support anti-abortion policies that protect women and reduce abortion rates'.[6] He has stated that 'I do argue that because abortion is evil, we can expect, and can even know, that it will harm those who participate in it. Nothing good comes from evil'.[7] These authors have a clear agenda and publish a steady stream of papers, based on faulty methodology, designed to prove their point. If we and other researchers know this, how is it that reviewers for esteemed journals such as yours consistently fail to recognise these deficiencies and biases?


1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.

2 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.

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 Royal College of Obstetricians and Gynecologists. RCOG statement on BJPsych paper on mental health risks and abortion. RCOG, 1 September. 2011.

5 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.

6 Reardon DC. A defense of the neglected rhetorical strategy (NRS). *Ethics Med* 2002; **18**: 23–32.

7 Reardon DC. A defense of the neglected rhetorical strategy (NRS). In *Proceedings of the University Faculty for Life Conferences, Life and Learning XII – 2002*: 77–96. University Faculty for Life, 2003 (http://uffl.org/vol12/contents12.htm).



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We strongly disagree with the conclusions of Coleman's analysis of research about the relation between abortion and mental health.[1] An earlier study by Munk-Olson *et al*,[2] not mentioned in the study, concluded that, contrary to what is generally assumed, a first-trimester induced abortion was not followed by an increase in mental disorders. The strength of the study is that mental health problems are studied in women before and after an induced abortion, and not only after. From Dutch primary care data,[3] we can confirm this: in a case–control study in family practice, we compared the medical history of women 3 years before and 3 years after they had an induced abortion with a control group.[4] Differences were found with regard to mental health (visits for mental health problems, psychopharmaceutical prescriptions or referrals to mental health facilities). However, compared with the control group, women who had an induced abortion had more social problems. This should be an important focus of attention in the care of women who choose to have an abortion.


1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.

2 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.

3 van Weel C. The Continuous Morbidity Registration Nijmegen: background and history of a Dutch general practice database. *Eur J Gen Pract* 2008; **14**: 5–12.

4 Kooistra PAA, Vastbinder MB, Lagro-Janssen ALM. No increase in medical consumption in general practice after induced abortion. *Ned Tijdschr Geneeskd* 2007; **151**: 409–13.



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The study by Coleman[1] and the following comments may offer a further useful point of view to the bioethical debate. Irrespectively of moral judgement, in the majority of cases abortion is performed by physicians to protect women's mental health from an unintended/unwanted pregnancy or birth, but as a minimum what we can say is that evidence does not support any beneficial effect on women's mental health as a result of having an abortion. On the public health level, abortion may therefore be considered no more than a procedure satisfying criteria for futility.[2,3] On the individual level, any abortive procedure should be instead preceded by an in-depth analysis of the various factors known to interfere with the psychological outcomes. But as far as we know this is almost never the case. If women's health is what abortion providers intend to preserve, they should accept a substantial revision of their protocols under the assistance of skilled psychiatrists.


1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.

2 Schneiderman LJ, Jecker NS, Jonsen AR. Medical futility: its meaning and ethical implications. *Ann Intern Med* 1990; **112**: 949–54.

3 Waisel DB, Truog RD. The cardiopulmonary resuscitation-not-indicated order: futility revisited. *Ann Intern Med* 1995; **122**: 304–8.



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**Editors' response:** The article by Coleman[1] was submitted in October 2010 and accepted for publication in March 2011, so predated the Munk-Olsen paper,[2] as Coleman has indicated in



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her reply below. The handling editor was W.W. and the paper was accepted after revision with two reviewers supporting publication and one recommending rejection. It was recognised that the paper was likely to attract attention and P.T. suggested that a commentary should be published alongside the article. Unfortunately the major concurrent work on this subject (commissioned by the Department of Health) had not then been completed and it was felt unfair to delay publication, so the article appeared without comment. Dr Coleman stated that she had no conflicts of interest to declare and when invited to revise this view subsequently when reminded of our guidance again reiterated this. She has again defended this in her letter; readers are free in the light of these full statements to come to their own conclusions. The failure to declare an interest is not a reason for retracting a systematic review even if failure was unequivocally demonstrated, and this situation is very different from other ones in which the publication of a paper has been retracted.[3] We have nevertheless decided to give new guidance for the preparation of reviews in our authors' instructions so there is greater clarity for both authors and reviewers. The correspondence and commentary in this issue indicates the importance of the subject and the value of an active correspondence column in a journal; it is not a reason to avoid the publication of a controversial subject.


1 Coleman PK. Abortion and mental health: quantitative synthesis and analysis of research published 1995–2009. *Br J Psychiatry* 2011; **199**: 180–6.

2 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.

3 Horton R. MMR – responding to retraction. *Lancet* 2004; **363**: 1328.



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**Author's reply:** In the barrage of recent letters, the sentiments have varied widely and the many supportive arguments presented are worthy of additional comment; however, given space limitations, I have decided to focus on the criticisms to help ensure the results are given the attention deserved.

There are some comments that I believe are without basis and may not have been made with a more careful, less emotional read of the article. For example, Littell & Coyne suggested that scientific standards for systematic reviews were not followed. The protocol employed is detailed in the methodology section and the strategy was in line with recommendations in the *Handbook of Research Synthesis and Meta-Analysis*. Rather than focus on these types of comments, I address criticisms requiring more information from me to allow readers to make informed decisions regarding the merit of the issues raised.

The studies included in the meta-analysis have a relatively high degree of heterogeneity given the demographic and cultural differences in sampling, the variability in control groups and outcomes, and differences in third variable controls. Counter to the claim by Polis *et al*, heterogeneity was addressed by employing a random effects model. The random effects model yields an estimate of the mean of a distribution of true effects; whereas in the fixed effects model there is an assumption that all the included studies share one common effect. When assigning weights to studies in a fixed effects model, the smaller ones are afforded less importance, since the same effect is believed to be more precisely assessed in the larger studies. In contrast, in the random effects model, individual studies of varying sizes contribute data from distinct populations, all of which must be considered in the pooled estimate. Weighting is therefore more balanced in the random effects *v.* fixed effects model, with smaller studies given relatively more emphasis. In recognition of the heterogeneity, I not only employed the random effects model, but I ran separate meta-analyses based on distinct comparison groups and outcomes.

Goldacre & Lee provided a funnel plot analysis and presented it as evidence of publication bias. However, the funnel plot is largely inappropriate for heterogeneous meta-analyses, wherein studies are not likely from a single underlying population,[1–4] and several investigators have warned that use of funnel plots with meta-analyses derived from heterogeneous samples may result in false-positive claims of publication bias.[1–4] When funnel plot asymmetry is detected in a heterogeneous meta-analysis, the cause is likely to be essential differences between the smaller and larger studies. For example, the majority of the smaller studies included in my meta-analysis employed substance use outcome variables and these outcomes tend to yield the strongest, most robust effects.[5,6] In addition, the larger studies were more likely than the smaller studies to include actual diagnoses for disorders, rarer events than cut-off scores on single surveys. In the context of this meta-analysis, the funnel plot most certainly does not provide evidence of publication bias.

My experience attempting to locate unpublished data/studies on abortion and mental health has been very disheartening over the past 15 years, with virtually all requests ignored. I suspect that reluctance to share unpublished data is an attempt to keep results that challenge contemporary views on abortion and indicate significant increased risks for adverse psychological effects out of the public domain. In contrast, I believe energy is likely invested in seeing to it that non-significant findings, suggesting abortion carries no increased psychological risks, find their way into the journals. If there is any topic wherein many editors, researchers and professional organisations are highly motivated to publish non-significant effects, it is this one, rendering publication bias less common than in other areas. Support for this notion can be found in the American Psychological Association's (APA's) 42-year history of abortion advocacy.

In 1969, the APA passed a resolution which made the pro-choice political position the organisation's official stance and declared abortion a civil right. For decades the APA has aligned itself with major organisations with pro-choice social agendas, frequently submitting amicus briefs and providing congressional testimony. Martel[7] recently discussed the APA's position on abortion, among other issues, noting that the organization's stance has led them to promote psychological research and disseminate data to lawmakers to inform the public and advocate for societal change. Martel further pointed out that the political stance of the APA lacks the strong backing of empirical data. With this long history of abortion advocacy by the strongest professional psychology organisation in the world, politically motivated efforts to publish null findings to support and legitimise their position is logical.

As indicated under the methodology section of the meta-analysis, studies identified using the Medline and PsycINFO databases were included based on sample size, comparison groups, outcome variables, controls for third variables, use of odds ratios, and publication in English in peer-reviewed journals between 1995 and 2009. In an effort to isolate the effect of abortion on mental health, use of comparisons groups and controls for third variables are basic methodological requirements consistent with the Bradford Hill criteria.[8] The majority of studies meeting these criteria and incorporated into the meta-analysis also had many other strong methodological features (multiple data points, nationally representative samples, etc.). I purposely avoided selecting from among the many more peripheral methodological criteria that could be argued as a necessary basis for including or excluding studies, when there is not universal agreement regarding