NCCMH – Royal College of Psychiatrists

Induced Abortion and Mental Health

A systematic review of the mental health impact of induced abortion

Consultation comments

NAME OF ORGANISATION (if relevant)	**CHRISTIAN MEDICAL FELLOWSHIP**
FULL NAME * (of person submitting comments)	Philippa Taylor, Head of Public Policy, CMF

*Personal names will not be made public

PAGE number	LINE number	SECTION number	COMMENTS Please insert each new comment in a new row
11	**12**	**1.3.1**	We welcome this timely and important review. We consider that this is more robust than other reviews and agree with the review group that the approaches of The APA Task Force on Mental Health and Abortion, 2008 and the Charles Review 2008 had many limitations. However we do note that there have been several other literature reviews that are not even mentioned, which are also recent. For example, Coleman et al 2005, Coleman et al 2006 and Thorp et al 2003. It would surely be appropriate to have cited and evaluated others, particularly since the APA and Charles review both have significant limitations. Generally however we concur that the scientific standards of studies in this area is, in general, poor.
15	**36**	1.3.3	The APA and Charles Reviews both have many limitations, as noted in the review. We add a further concern, regarding the factors associated with mental health problems. Clearly, the results will depend on what factors you choose to look for: if one starts with different factors and therefore questions, different outcomes may well be obtained.
17	**38-39**	**2.1**	Three members (Tahir Mahmood, Claudette Thompson and Lisa Westall) were also on the RCOG consultation on '*The Care of Women requesting Induced Abortion'*. This was very selective with the evidence it collated and the conclusions it drew, suggesting that there are very few adverse effects of abortion on women. We are concerned that no members have been required to declare neutrality on the topic and, in the light of the conclusions of the recent draft of the RGOG report, and given that they were members of it, these three may not be entirely neutral. Moreover, the review Chair, Dr Roch Cantwell, and Dr Ian Jones both stated in a commentary in 2008 that: "*Informed consent for surgery does not include a warning of psychological hazard. We do not believe that the evidence is strong enough to support mandating such advice for abortion*." It could therefore be argued that both may hold predetermined positions on this issue. No reassurance is provided as to whether or not this remains their position as there is no statement of neutrality offered. Dr Ian Jones was also involved in the Munk-Olsen 2011 report, reading and commenting on an early draft. Given the reliance on this research paper in the review and its conclusion that there is no increased risk of mental disorders after a first-trimester abortion, we question the lack of publicly declared neutrality by all the authors of the review.

18	**24**	**2.3**	There is an attempt at transparency, consistency and rigour in analysis. Nevertheless, we have many concerns with both the data and conclusions.
18	**14-16**	**2.2**	The report has two wordings for Question 3, which are different. The question on p65, line 9-11 is different to p18, line 14-16. Considering it is one of the three key questions under consideration this inconsistency reflects sloppiness in report writing. P87 uses the same wording as p18. Section 5 of the review answers the question on p65, so we suggest that for consistency the p18 and p87 wording should be amended to match that on p65. Or answer both. More importantly, the two **answers produced could be different:** The answer to the p65 question would be yes, based on the evidence presented in this review. The p18 question is harder to answer and depends on 'wantedness' which is not only very difficult to measure difficult to measure but is subjective and may change at any point throughout pregnancy and therefore should only be used with caution. A pregnancy can begin as unwanted, or woman may be ambivalent to begin with, particularly if unplanned, but then become wanted. The influence of a partner and family members can also affect 'wantedness'. Assumptions cannot be made, this issue needs a direct question to the woman about whether the pregnancy is wanted, ideally at different stages of it. It is the question on p65 that is the important question to answer, and more measurable.
19	**6**	**2.3**	By limiting the criteria used for inclusion of research in this study to only those that measure outcomes occurring more than 90 days post-abortion excludes a large body of evidence and literature on mental health disorders (see for example our comment on p45 below). For example, for just one review question this accounted for 27 studies being excluded (p27). While there is some evidence that mental health may improve in the short term after abortion, there are also many women who suffer mental health disorders in the two months post-abortion, and this group is excluded. Outcomes will vary with time post-abortion. The limitations of these inclusion criteria and the evidence that the consultation cannot therefore consider should be made clear.
19		**2.3**	One problem with measurement is that many people with mental illness do not seek treatment. Women who have negative reactions to abortion are less likely to return to the clinic. The eligibility criteria therefore will be likely to have excluded many women who do not return to the health professionals who were involved in the abortion process. Poor follow up post-abortion compounds this problem. Clearly this would underestimate prevalence of mental health disorders. Compounding this, women delivering will be more likely to have regular contact with health professionals than those having a termination and so a higher reporting of their mental health problems will be likely, again introducing a bias in the groups.
19	**30**	**2.3**	Note our comments at the end of this form and those on p33. The lack of UK based studies highlights the urgent need for linkage-based studies in the UK. Population-linked longitudinal data in England is not available. Every termination provider should routinely be required to record the patient NHS number. This data is needed urgently in order to test the UK evidence of sequelae from abortion and thus to enable future longitudinal studies of patient outcome.

21	**19-48**	**2.6**	As our concerns on p23 below suggest, there is insufficient information and transparency provided on how the NICE guidelines were applied to specific research studies and what scores were given.
21-22	**35-48**	**2.6**	Despite the shortcomings of the Charles review, this was used as the basis for rating the research. The shortcomings and subjectivity of using the Charles criteria need to be acknowledged and taken into account in the review. For example, the Charles criteria ignore several key elements, such as ranking for high drop out rates and non participation. This can clearly bias results.
23	**26-32**	**2.8**	Although this is not an entirely unusual rate of exclusion of studies, **there is a concerning lack of transparency in the inclusion and exclusion process**. The authors exclude studies if they do not contain 'useable data' or did not use a 'validated measure of mental health' but they fail to explain what these actually constitute. **There is insufficient transparency regarding the reasons for excluding hundreds of peer-reviewed studies**, many of which may have failed in just one or two criteria but could still provide useful findings.
23	**35-37**	**2.8**	While it is reasonable in terms of resource availability to do a systematic review that only includes papers published in English, it is noteworthy that, consequently, papers not published in English are excluded which may well introduce bias.
23	**41**	**2.9**	It is not possible to compare all the selected data that is used in the text with the original papers because the data extraction tables have not been included. It would have been helpful if the authors had included the data extraction tool and the data extracted from the original studies. **These data tables should be provided**. Since the data selection used is not fully transparent we are unable to verify all the analysis, **leaving some of the analysis more open to question.**
24-25	**41**	**2.10**	Similarly, it is **not clear on what basis the gradings were made for quality**. Which criteria were more important than others? How did the reviewers reach conclusions about the quality of studies? Which criteria were met or not met? For example, we are concerned that Fergusson 2009 is rated as 'fair' while Steinberg study 2 is rated as 'very good', which is different to previous reviews. Fergusson 2009 (and 2008) is a longitudinal study, a primary analysis and controls well for confounders. In contrast, Steinberg 2008 study 2 is a secondary analysis, it is cross-sectional and it uses data from a pre-existing database. It should not be graded as very good. More justification and transparency on the ratings is necessary here.
25-26	**47-48**	**2.11**	Although this review uses well-recognised methodological analysis, 'evidence' can come in many forms. Considering the research evidence is still poor, and given the limitations in quantitative research, the views, and experiences of women, clinicians and other experts should be consulted along with the statutory organisations and relevant Royal Colleges. Their voice should therefore provide an important source of 'evidence' and should contribute to the review. Fully randomised trials on abortion are clearly unethical to carry out, therefore qualitative studies should have a place in the review. Of course qualitative research is not easy either and the researcher's prior position can be an important confounder if not dealt with properly. But given the limitations in the current data, other methods to seek the views and experiences of those involved in the care of women who have had an abortion should be considered as a valid source of evidence. Indeed, this would add "depth" and richness to the data which is frequently lacking in quantitative statistical methods. The voices of

			women's experiences are lost in the statistics. We note as illustration, Goodwin and Ogden's qualitative study of 10 women "*Women's reflections upon their past abortions: An exploration of how and why emotional reactions change over time*," Psychology & Health Vol 22, Issue 2, 2007, Pages 231 - 248.
25-6			This is welcome. However there is no detail provided as to who will be, or has been, contacted and whether their responses will be made public. Nor is any information offered as to the weight that will be given to any comments provided. **Clearly it is in the interests of transparency, objectivity and rigour to know who has been, (and who has not been), approached directly and specifically for comments**. We emailed the RCPsych to request this information but it has not been supplied to us.
26	**1-4**	**2.11**	While the research itself highlights the fact that the scientific standard of studies is poor and there are real limitations within the data, the evidence statements, which in effect provide the conclusions to the review, fail to reflect this **uncertainty of the data findings**. They are too definitive in claiming there are no differences between outcomes of pregnancy and abortion, when **the data itself is less clear and convincing.** Moreover, the report should state clearly, where appropriate, that failure to demonstrate a statistically significant relationship is not the same as demonstrating the absence of such a relationship.
29	**Table 3**	**3.3.1**	The quality ratings for the three NLSY all rely on the same data set, yet Schmiege 2005 is rated as "fair," Cougle 2003 "poor" and Reardon 2002 is rated as "very poor." If Table 2 criteria are applied consistently, all three studies should be rated the same. All should be rated as poor.
33	**41-50**	**3.3.2**	We strongly question the review rating of the Gissler studies as poor, and that 'very little information was provided'. **Did the reviewers attempt to contact the author to find out more information**? The Finnish registry linkage studies are useful in linking abortion with hard endpoints such suicide or death from other causes. **We are unable to do this in the UK because abortion notifications do not yet include the NHS number on the forms, despite this being at odds with current DOH policy**. We suggest this should be rated as moderate, rather than poor. (see our comment at the end of this form)
34-36	**Table 4**	**3.3.2**	This table sets out a high prevalence of mental disorders after abortion, compared to the general population. Although these findings do not control for prior mental health problems, and may therefore be dismissed as not useful for this review, it is important to note that rates are **still higher than the general population** i.e. those not having an abortion. (see our comment on p45, section 3.6).
41	**18-48**	**3.4.2**	Munk-Olsen's work is drawn on heavily in this review. However we question such a reliance on this research and consider it is not merited. There should be more clarity in the review regarding the discrepancies and weaknesses in the data collection of Munk-Olsen. For example, they exclude women with any in-patient history and examine only nine months prior to the pregnancy outcome. The nine months prior to birth, if it is unplanned, is not reflective of 'normal' stress levels. Instead, high levels of stress are common among women facing an unplanned pregnancy and considering whether or not to have an abortion. It would have been more accurate to have the assessment before the pregnancies were detected. Munk-Olsen report only on rates of first psychiatric contact, not all psychiatric contact. They do not measure frequency of use of mental

			health services nor severity of problems. There is some problem with misclassification especially in early years, during the study, of women who were described as new contacts but may have been current contacts. This could lead to the possibility of systematic bias in early data for outpatients. They also only look at one year post-abortion, even though there is ample evidence that many women have more delayed reactions and that negative reactions increase over at least two years (Major 2000). While the Munk-Olsen2011 study is better than most in that it includes at least some data regarding pre-abortion mental health, this data can only be characterized as a weak measure of pre-pregnancy mental health. Women who experience repeat abortions are likewise not considered. a substantial number of women seeking an abortion have a prior history of abortion Lastly, Munk-Olsen failed to include many important controls, such as marital status, income, education and wantedness. **We recommend that this research be rated as good, not as very good.**
45	**23-25**	**3.6**	**Statement 2 and 3 are too strong for the evidence available.** In the absence of meta-analysis (rightly, due to high levels of heterogeneity) this statement is based on a subjective assessment of the general trends in the numerical data. So, for depression, the prevalence rates in one study that accounted for previous mental health are 18.14% with a confidence interval of 14.59 to 21.69. However in studies that do not account for previous mental health, rates range from 11.1 to 40.6 and the confidence intervals range from 1.93% to 45%. These rates show that in fact lower prevalence was found in some of the studies that did not account for previous mental health. Whereas for suicide, rates are higher in those studies that do not account for previous mental health. And **for 6 outcomes there is no comparison** (see table 7). **Anxiety/related disorder is the only outcome where the statement appears to hold true**. So evidence statement 2 and 3 should more accurately read: *'Controlling for previous mental health problems has an impact on the prevalence rates for anxiety following an abortion. There was insufficient evidence to see a difference in depression, PTSD, outpatient treatment, psychiatric admissions, alcohol and drug related disorders. Rates of suicide were higher in the studies where previous mental health was taken into account*." Statement 3 as it stands also fails to make clear that mental health problems are <u>not</u> eliminated … and there is still an **increased incidence of mental health problems after abortion than for the general population**, even though controlling for previous mental health does **reduce** the apparent risk. Of course, better would be to have a general population comparator for each disorder to compare with table 6 p42. We do note the important point acknowledged on p85 where the reviewers accept the data that **even when there is no history of mental health problems, and prior mental health is controlled for, there are higher rates of mental health problems post-abortion**

			than the general population. **This should be brought into evidence statement 3**, lines 21-26. Also see our comments and suggestions for the statements on p89.
64	**2-4**	**4.5**	This may indeed be the most reliable predictor of adverse effects, according to the research, however there are a large number of other risk factors which have been identified and confirmed and have also been shown to have an effect (for example the APA task force 2008 details many). There is not real clarity in the literature as to the risk factors for poor outcomes thus this evidence statement is an overstatement as it stands. Ideally it should be expanded to clarify. At the least, the word 'only' should be changed to 'main'. 'Only' suggests a bias in favour of those who imply that abortion in and of itself does not carry any risk factors, whereas just a few lines down, line 14 suggests that 'women's personal experience of abortion may impact directly on mental health.'
64	**2**	**4.5**	There are two groups of women, those who have a predisposing mental illness but who do not have an abortion and therefore may not develop further relapses of their mental disorder and, secondly, those who do have predisposing mental illness/history, for whom abortion is a significant life event (whether they acknowledge it at the time or not) which triggers a relapse of mental illness. We are concerned that the report, in diminishing the importance of abortion as a potential stress trigger by merely suggesting that a past history of mental illness predicts future mental illness and that having no past history predicts no future mental illness appears to imply that abortion can be ignored as potential stress trigger causing relapse/ increasing vulnerability. Moreover, there is always the likelihood that the more abortions a woman has, her vulnerability will increase.
64	**13**	**4.5**	"*some suggestion"* is too weak a term to use as the evidence is stronger than this suggests. For example, as noted, distress after abortion IS a predictor (Fergusson 2009). Some studies (Broen 2006 and Fergusson 2008) have found that 'negative attitudes' to abortion can increase risk of poor outcomes. Moreover, only mentioning one or two factors gives them greater prominence when other factors not listed are also relevant. Fergusson 2008 actually found that mental disorder can be *caused* by abortion: "…*exposure to abortion has a small causal effect on the mental health of women*…" Therefore the evidence statement should be amended to be stronger. This is important because of the need to identify women who could be adversely affected, in order to try to avoid or alleviate poor outcomes.(and indeed, post-abortion)
65	**9-11**	**5.1**	See our comments on p18, lines 14-16. The report has two wordings for Question 3, which are different. The question on p65, is different to p18, line 14-16. Considering it is one of the three key questions under consideration this inconsistency reflects some sloppiness in report writing. More importantly, the two **answers produced could be different**.
68	**20**	**5.3.2**	See our comment on p81, line 47.
69	**23-25**	**5.3.2**	Munk-Olsen do not use a population-based cohort of Danish women with '…no previous history of mental health problems' as the review states. The study included women with an out-patient history. This therefore needs changing to say with '…*no history of in-patient treatment for mental health problems*.' It is quite possible that some women with ongoing out-patient care were included. See also our comments above on p41.

73	**Table 14**	**5.3.2**	Despite the limitations of the evidence, which are detailed in the review on p73-74, Table 14 clearly shows that the risks of psychiatric treatment follow-up, psychiatric outpatient treatment, suicide, alcohol problems, cannabis use and illicit drug use are increased in women who have abortions, compared to those giving birth. (Why are the findings from this table of evidence not summarised?) Despite the limitations of the evidence, **this does not justify the claim in evidence statement 1 (p81 line 38) that there is '…*no evidence* *of elevated risk of mental health problems*…'** This should be noted in the evidence statements for Q3, p81. Indeed, this evidence would answer 'yes' to question 3 of the review on p65.
75	**5-8**	**5.4.1**	By narrowing down the studies to only four, thereby excluding some key studies, the basis of the conclusions will inevitably be affected and therefore the conclusions drawn, limited. The reduction to just 4 studies is based on the desire to control for 'wantedness'. However as we note above (p18, lines 14-16), this is very difficult to measure and control for, but by using it, the review have thereby excluded a number of other important studies. **Four studies is a weak base for the evidence statements, particularly when one of these is then re-analysed with no explanation** (see comments on p78, lines 21-26).
78	**21-26**	**5.4.2.5**	The authors state that they received additional figures from Fergusson, leading them to **reanalyse Fergusson 2008** data and reach a conclusion that is different to his published paper. However the authors **do not provide these new figures**, nor describe how the new analysis was undertaken, and nor do they state what the original findings clearly showed. Since this new 'evidence' actually **contradicts** the original evidence in the Fergusson paper, more rationale must be provided to explain this conclusion, along with the new and original 'evidence'. This is an important point to rectify as Fergusson's 2008 findings have been widely cited to indicate a higher relative risk for those having an abortion. [The original paper states: "…*women exposed to induced abortion had risks of mental health problems that were about 30% higher than women not exposed to abortion.*" Fergusson, D. et al, 2008]
80	**Table 17**	**5.4.2**	This table compares like with like groups. It reveals weak evidence of a higher risk of anxiety disorder and self-harm outcomes for women post-abortion. It also shows weak evidence of higher risk of psychotic illness for women post-birth than post-abortion (but see our comment on p81, line 37-40). Whilst only weak evidence, the authors should not conclude, page 81, line 38, that '*there is no evidence of elevated risk of mental health problems'* post-abortion if they feel able to conclude that there is *'some evidence of lower rates of psychotic illness'* post-abortion. As it stands, this evidence statement thus favours (cites) only the one outcome that demonstrates a positive effect (post-birth) whilst ignoring the two outcomes that show a negative effect (post-abortion).

			The evidence statement <u>should be amended</u> to either state: *'there is some evidence of elevated risk of mental health problems and some evidence of lower rates of psychotic illness for women who have an abortion compared with those who deliver a pregnancy'* OR, there is no evidence for an elevated risk for either.
81	**29-47**	**5.5**	The quality of the evidence was graded very low by the reviewers so the evidence statements should more clearly reflect this **lack of validity and reliability**.
81	**37-40**	**5.5**	Note our comments on Table 14, p73 above. Despite the limitations of the evidence, which are detailed in the review on p73-74, Table 14 clearly shows that the risks of many mental disorders are <u>increased</u> in women who have abortions, compared to those giving birth. Therefore **this does not justify the claim in evidence statement 1 (p81 line 38) that there is '…*<u>no evidence</u>* *of elevated risk of mental health problems*…'**. We are highly concerned about this statement which does not reflect the evidence.
81	**39-40**	**5.5**	The statement that there is: "*some evidence of lower rates of psychotic illness for women who have an abortion compared with those who deliver the pregnancy*" needs amending as it **misrepresents Gilchrist's actual findings** in the data, which are far less certain. It overstates rates of psychosis and relies on statistically insignificant data: Gilchrist states in her research paper that many of the episodes were actually "*mild*" and there was "*insufficient information to identify truly psychotic episodes*". Moreover, the numbers on which this was based were very low - 7 women post-birth and 6 post-abortion. Gilchrist et al, 1995, p244. (Note also our comments on Table 17 above and the failure to include evidence for increases in two disorders post-abortion.)
81	**37-40**	**5.5**	The evidence used in statement one is selective and conflicting in also **ignoring Steinberg and Fergusson's findings.** Steinberg found increased risks for multiple abortions, although not for one. Fergusson 2008 clearly found higher mental health problems for those exposed to abortion. (see comments on p78 above. '… *exposure to abortion was associated with significant increases in risks of mental health problems'*. Fergusson 2008 p.449). Indeed, of the four studies selected, all four found some mental health problems post-abortion. The other two found increased self-harm (Gilchrist) and anxiety (Cougle). <u>Hence it is all the more inaccurate and misleading to claim in the 'evidence' statement claims that there is "NO evidence of an elevated risk…"</u> The statement should include words to the effect that there is a relationship between abortion and mental health problems identified by several studies. At the very least, there is **uncertainty** with the analysis, and **conflicting** evidence, which must be stated very clearly in the evidence statement. As this evidence statement one stands, it is misleading and open to

			misinterpretation by those less familiar with the research findings.
81	**47**	**5.5**	This statement also fails to reflect all the findings in the research. P68, line 20, finds the <u>opposite</u> to the evidence statement. This finding should be reflected in the evidence statement. (*Coleman reported that women who had an abortion were significantly more likely to receive outpatient psychiatric treatment up to 4 years later".*)
84		**6.2.1**	See also comments on p45 above and p89 below. Whilst this is true as far as it goes, it should add that the rate of mental health problems after abortion are still higher than the general population.
84	**36-39**	**6.2.1**	The use of the wording 'appear to be' in statement 1 reflects some uncertainty in the data, which is appropriate. However the following statement should similarly use the same cautious wording otherwise it appears that statement 1 is based on uncertain findings but not statement 2, which is incorrect.
85	**18-21**	**6.2.1**	Interestingly, here, despite the unnecessarily convoluted phrasing, the authors do acknowledge that <u>even when prior mental health is controlled for</u>, <u>there are higher rates of mental health problems post-abortion in the general population.</u> This is not clarified in the evidence statements (see page 45 and 89). It would be clearer and easier to read as follows: "*the included studies for this review show that the rates for post-abortion mental health problems amongst women with no history of mental health problems occur at rates higher than women in the general population*."
85	**48**	**6.2.2**	See our comments on p64-5 above. This needs strengthening, and only mentioning one or two factors gives them greater prominence when other factors not listed are also relevant.
89		**6.3**	**The findings are based on weak and often uncertain evidence, which should be much more clearly reflected in the evidence statements. This is a concern we have for Q3 more generally.**
89	**13-16**	**6.3**	We generally agree
89	**18-19**	**6.3**	This is not an accurate summary of the review findings. It does not correctly reflect the uncertainties and inconsistencies in the data. Nor does it reflect that there is a range of mental disorders more common after abortion than those who continue with their pregnancy. As we note above, (see p45 and p85) the rates of mental health problems are higher post-abortion than in the general population, even when controlling for mental health problems. Therefore **we suggest that on p89 an extra evidence statement is added to clarify that when prior mental health problems are controlled for, rates of post-abortion mental health problems occur at higher rates than the general population.** This would fit with the evidence cited in the review at p 85, and p45. Or alternatively, add a statement to the effect that mental health outcomes are not better if a woman opts for abortion rather than continuing with unwanted pregnancy.
89	**21-23**	**6.3**	We generally agree. We note however that abortion does not offer <u>protection</u> from mental health problems post-abortion.
89	**25-31**	**6.3**	NICE guidelines would be an improvement on the current situation where there is no guidance on treatment, women with mental health problems post-abortion will ideally need more <u>specific psychological interventions</u>. For example, they may feel guilt, anger or longing for the baby. There are few targeted, specialist, interventions for women

			experiencing these feelings. What there is, is primarily delivered by charities, such as CareConfidential.
89	**26-27**	**6.3**	We agree with the need for support and monitoring. This should be made available during the consent process prior to the abortion. **Properly informed consent to abortion requires information on the risks to mental health and should be a standard part of professional practice**.
89	**33-36**	**6.3**	We support this. We would add that health professionals dealing with women with unplanned pregnancies and/or abortion need to be fully informed and aware of factors that can lead to negative outcomes in order to offer information, including other options, and to be able to signpost on where necessary.
93		**Appendix 2**	We question why only these few researchers were contacted. Others will have papers forthcoming, particularly those who have already published widely in this area and whose papers are cited in the review. Specifically, we question why Gissler (see p33 above) and Coleman were not contacted?
			There are no recommendations offered in this review, as would usually be expected in a major review of literature. One that we would strongly recommend, as noted in our comments on p19 and p33 above, is that in order to enable further studies on this in UK **there is an urgent need for record linkage studies.** The review's reliance on studies from other countries highlights the lack of UK data and the need to rectify this. Every abortion provider in the UK should be required to record the patient NHS number. Linked data, using the NHS number linked to the female health record, is needed in order to enable future longitudinal studies of patient safety and outcome in the UK.

Please send completed form to AbortionMH@cru.rcpsych.ac.uk by 5pm on 29 June 2011

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