Author's response to reviews

Title: Depression and termination of pregnancy (induced abortion) in a national cohort of young Australian women: the confounding effect of women's experience of violence

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Author's response to reviews: see over

Preliminary response to Reviewers' reports

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Title: Depression and termination of pregnancy (induced abortion) in a national cohort of young Australian women: the confounding effect of women's experience of violence

Reviewer 1: Stanley Henshaw

Reviewer's report:

General

This study addresses an important issue with both clinical and policy relevance. Though handicapped by a low response rate, the dataset offers the possibility of shedding important new light on the relation of abortion to depression and domestic violence. I strongly recommend that you pursue this study with the revisions listed below.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

This study fails to take advantage of the dataset's potential. The analysis appears to be essentially cross-sectional. All the key variables, domestic violence, abortion, and depression were taken from the same survey in 2000. The variable "termination first in 1996" adds little. One of the problems with this analysis is that domestic violence could have occurred after the abortion and even been caused by it. In this case, domestic violence should not be used as a control variable in studying the effect of abortion on depression. Another is that depression could have pre-existed the abortion and even been a factor contributing to unintended pregnancy and abortion.

The important questions that should be addressed are whether abortion causes later depression and whether domestic violence is predictive of later abortion. The influence of depression on abortion can be assessed by a regression with depression in 2000 as the dependent variable, abortion between 1996 and 2000 as the key independent variable, and depression in 1996 as another control variable. Other control variables – domestic violence before 1996 and the socio-demographic characteristics -- should of course be included in the regression. This will provide some evidence as to whether the experience of abortion resulted in an increase in depression. If a relationship is found, it may not be conclusive because the increase in depression could have been the cause of the abortion rather than vice versa. If no relationship is found, this would be strong evidence that abortion does not cause depression. This regression will also indicate whether depression and domestic violence are predictive of later abortion.

***Response**:*

We have conducted extensive re-analysis, investigating the considerations raised by this reviewer. CESD was not measured in 1996. We refer below also to an additional (while limited) measure of depression in the 2000 ALSWH (i.e. has a doctor ever told you that you had depression either in the last four years - or more than four years ago) which we found strongly associated with CESD. We created a variable for depression in 1996 to add to the revised model which we are now submitting. We also added a variable for number of births. However our re-analyses have found a remarkably consistent pattern of effects.

As the Reviewer (Henshaw) suggested, we fitted a regression model with abortion first in 2000 as the key independent variable, adjusted for partner violence in 1996, depression in 1996, number of births and other SES variables. This gives an OR of 1.25 (1.02-1.55, p=0.03) for effect of 1st abortion in 2000 on depression in 2000 measured by dichotomous CESD. This does not differ much in this model from the impact of having one or more births OR 1.32 (1.02-1.72, p=0.04)

However, in the same model the impact of partner and recent violence in 1996 on CESD shows an OR of 2.22 (1.73-2.87, p<0.001) and partner violence prior to 1996 OR> 1.55, p<0.001. Depression diagnosed by a doctor prior to 1996 has OR of 1.66 (1.24-1.94, p=0.001). Analysis of CESD as a linear variable provides a similar picture: a 0.60 (0.06-1.14, p=0.03) change in CESD

for 1st abortion in 2000, compared with (statistically) significant changes of 2.5 for partner and recent violence in 1996 (1.72-3.22) and at least 1.5 (with lower limits > 0.9) for previous partner and other violence categories or OR 1.9 for depression in 1996. These associations have been adjusted for number of births and SES variables.

Investigations of transition variables for all of depression, violence and abortion show that there remains significant associations between these and CESD in 2000. Parameter estimates are very similar to those in the model suggested by the Reviewer.

Our aim was not to prove or disprove causality (as we did not think this achievable) but to indicate the level of impact, which either model demonstrates. Accordingly we present an adjusted analysis and have restructured our discussion to reflect the findings. We have retained a transition variable for violence, for abortion, adjusted for depression as diagnosed by a doctor in 1996 and added a variable for the number of births in this young cohort up to 2000. We have retained the transition variable for partner violence since it provides more information and makes no difference to the estimate of the association with abortion since 1996 (an adjusted OR of 1.22 compared with 1.25).

We are limited by the questions asked. For instance, we can not tell which women had an abortion since 1996 as well as before 1996, since the question asked in 2000 was for 'any' event. However, by comparing these with women's responses reporting abortions in 1996, we constructed a transition variable, to distinguish 1st abortion reported in 1996 from 1st reported between 1996 and 2000. Similar constraints apply for partner violence – we have used a transition variable developed for a companion paper (Ref Watson L, Taft A. Associations of Self-Reported Violence with Age at Menarche, First Intercourse, and First Birth Among a National Population Sample of Young Australian Women. Women's Health Issues 2007;17(5):281-289

We have two assessments of depression, in 1996 the variable we refer to is constructed from a question about a doctor's diagnosis prior to and after 1996, but will be subject to recall bias. The transition variable we constructed from this question is highly predictive of CESD. Using it in the model was consistent with parameter estimates of both abortion and violence. Now described in paper (1st page of Results)

2. The number of children should be included as one of the demographic control variables. This has been found to be associated with depression.

Response: We thank the reviewer for his suggestion as this has been an interesting addition to the results. Number of children (births) has been added to Table 2 as suggested. We note that the borderline impact of abortion between 1996 and 2000 on depression is the same as that of having one or more births and we have added a consideration of this to our discussion.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. There should be more detail about the survey, in particular, the response rate and an estimate of the extent of abortion underreporting. Given that 11% reported abortions, the lifetime expectation is that 25% of women will have an abortion, and most abortions occur when women are young, there is evidently a significant amount of underreporting, as is the case in most other surveys. Despite what Pratt et al. reported to Parliament, Medicare and state abortion reports give a pretty good idea of the abortion rate in Australia.

Response: We have added a little more detail about the study (especially as it been written about extensively) and an outline of the limitations especially those which may have contributed to under-reporting in our discussion.

2. In Table 1, the sum of cases under Age is greater than the total number of cases.

Response: We thank the Reviewer for pointing out this discrepancy. The problem arose because we are using a weighted analysis and the estimation of numbers depends on the subset involved, in other words, different groups of missing data make small differences in totals. This has now been fixed, however, similar to data provided in a companion paper (Watson, Taft and Lee, 2007) we present %'s only.

3. You evidently dichotomized depression in order to emphasize its clinical significance, but a continuous measure of depression might be more powerful statistically. Many readers would be concerned with depression even if it did not reach the level of clinical significance. If you think the dichotomous variable is more important, you could mention in the text that a similar analysis using a continuous variable gave similar (or different) results.

Response: Analysis has been done using CESD as both a linear and dichotomised measure. The results show a consistent effect (see Major #1 above). In fact the difference in magnitude of changes is more apparent for the linear variable and easier to explain. Results of both are now included.

4. The language is unclear in several places. For example, in the last sentence of the first paragraph of Background, "proportion of termination of pregnancies" could be referring to the number of terminations of pregnancy or to the proportion of pregnancies terminated by abortion.

Response: Sentence now reads: 'Younger women (particularly those in their teenage years) were more likely to report termination of pregnancies (occurring before the 1996 survey), compared with those in the 2000 survey, who were aged in their later twenties.'

5. In the last sentence of the first paragraph under Methods, how could 14,779 responses in Survey 1 be linked with 9683 responses to Survey 2? I suppose you mean that 14,779 women responded to Survey 1 but only 9683 responded to Survey 2.

Response: Sentence now reads: Data for 9683 who responded to Survey 2 in 2000 were linked to their responses from the Survey 1 in 1996 for this analysis.

6. In the fifth paragraph of the Discussion, you say that you demonstrated that "women were not more likely to experience probable depression if they had terminated a pregnancy", but the relative risk is 1.21 (0.98 – 1.50). A relative risk of 1.2 is not trivial, and while it is not statistically significant, you can't conclude that there is no risk. Based on this information, one might conclude that there is likely to be a risk but we are not 95% sure.

Response: The expression appears to be in the 4th paragraph and has been tempered in the light of both Reviewers' concerns and our re-analysis.

Discretionary Revisions (which the author can choose to ignore)

1. Under Occupation, what is "home duties"? Shouldn't these women be counted as "No paid job", or are they paid for their domestic duties?

Response: We don't know what the characteristics of these women are. Analysis of the group showed it to comprise women more likely to be married and to have children than those in other groups. We have changed the note for the Table to reflect this. Ie " Other comprises women more likely to be married, and to have children than the other Occupation groups".

2. Most U.S. studies use the terms "univariate" and "multivariate" rather than "univariable" and "multivariable". Are these Australian terms?

Response: These terms have been recommended as being more accurate by several epidemiologists in the UK and US – eg, Kenneth Rothman (Rothman K. Epidemiology: an introduction. New York: Oxford University Press; 2002) and Hosmer and Lemeshow, Kirkwood and Sterne,

Kirkwood and Sterne write in Essential Medical Statistics, 2003 2nd edition (Blackwell) p.106 "Multiple regression and other regression models often referred to as multivariate methods, since they investigate how an outcome variable is related to more than one exposure variable. A better term for such models is to call them multivariable regression models. In the strict statistical sense, multivariate analysis means the study of how several outcome variables very together.

Reviewer's report

Title: Depression and termination of pregnancy (induced abortion) in a national cohort of young Australian women: the confounding effect of women's experience of violence

Version: 1 **Date:** 8 October 2007

Reviewer: David Fergusson

Reviewer's report:

General

This paper uses data gathered over the course of a large longitudinal study of Australian young women to examine the linkages between depression and termination of pregnancy. The authors find evidence of an association between termination and depression but argue that this can be explained by confounding factors (notably exposure to partner violence and social disadvantage.). However, I found the paper to have a number of limitations that made the results far less clear cut than claimed by the authors. These problems include:

1) Inadequate measurement of outcome: The literature on abortion and depression has focussed on the linkages between exposure to abortion and longer term risks of depression. This study does not measure these longer term risks adequately. The measure used assesses depression in the last week using a short form non diagnostic measure. This falls far short of measuring the individual's history of depression. The result of using a limited measure based on a short time period is likely to be an under-estimation of the linkages between abortion and life time risks of depression and increased risks of type II errors. Nowhere in the report do the authors discuss these limitations of their results. To adequately test the hypothesis they propose requires a far more searching and extensive measurement of depression than attempted in this study.

Response: The CESD is a legitimate and well validated epidemiological measure of depression, developed from sound psychological measures, We do not agree that with this reviewer that it is inadequate. In our re-analysis we have included another measure of depression – that of reported medical diagnosis. See response to Reviewer (above).

2) Misinterpretation of the Test of Significance: In their discussion and abstract the authors imply that they found no evidence to suggest an association between abortion and depression. These sweeping claims mis–represent the data. There was, in fact, a marginally significant relationship (OR = 1.21 CI = .98 – 1.5) between abortion and depression even following all adjustments. In the discussion and abstract sections of this report this evidence becomes transmuted into the claims : "There is no evidence in this paper suggesting that pregnancy termination increases the risk of depression:" (Abstract) and "The evidence in this paper does not support a causal link between termination and depression" Neither claim is sound – what the paper produces is somewhat inconclusive evidence of a relationship between abortion and a weak measure of depression. The conclusions drawn by the authors only succeed by process of translating what are clearly "grey" results into black and white conclusions using the test of significance as the arbiter of this process

Response: We have undertaken a reanalysis and more careful interpretation of the findings. See also Minor #6 in Response to Reviewer above.

3) Mis–representation of regression results: The paper makes the strong claim that most of the association between abortion and depression is explained by partner violence. Specifically, the Discussion notes "This has allowed us to demonstrate that women were not more likely to experience probable depression if they terminated their pregnancy- whether in the teens or later provided there was adjustment for experience of violence". Inspection of the coefficients in Table 2 show that this is not the case. These coefficients show that controlling for partner violence only reduces the ORs marginally and that the association remains significant after such control. What the paper shows is that control for both partner violence and socio demographic factors has small accumulative effects that reduce the association to marginal statistical significance. Again this

reflects a tendency for “grey” findings in the Results section to be translated into black and white conclusions in the Discussion sections

Response: See Response Major #2 above.

My overall impressions of this research are that it contains a number of design limitations that preclude the authors from making the sweeping conclusions they present in the abstract and Discussion. What the study produces is the somewhat inconclusive finding of a weak association between abortion and a very limited measure of depression. With sufficient determination it would be possible to represent these results as either favouring the view that abortion does not increase abortion risks or as producing findings that are consistent with the view that abortion may increase risks of depression. I think that if this paper is to make a useful contribution to the literature both the Abstract and Discussion will need to be rewritten in a far more even handed way that recognises the limitations of the data set and the problems that arise in interpreting a marginally significant association between abortion and a less than comprehensive measure of depression.

Response: We have acknowledged the design limitations and modified our interpretations . See Responses above.