Author’s response to reviews

Title: Factors Associated with Occupation Changes after Pregnancy/Delivery: Result from Japan Environment & Children's Pilot Study

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Author’s response to reviews:
Reviewer reports:

#1. Lisa Russell (Reviewer 1): This is an interesting topic area that deserves attention. The paper is generally written well but here are a few suggestions that I think may help clarify its focus and coherency.

RESPONSE TO REVIEWER 1:

We wish to express our appreciation to the reviewer for your insightful comments, which have helped us significantly improve the paper.

First, with reference to comments, we corrected the following points of the whole. We would be pleased if you can check it.

1. The Japan Environment and Children’s Pilot Study (JECS) is the name of the epidemiological study conducted in Japan. JECS includes a nationwide study and pilot study. For the sake of clarity, we have used “JECS Pilot Study” to refer to the pilot study.

2. We have added some references and changed the number.

3. There are two kinds of leave program concerning childbirth in Japan as follows. It is a leave program before and after childbirth (the maternity leave program), which is regarded as obligatory by law, and a leave program for child rearing after that (the child-care leave program). For the sake of clarity, we have changed “the maternity leave program” as “the child-care leave program”.

1) The title is a little cumbersome - The terminology 'Japan environment' may be more succinctly put (or indeed left out) and I had difficulty understanding what the 'Children's Pilot Study' actually was - was this the name of the project? This aspect requires clarification further on in the paper.

Response:

We thank the Reviewer for this pertinent comment. The Japan Environment and Children’s Pilot Study (JECS) is the name of the epidemiological study conducted in Japan, by the Ministry of Environment and the National Institute for Environmental Studies, at 4 universities.

In accordance with the reviewer’s comment, we have made the following changes:

1. We have added “Result from” in the Title. (p. 1, line 2)

2. We have added an explanation about JECS Pilot Study, including the aim and recruitment method, to “2-1. JECS Pilot Study design” in the “Methods” (p. 9, line 195–line 211)
Furthermore, we have also added some words to the “Abstract” according to the “Methods” (p. 4, line 84–line 85). (This response is same to that to Comment 5 of Reviewer 1, and Comment 2 of Reviewer 2).

2) In the introduction 'background' context I recommend adding some more specificities such as using actual figures for percentages of females returning to work after having children - 2013 data from the Ministry of Health is later quoted but this seems a little dated, do you have access to more recent data?

Response:

Thank you for your advice. In accordance with the reviewer’s comment, we have made the following changes:

1. We have added some sentences regarding actual ratio and number of working women, and have replaced the 2013 data with the 2016 data (p. 6, line 124–line 126).

2. We have also added the recent ratio of women who continued to work after delivery or those who did not continue to work after delivery from the 2015 data (p. 8, line 172–line 177).

3) I might be tempted to use 'employment' rather than 'occupation' in the key words to access international audiences better.

Response:

In accordance with the reviewer’s comment, we have added “employment” to the key words (p. 5, line 110).

4) Reference the 'internet based interview' study mentioned to add credibility to the argument.

Response:

We have explained one previous study that used an “internet-based interview” (p. 8, line 186–line 187).

5) Further details would add context to the study and help develop a firmer conclusion section. For example - how was the questionnaire data analysed? What type of questionnaire was used? When was the questionnaire administered and how? More context in terms of
'occupational split' would be of use and a stronger link to how this may be related to socio-economic status would be helpful, the literature informs us that socio-economic status and type of employment matter! What was the geographic spread of these women (in Japan)? What is the 'Children's Pilot Study?' How were these women 'targeted' exactly, what was the sampling technique used? More on the policy context of helping working mothers get back to work (or not) would also be of interest here and would all help develop the rather scant conclusion offered. The conclusion is succinct but needs to be developed to link better to the existing literature and policy context.

Response:

We wish to thank the reviewer for detailed comments. In accordance with the reviewer’s comment, we have made the following changes:

1. We have classified “Methods” section into three paragraphs, using some headings. (p. 9, line 195, line 212, p. 10, line 230). Accordingly, we have also numbered “Background”, “Methods” “Results” “Discussion” “Conclusions” section (p.6, line 121, p. 9, line 194, p.11, line 259, p. 14, line 321, p. 18, line 423). (This response is the same as that to Comment 2 of Reviewer 2)

2. We have added the explanation about the JECS Pilot Study, including the aim and recruitment method, to “2-1. JECS Pilot Study design” in the “Methods” (p. 9, line 195–line 211). Furthermore, we have also added some words to the “Abstract” according to the “Methods” (p. 4, line 84–line 85). (This response is same to that to Comment 1 of Reviewer 1, and Comment 2 of Reviewer 2)

3. We have added the participant inclusion flowchart (Figure 1) (p. 9, line 213-line 214). (This response is same to that to Comment 2 of Reviewer 2).

4. We have explained the questionnaire in detail; we extracted information from the basic questionnaire and occupational information from the enclosed questionnaire regarding occupation. We have also added following information: the method of sending and retrieving, measurement time (from 0 to 7 weeks of pregnancy and 1 year after delivery), reasons for choosing, and response rate (p. 9, line 216–p. 10, line 229). Furthermore, we have also added the explanation about the questionnaire to the “Abstract” according to the “Methods” (p. 4, line 81–line 82). (This response is the same as that to Comment 2 of Reviewer 2)

5. We have added some sentences regarding analysis. Specifically, the participants answered according to the Japanese Standard Classification of Occupation 2009, and the answers were categorized into six groups based on the Japanese Standard Classification of Industries by the Ministry of Internal Affairs and Communication (p. 10, line 232–line 234)
6. We agree with your comments. We considered that socio-economic status could affect occupation change and added “mother’s education” and “household income” to Table 1 (p. 11, line 263–p. 12, line 266). We have also added the categories of these, to “2-3. Analysis Method” in the “Methods” (p. 11, line 245–line 252). Moreover, we have analyzed again using the 164 participants with all the basic information and occupation data as the population, although we have excluded the number of “unknowns” in the analysis in the original table before. Therefore, we have changed the tables and the numerical value of analysis result in the whole paper.

Although analyses using “mother’s education” and “household income” were conducted, there was no significant association between occupation change and these factors (Table 2) (p. 12, line 279–line 280). Therefore, we have not added these factors to the covariates in Table 3 and 4 (p. 12, line 283–line 286).

However, considering the influence, we have discussed this finding in “Discussion” and “Limitations” sections. (p. 16, line 377–line 380, p. 17, line 407–p. 18, line 413). (This response is the same as that to Comment 5-b) of Reviewer 2)

7. We have added the relation between the result of the existing literature and the child-care leave program to the “Conclusions” in the “Abstract” and the “Conclusions” section (p. 5, line 102–line 105, p. 18, line 426–line 431).

I encourage the author to add these details and make these amendments as the content of the paper is really interesting.

We wish to thank the Reviewer again for his or her valuable comments.
Reviewer reports:

#2. Janice F. Bell (Reviewer 2): This is an interesting paper; however, there are several issues that could be addressed to strengthen its contribution to the women's health literature.

RESPONSE TO REVIEWER 2:

We wish to express our appreciation to the reviewer for your insightful comments, which have helped us significantly improve the paper.

First, with reference to comments, we corrected the following points of the whole. We would be pleased if you can check it.

1. The Japan Environment and Children’s Pilot Study (JECS) is the name of the epidemiological study conducted in Japan. JECS includes a nationwide study and pilot study. For the sake of clarity, we have used “JECS Pilot Study” to refer to the pilot study.

2. We have added some references and changed the number.

3. There are two kinds of leave program concerning childbirth in Japan as follows. It is a leave program before and after childbirth (the maternity leave program), which is regarded as obligatory by law, and a leave program for child rearing after that (the child-care leave program). For the sake of clarity, we have changed “the maternity leave program” as “the child-care leave program”.

1. The Background would be strengthened by including a rationale for the importance of this research to women's physical, social or psychological health. Other than mentioning the timing of women's participation in the labor force with respect to marriage, pregnancy and delivery there are not details about why this question is a women's health issue. As written, the introduction is presented from a demographic perspective without any supporting evidence for the women's health implications.

Response:

We thank the Reviewer for this pertinent comment. In accordance with the reviewer’s comment, we have added some sentences regarding the impact of the women’s physical, social, or psychological problems on their decision to continue to work (p. 6, line 126–p.7, line 149). Furthermore, we have also discussed about the psychological and physical stress in the “Discussion” (p. 15, line 360–p.16, line 362, p. 16, line 366–line 367).
2. More details are need to describe the study population from which the participants were drawn. Who participates in the JECS? (It is implied that all respondents are women and they "continued to work after pregnancy"). Are JECS respondents recruited pre-conception, while pregnant or after birth? Did this study use data collected in the JECS or did the researchers send a separate survey to a subset of JECS participants? If so, how were these subjects identified? If I understood the methods correctly, the researchers sent a survey to a subset of JECS respondents at six time points. Why were only two time points used for this work? What was the response rate to this survey?

Response:

In accordance with the reviewer’s comment, we have made the following changes:

1. We have classified “Methods” section into three paragraphs, using some headings. (p. 9, line 195, line 212, p. 10, line 230). Accordingly, we have also numbered “Background”, “Methods” “Results” “Discussion” “Conclusions” section (p.6, line 121, p. 9, line 194, p.11, line 259, p. 14, line 321, p. 18, line 423). (This response is the same as that to Comment 5 of Reviewer 1.)

2. We have added an explanation about JECS Pilot Study, including the aim and recruitment method, to “2-1. JECS Pilot Study design” in the “Methods” (p. 9, line 195–line 211). Furthermore, we have also added some words to the “Abstract” according to the “Methods” (p. 4, line 81–line 85). (This response is same to that to Comment 1 and 5 of Reviewer 1).

3. We have added the participant inclusion flowchart (Figure 1) (p. 9, line 213-line 214). (This response is same to that to Comment 5 of Reviewer 1).

4. We have explained the questionnaire in detail; we extracted information from the basic questionnaire and occupational information from the enclosed questionnaire regarding occupation. We have also added following information: the method of sending and retrieving, measurement time (from 0 to 7 weeks of pregnancy and 1 year after delivery), reasons for choosing, and response rate (p. 9, line 216–p. 10, line 229). Furthermore, we have also added the explanation about the questionnaire to the “Abstract” according to the “Methods” (p. 4, line 81–line 82). (This response is the same as that to Comment 5 of Reviewer 1)

3. Including the rationale for the covariates selected would be useful. On the other hand, many important potential confounding variables were not addressed in the analysis (see comment below).
Response:

We agree with your comments. We considered that various individual factors could affect occupation change and added “mother’s education” and “house income” to Table 1 (p. 11, line 263–p. 12, line 266). We have also added the categories of these, to “2-3. Analysis Method” in the “Methods” (p. 11, line 245–line 252).

As a result, although analyses using these factors were conducted, there was no significant association between occupation change and these factors. However, we have discussed as a Limitation of this research. For details, we would like you to refer to the response of 4-b).

4. Definitions for "Regular" and "non-regular" employees are required. In addition, this distinction—which appears as the primary conclusion of the study-- is not addressed in the background, making it difficult to follow the study through from its purpose to its conclusion.

Response:

In accordance with the reviewer’s comment, we have defined “regular” and “non-regular” employees in the methods section (p. 10, line 235–line 239).

5. There are several important limitations to this study that are not mentioned or discussed:

a) The background supports examination of "participation" in the labor force in terms of working/not working. Yet the outcome was examined as binary changed/unchanged with the group "changing" occupation including those who resigned altogether. This limits the papers' contribution as it is unknown whether a change of occupation was to a more or less demanding job (e.g., what does a change from clerical work to sales work actually mean? Is it the same as a change from sales work to clerical work?) -- moreover this "change" as measured includes not participating in the workforce at all.

Response:

We thank the Reviewer for this pertinent comment. We thought that the reason for changing the job or resignation could vary from person to person, and therefore, we should consider the influence of the individual factors. We have discussed this problem as a limitation because the present study did not focus on this point (p. 17, line 407–p. 18, line 413).

b) Many potential confounding variables are not measured or considered--including socioeconomic status (household income, education, wealth).
Response:

We agree with your comments. We considered that socio-economic status could affect occupation change and added “mother’s education” and “household income” to Table 1 (p. 11, line 263–p. 12, line 266). We have also added the categories of these, to “2-3. Analysis Method” in the “Methods” (p. 11, line 245–line 252). Moreover, we have analyzed again using the 164 participants with all the basic information and occupation data as the population, although we have excluded the number of “unknowns” in the analysis in the original table before. Therefore, we have changed the tables and the numerical value of analysis result in the whole paper.

Although analyses using “mother’s education” and “household income” were conducted, there was no significant association between occupation change and these factors (Table 2) (p. 12, line 279–line 280). Therefore, we have not added these factors to the covariates in Table 3 and 4 (p. 12, line 283–line 286).

However, considering the influence, we have discussed this finding in “Discussion” and “Limitations” sections. (p. 16, line 377–line 380, p. 17, line 407–p. 18, line 413). (This response is the same as that to Comment 5 of Reviewer 1)

c) There is no discussion of selection bias in terms of survey participation and the possible implications for the study findings and generalization. This is difficult to judge given the sparse information about the original cohort.

Response:

In accordance with the reviewer’s comment, we added a discussion of selection bias as a limitation of this study (p. 18, line 413–line 418).

d) There is no discussion of social desirability bias in terms of its effects on responses to the survey questions.

Response:

In accordance with the reviewer’s comment, we added a discussion of social desirability bias as a limitation of this study (p. 18, line 418–line 421).

We wish to thank the Reviewer again for his or her valuable comments.