Author’s response to reviews

Title: Determinants of exclusive breastfeeding in an urban population of primiparas in Lebanon: a cross-sectional study

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Title: Determinants of Exclusive Breastfeeding in an Urban Population of Primiparas in Lebanon: a Cross-sectional Study

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Author's response to reviews: see over
Reviewer's report

Title: Determinants of Exclusive Breastfeeding in an urban population of Primiparas in Lebanon: a cross-sectional study

Version: 2 Date: 18 March 2013

Reviewer: Gabriel Agboado

Reviewer's report:
Thanks to the authors for their review. I am happy with many of the responses and the amendments made. Below are some of the areas that have not been adequately addressed.

1-Method:

a-Randomization needs to be described as the study based on the original data collection has not yet been published for readers to refer to [Major Compulsory Revisions].

The randomization process of the original study was described in the manuscript as the following: “The original study aimed to assess the effect of a 24-hour hotline service and a postpartum support film on reducing postpartum stress among first-time mothers. First-time mothers were randomized according to a computer-generated random list into one of four groups (postpartum support film, hotline service, postpartum support film and hotline service, or control group). A randomized controlled single-blind design was used. The postpartum support film was recorded on a DVD, the hotline service number was marked on a card and the control group entailed a music CD. All materials were placed in a hard DVD cover and in consecutively numbered opaque envelopes that looked and felt the same. These were handed to every mother by recruiters who were blinded to their contents”.

b-It is still not clear why they recruited consecutive primipara deliveries. The rationale for this approach needs to be justified particularly given the fact that all primips were eligible for inclusion in the except for those with specific clinical indications [Major Compulsory Revisions].

The term “consecutive” was omitted, as the meaning may be confusing to readers. All primipara deliveries that met the inclusion criteria were recruited.

c-Some aspects of the method section are better suited for result section e.g. non-significant differences for socio-demographic characteristics and information on loss to follow up. These pieces of information have been provided under
results hence could be deleted [Minor Essential Revision].

This information has been deleted from the method section. It is now described in and restricted to the result section.

d-On variable selection, I suggest the authors cite the relevant literature from their review e.g. for “health attitude”, postpartum health, infant characteristics etc. to support their selections [Major Compulsory Revisions].

The literature supporting the association between breastfeeding and the various exposure variables used in the study have been cited and include references # 9-12. Additional references that explore mothers’ postpartum health and breastfeeding have been included (references # 13 and 14).

2-Statistical analysis:

a-I have reservations about the appropriateness of the approach used to eliminate factors from the multivariate model. I will therefore recommend an independent statistician’s review as the method has the potential to undermine the credibility of the study. If the elimination is based on p value of less than 0.1 while 95% confidence interval is used to identify significant associations then there is an obvious inconsistency.

An independent statistician was consulted who re-affirmed it is appropriate to be less restrictive in the bi-variate analysis; this allows for the inclusion of more potential confounders into the multivariate analysis. Other researchers /biostatisticians use a p-value of 0.2 to eliminate confounding. The choice of p-value and 95 % CI in the multivariate models to draw conclusions is independent of the p-value chosen for the bi-variate analysis

b-The authors also made reference to p values # 0.1 as indicating significant association (on Page 9 “Factors significantly associated with exclusive breastfeeding”.

In the bi-variate analysis, the alpha was selected as p<0.1 and all the associations with p-values <0.1 were carried to the multivariate analysis as discussed above. In the text, the factors significantly associated with exclusive breastfeeding are those with p-values <0.1 and are referenced as such: “Factors significantly associated with exclusive breastfeeding included maternal age, employment and household income, gestational age and mode of delivery, intention to breastfeed at the time of delivery, baby’s health and main source of emotional support for the new mother.”
c-Also using bi-variate association as a basis for elimination irrespective of the threshold p value has the potential to neglect the role of confounding in any observed association or lack of association in such analyses. [Major Compulsory Revisions].

As mentioned above, we have attempted to minimize this by using a less restrictive p-value. This systematic way of eliminating confounding is very common in statistical analyses.

d-Variance inflation factor (VIF): there is no level indicative of absence colinearity though some would consider values below 10 as acceptable but not an indication of its absence. VIF of 1 indicates absence of colinearity. Some however state that the general rule of thumb is VIF of 4 would warrant further investigation. It will also be good to have the VIF stated in full [Minor revision].

All VIF’s were in the range of 1. It is, however, not common practice to mention all VIF’s in manuscript. In order to further clarify, we have stated that they were less than 4.

3-Grammar:

a-Though much has been done in this regards a few errors remain e.g. on page 9 “Mothers whose age was between 20 and 24 were more likely to exclusively” could better read “Mothers whose ages were between 20 and 24 were more likely to exclusively”. I recommend the authors proof read their work prior to submission [Minor revision].

This was corrected.

4-Consistency in reporting:

a-The outcome for this study was “exclusive breastfeeding at 8-12 weeks” but reporting has not consistently stated exclusive breastfeeding e.g. on page 9 “Maternal stress and whether the infant was the result of a planned pregnancy were factors marginally associated with breastfeeding” and page 10 “Compared to the control group, women in any of the three intervention arms were more likely to breastfeed with those receiving both the video and the hotline interventions being the most likely to breastfeed” [Minor revision].

This was corrected to reflect exclusivity as the outcome in the text.

b-Inconsistency in the following: PSS10 versus PSS, SSTAI versus STAI [Minor revision].
The reporting of the scales have been corrected to be consistently cited as PSS-10 and SSTAI

5-Recall bias:

I accept some of the pieces of information collected were retrospective. A bias would only occur if one group could systematically provide a given set of responses compared to the other group(s). If those who exclusively breastfed their babies at 8-12 weeks were equally prone to forgetfulness as those who did not, then recall bias could not exist. It would be helpful to elucidate which variables were prone the recall bias in the study [Minor revision].

We feel the potential for recall bias may still exist for some of the tested variables and should be mentioned as a limitation. These may include the social support variable as mothers who exclusively breastfeed at 8-12 weeks may systematically perceive more support than those who do not. Similarly, delivery experience, rooming-in, holding the baby and the baby’s characteristics may all be prone to recall bias or a systematically skewed perception among those who had breastfed for 8-12 weeks compared to those who had not.

6-Other points:

a-Tables: column totals not included and there apparent inconsistencies in the totals. E.g. in Table 2 under “Pregnancy and delivery indicators” the total number of participants was 447 for mode of delivery and 420 for gestational age. For each section the totals should be 452 primips who provided pieces of information for the study (some of which may be incomplete) with rows included to account for missing data. Indeed almost all of the sections did not add up to 452. I would have expected that at least the section for age group to would add up to 452 but it was 448 [Major Compulsory Revisions]

Thank you for your comment. The totals do not always add up to 452 due to missing data. In order to avoid confusion and upon the recommendation of a statistician, we have further clarified this by adding the following footnote: “all figures do not add up to original sample size due to missing values”. The missing values were not included as separate rows due to their small numbers (<5% of original sample size).

b-Table2: Household income – this should be “monthly household income” [Minor revision]

This was corrected.

c-Table2 – Valuable, Invaluable: the latter means “extremely useful”. I suggest this be changed to “not valuable” unless the intended meaning is “extremely
useful” [Minor revision]

This was corrected.

d-Table 2: Women health attitudes - the variables listed there are mainly related to lifestyle in exception of intention to exclusively breastfeed. I suggest the sub-heading is modified.

We accept that smoking and weight gain are part of a woman’s lifestyle. We feel strongly this lifestyle necessarily reflects an attitude towards health. To further explain the issue and accommodate for the “lifestyle”-related variables, we have added the term “behavior” to the label.

e-It is also not clear if these are smoking was during pregnancy or after delivery [Minor revision].

The variable smoking was assessed by asking if a mother ever smoked or not. The exact timing of the behavior was not captured by our questionnaire.

f-Table 2: Social support - it is not clear if this was ante-natal or post natal support [Minor revision]

The social support variable was first mentioned in the “Variables” section on page 7 as postpartum health and social support. This was intended to reflect postpartum social support. To avoid confusion, the qualifier postpartum was added as such: “postpartum health and postpartum social support”.

g-On page 14 the authors stated “the study was not originally designed to capture breastfeeding outcomes”. This could mean the power calculation done for the original study may not be adequate for this study [Minor revision].

The power calculations conducted for the original RCT allowed for multiple outcomes to be investigated. As such, we feel these calculations are adequate for this study. These calculations have been included in the revised version of the manuscript under “Sample size”.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being Published

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
I declare that I have no competing interests