Author’s response to reviews

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

Authors:

Haya Hamade (hayahamade@gmail.com)
Monique Chaaya (mchaaya@aub.edu.lb)
Matilda Saliba (dida91@hotmail.com)
Rawan Chaaban (rawan.chaaban@gmail.com)
Hiba Osman (hibahosman@gmail.com)

Version: 4 Date: 17 June 2013

Author’s response to reviews: see over
Author's response to reviews

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

Authors:
Haya Hamade (hh86@aub.edu.lb)
Monique Chaaya (mchaaya@aub.edu.lb)
Matilda Saliba (dida91@hotmail.com)
Rawan Chaaban (rawan.chaaban@gmail.com)
Hibah Osman (ho02@aub.edu.lb)

Version: 3 Date: 7 June 2013

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

---

**Reviewer's report**

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

**Version:** 2  **Date:** 7 May 2013

**Reviewer:** Oyindamola Yusuf

**Reviewer's report:**

1-Abstract

a-Background information is too long for an abstract. First sentence is not relevant. WHO recommendation is also not relevant. The most relevant information is that proportion of mothers who exclusively breast feed is low and the determinants of breastfeeding as not been documented in the Middle East.

This information has been omitted from the abstract in favor of a longer result section.

b-Results section of the abstract contained too little information as well. More statistics of the factors associated with exclusive breast feeding should be given such as the odds ratios and p values.

More information has been added to the result section as such” Factors associated with exclusive breastfeeding included maternal work (OR=3.92; p-value<0.001), planned pregnancy (OR=2.42, p-value=0.010), intention to breastfeed (OR=3.28; p-value=0.043), source of maternal emotional support (OR = 1.87, p-value=0.039) and the use the postpartum support video, the hotline service or both (OR=2.55, p-value=0.044; OR=3.87, p-value=0.004 and OR=4.13, p-value=0.003)”

2-Design and data source

a-More information is necessary on the original RCT. What was the aim of this RCT? Details of sample size and power calculations are also necessary. The sample size of 552 for the cross sectional survey needs to be justified. Since the
aim of the RCT was not to determine the predictors of exclusive breastfeeding and was not powered for that, the authors need to show that the sample size of 552 which was from the RCT is also sufficient for the analysis on the determinants of exclusive breastfeeding.

The RCT aimed to assess two different interventions designed to reduce perceived postpartum stress among first-time mothers. Sample size was calculated based on the aim of reducing the PSS-10 mean by 4 points. The mean score for the PSS-10 was found to be 18.3, with a standard deviation of 4.9 in the validation study among postpartum women in Lebanon. Based on the assumption that 50% would watch the film, the mean for the intervention group was considered to be 16.3. Therefore, 126 women were needed in each arm with an alpha of 0.05 and a power of 90%. Accounting for 10% loss to follow up, 140 women were needed for each arm. The current study is a cross sectional study targeting the women who were followed up. The sample of 452 has enough power to detect statistical difference with the number of predictors that were chosen. The above-mentioned 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”.

b-Analysis
Statistical software used should be mentioned. p value <0.1 should be justified. Was there multiple testing? Was the type two error modified? If yes, why?

SPSS version16 was used. Concerning the choice for a p-value<0.1, an independent statistician was consulted: it is appropriate to be less restrictive in the bivariate analysis; this allows the inclusion of more potential confounders into the multivariable 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 bivariate analysis. There was no multiple testing and the type II in the multivariate analysis was not modified.

3-Results

a-The number of women who refused to participate and the number who were lost to follow up should be given. It is not enough to say there are no significant differences in the characteristics between these groups without providing the numbers and or percentages.

The numbers of women who refused to participate and who were lost to follow-up are mentioned in the results section of the manuscript as such: “A total of 751 primiparous women were approached to participate with a 74% enrollment rate
(119 were excluded and 80 refused to participate).... Of the 552 women who received the baseline assessment, 452 (82%) completed the postpartum questionnaire and 100 (18%) were lost to follow-up”

b-Line 17 under the results: percentage of term of 91.4% and c section of 46.3% was greater than 100%. Why is this so? What were the denominators used to get these percentages?

The percentage of term infants is independent from the percentage of infants who are born by C-sections. Many term infants are also born by C-section, which explains the percentages. We would not expect them to add up to 100%.

c-Bivariate Analysis

Line 12: This result should start by giving the percentage of mothers between age 20 and 24yrs who breast fed compared to the other mothers before giving the p value. It is important to see the effect size before judging with the p value. The choice of the words "more likely and less likely should be used in the multiple regression analysis where the odds ratios are reported and not to be used in the bivariate analysis.

The wording was changed to the following: “41.2% of mothers whose ages were between 20 and 24 exclusively breastfeed their infant (p-value= 0.003) compared with 30.4% of younger mothers”. The wording “more likely and less likely” was dropped from the bivariate analysis description in favor of showing comparative percentages and p-values.

d-Multivariate analysis

Line 2. Why was intervention arm used as covariate in this analysis? The background information nor the methods mentioned the description of these arms. This is a cross sectional study and not the RCT.

The RCT methodology was further explained in the main manuscript, including a more detailed description of the intervention arms (design and data source section-page 5). The intervention arm refers to mothers who received either a postpartum support film or used a support hotline or both. The data supporting the associations between postpartum stress/support and breastfeeding is strong and has been cited in the background section (references #13 and #14). The intervention arm in this study is another exposure variable we feel is important to include.

Odds ratios should be reported with 95% confidence intervals.

The confidence intervals for the odds ratios are included in table 3. We feel
readers can refer to the table when confidence intervals are sought. In the main text, odds ratios are reported with their respective p-value to indicate significance levels, which we believe is sufficient.

Issue of control group was also mentioned. This is confusing and may be misleading since it’s a cross sectional study and not a report of the original RCT.

Thank you for your comment. As the phrase “control group” may be confusing, we dropped it in favor of “mothers who have not received a postpartum film or used a hotline” in the text and the qualifier “neither” in table 2. To further clarify, we included a more detailed explanation of the RCT methodology (page 5 design and data source section).

4-Discussion

a-Authors conclude that the sample of 552 was representative of women in Beirut. Is this correct? They need to justify this in the methods. Moreover, the women in this study were recruited from a trial, so how can they really be representative of women in Beirut?

The sample of women is indeed representative of the Beirut population as all Beirut hospitals were included in recruitment. Furthermore, mothers who were excluded from the study or refused to participate were not significantly different from those who participated.

b-Discussion includes explanations of the intervention arms, however, these interventions were not mentioned at all in the background information. There is really no link between these interventions with the aim of determining factors associated with exclusive breastfeeding. This information may be deleted or the authors should properly describe these arms in the background as well as the methods. This may change the title of the study. So at best, the authors’ should delete information on these arms so that the study can stand as a cross sectional study.

The intervention arms entail providing postpartum support to new mothers. This variable has been extensively studied in association with breastfeeding. The references have been added (#13, #14) to the text in the background section. We feel strongly that this variable should be explored as a potential breastfeeding determinant and is, as such, important to include in our analysis and report.

4-Conclusion

a-Authors should not make conclusions outside the scope of study such as mentioning macro analyses of breastfeeding policies.
The reference to macro-analyses was deleted and the conclusion modified to “Beyond the epidemiology of breastfeeding, further research is needed to assess which multi-level strategies most effectively raise breastfeeding outcomes”

5-Tables

a-Table 2. Footnote: "not all variables add up to 100% because of missing values". Why is this so? All missing values should have been subtracted from the original sample size. All figures in this table should up to the total of the respective variable in the table, but may not add up to the original sample size. I checked and saw that they add up but not to the original sample size. This footnote information is misleading. It should be deleted and replaced with: “all figures do not add up to original sample size due to missing values”.

The footnote was modified to “all figures do not add up to original sample size due to missing values”.

b-Table 3: Unadjusted OR need not be presented. Adjusted OR, 95% CI and p value is sufficient.

We chose to present crude odds ratios to enable readers to estimate and assess the effect of confounders on each exposure variable. We believe this can underscore the multitude of factors that affect breastfeeding outcomes.

c-Row one of table has reference in front of the variable? What does this mean? a reference category? This may be misleading and should be deleted. The reference category can be marked with an asterisk and a footnote given at the bottom of the table to show that the asterisk is the reference category.

The reference term indicates reference categories between parentheses. To minimize confusion, the term was replaced by an asterisk and a footnote that reads “The variables in parentheses indicate reference categories”.

d-Social support. Reference category here is other. What constitute these others?

“Others” refers to sources of social support that are not the mother, including other family members such as the husband and sister or non-family members.

Level of interest: An article of importance in its field
Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.