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: 2 Date: 28 February 2013

Author's response to reviews: see over
Point-by-point response:

1-Editorial changes:

1-TRN number of original trial

Kindly note the TRN of the original trial (NCT00857051) was included in the main manuscript and BMC public health submission system

2-Make sure formatting style is conforms with the BMC PH style

The formatting style has been corrected to conform to the BMC-PH style.

2-Reviewer 1: Gabriel Abgoado

Thank you for your comments and feedback. Kindly find the below responses and suggested changes.

1-Title:
Though the study used data from a longitudinal study, this study, in itself, should appropriately be classified as a cross-sectional study particularly as its main aim is to assess prevalence of exclusive breastfeeding and factors associated with it. Also there is the need to clarify in the title the study was in primips. The study was mainly about exclusive breastfeeding and this should be reflected in the title.

In order to accurately reflect the methodology and population, the title was changed to “Determinants of Exclusive Breastfeeding in an Urban Population of Primiparas in Lebanon: a Cross-sectional study”

2-The authors' use of exclusive breastfeeding will need to be defined.

The World Health Organization definition was used in this paper. Exclusive breastfeeding is defined as “giving no other food or drink, not even water, except breast milk (including expressed expressed or milk from a wet nurse) but allows the infant to receive ORS, drops and syrups (vitamins, minerals and medicines)”. The above definition was integrated in the text.

3-Page 4: the authors stated “To date, studies that have addressed possible predictors of breastfeeding behavior in Lebanon remain scarce”. However they cited several Lebanese studies on breastfeeding. Surely these might have addressed several aspects of breastfeeding behavior in addition the overall prevalence e.g. influence of religion and pediatrician gender, cultural beliefs.
Thank you for your comment. Although some studies have indeed addressed breastfeeding predictors in this context, they remain small in number and have demonstrated a wide range of variability in breastfeeding rates and predictors. This study contributes additional insights to the available evidence thereby assisting in the revival of the breastfeeding discourse among policymakers and public health experts.

4-On Page 4: What does the “breastfeeding ecosystem” mean?

The breastfeeding ecosystem refers to the breastfeeding environment in a given setting including the various factors that may support or limit its growth such as the presence of baby friendly hospitals, short maternity leaves etc...

5-I suggest “Setting” comes before “Design and data source”. I also suggest “Setting” section be expanded. It may be helpful to move the paragraph in the introduction beginning with “Lebanon is a small middle-income country on the Eastern Mediterranean” to “Setting”. It should also include total number of hospitals serving the population of Beirut and which proportion or number provides maternity services, and if applicable the numbers implementing any evidence-based breastfeeding promotion programme e.g. UNICEF Baby Friendly scheme.

We have re-structured these sections based on the above suggestions; we have expanded the setting section and moved it to the beginning of the methods section-ahead of design and data source-More demographic data about Beirut healthcare services has been added as such “Close to 100% of all births in Beirut are attended by a skilled birth attendant. All hospitals (26 private and 1 public) with maternity wards in the city and its close suburbs were considered eligible for enrollment. Twenty-three (22 private, 1 public) hospitals agreed to participate. None of these hospitals implement the WHO/UNICEF Baby-Friendly Hospital Initiative.

6-I find the data description inadequate. For instance which pieces of information were obtained at baseline and which was collected post-partum?

We have described the baseline and postpartum information in more detail than in the first draft; please refer to the main text under the “design and data source” section for further clarification as such:

“Baseline data was collected daily over a 7-week period by 8 recruiters who included midwives and public health graduate students. Recruiters visited participating hospitals daily at same time (8-10am) to include all consecutive primipara deliveries who met study inclusion criteria. At each hospital visit, the recruiter reviewed the list of deliveries in the last 24 hours and visited every woman who met the inclusion criteria in her room. Written informed consent was obtained from each prospective participant. A 3-minute baseline interview was conducted to gather information about the woman’s socio-demographic status, her health and
that of her baby as well as her pregnancy, delivery experience and intent to breastfeed.

Fourteen assessors were trained to conduct the postpartum interview. Interviews were conducted face to face at women’s homes when possible. Telephone assessments were conducted when assessors were unable to visit women at home. The postpartum interview lasted between 30 to 50 minutes. It included questions about general health, infant health and care, breastfeeding attitude and behavior, marital life, employment and perceptions of self-efficacy and social support. In addition, screening tests for postpartum anxiety and stress as well as postpartum depression were conducted”. Kindly note the original RCT has been submitted for publication and includes the detailed description of each baseline and postpartum variable.

7-I find the “Sampling” section perplexing. This should be under “Design and data source” and not be a sub-heading. Sampling was not properly addressed in the section. For example, why were “all” consecutive deliveries included? Is there a pattern in the hospitals whereby all consecutive deliveries were in primips? The sampling method employed was also not described e.g. what was the sample size, and how was it determined; any power calculations; did the researchers recruit the minimum number of subjects required? Also the data came from a randomized controlled trial. The randomization was not described.

The sampling method is now included under the design and data source section. The text has been further clarified with respect to sampling: “all consecutive primipara deliveries that met study inclusion criteria were approached”.

“Based on power calculations for the original RCT, a total of 751 primiparous women were contacted: 119 were excluded and 80 refused to participate. There were no significant differences between the socio-demographic characteristics of women who participated and those who refused (data not shown). Of the 552 women interviewed at baseline, 18% were lost to follow up and 442 women were assessed postpartum. There were no significant differences between those who were lost to follow up and those who remained in the study (data not shown).”

The randomization procedure is detailed in the main manuscript for the RCT, which has been currently submitted for publication. The minimum subjects required for statistical power were recruited.

8-Variables section: The predictor variables were poorly described. For example what are “pregnancy and delivery indicators, health attitude, postpartum health and social support, the infant’s characteristics” and which variables were continuous ones and why were they dichotomized? The authors did not provide any justifications for the myriad of predictor variables they elected to evaluate. This should be done with appropriate supporting evidence. It seems to me authors set out to dichotomize all variables. This approach could lead to residual confounding. I will suggest the approach is reviewed and appropriate revisions made. I also find the list of variables too long and could lead to over adjustment in a multivariate model.
The predictor variables are now defined under the variables sections and details on scales used are included. A separate table (table 1) is provided to describe variables and their bracketing. The choice of these variables is based upon the literature and the evidence-based known predictors for breastfeeding in other settings detailed in the background section. Please refer to the background section for further explanation of the association between the different predictor variables chosen and breastfeeding in the literature.

A statistician reviewed the analyses. The decision to dichotomize was made after plotting a spine between log of breastfeeding and each exposure; the spine was not linear and therefore variables were dichotomized based on their distribution according to the median. Most determinants were categorical and binary in the original data.

9-Analysis:
It is not clear why the authors elected to only include variables associated with a P< 0.1. The cut-off point for statistical significance is normally not P< 0.1, it is 0.05. It is also not clear why they decided to exclude these factors based on bivariate analyses outcomes instead of using recognised methods of eliminating variables that do not contribute to the fit of a regression model e.g. backward or forward stepwise procedure. The statistical software used for the analyses was also not mentioned.

I suggest a statistician reviews the analyses on the following grounds:
1. Dicotomization of variables with more than 2 levels and continuous variables
2. Setting the cut-off point for significance level at 0.1
3. Failure to use recognised methods for eliminating variables that do not contribute to the fit of the regression model
4. The rather long list of variables we need to define each variable

Dichotmization and grouing of variables were addressed earlier

The methodology used to decide which variables to keep in the multiple regression model is a well-recognized statistical methodology. We systematically eliminated variables based on the bivariate analyses. The choice of 0.1 as a significance level is to be conservative and this is based on a biostatistician recommendations. Other statisticians consulted advised to have a P < 0.2 for the selection of variables from bivariate to multivariate. SPSS 17 was used for analysis

10-Earlier in the report the author’s stated the data came from a large randomized trial. However the sample size of 552 primips is not particularly large. The authors reported “…the prevalence of stress, anxiety and depression symptoms was relatively high (50 %, 47.7 %, and 33.6 % respectively)”. How were presence of stress, anxiety and depression in study subjects determined and which levels are low and which are high? Apart from stress level, no results were presented for the other two variables in the logistic regression analyses.
The qualifier “large” from large RCT has been omitted. The variables of stress, depression and anxiety were determined using standardized scales described in the revised version of the text as such: “Postpartum health included three measures of psychological health. Stress was measured by the Arabic Cohen Perceived Stress Scale- PSS10, a validated scale in Arabic (21). Each item is measured on a 4-point scale. The total score was dichotomized based on the median value. Postpartum depression was assessed using The Arabic Edinburgh Postnatal Depression Scale (EPDS) (22); women who scored above a threshold of 12/13 in the EPDS were considered to have postpartum depression. Anxiety was measured by the Spielberger State-Trait Anxiety Inventory (SSTAI): This questionnaire had been previously translated into Arabic and validated (23). A total score was calculated and then regrouped into two categories. The results for depression and anxiety levels were not included in the logistic regression model because they were not significantly associated with breastfeeding at the bivariate level. They would then have to be excluded from the regression according to our statistical analysis plan.

11-On page 8, the authors stated “These were categorized into seven groups”. Factors classified under each group should have been defined earlier in the method. These can be put in a box or table.

Kindly refer to comment # 8 of this document. The predictor variables are now defined and explained in a separate table (table 1).

12-Also on page 8: “……whose household income was more than 2 million Lebanese Pounds (around 1,333 US Dollars)…. It is not clear if the household income an annual income, monthly income or daily income.

The income has been clarified as “ working mothers, and those whose monthly household income was more than 2 million Lebanese Pounds…”

Discussion
The authors stated “This study confirms the low prevalence of exclusive breastfeeding at 8#12 weeks postpartum in a representative sample of women in Beirut”. It is not clear from the description of the sampling method that the sample was representative of the study population. I suggest sampling is described in more details under methods.

The sampling method entailed including all hospitals with maternity wards in Beirut; it is therefore representative of the Beirut population as a whole. In addition, the C-section rates found in this study was similar to C-section rates established in other studies in Beirut, which provides additional support to the representativeness of the sample in this study.

Page 10: The authors stated “The large C#section rate found in this study was only slightly higher than the C-section rates in Beirut reported in other studies”. This is not relevant as it is not what the authors set out to investigate and I
suggest this be dropped.

The qualifier “slightly higher” was dropped in favor of “similar” rates as such: “The large C-section rate found in this study was similar to C-section rates in Beirut reported in other studies”. This is important to mention as it supports our sample is representative of the general population in Beirut, which makes our conclusions generalizable to the Beirut population.

Page 11: The authors stated “There is a wealth of evidence suggesting C#sections are detrimental to breastfeeding”. However the authors did not find significant association between caesarean sections and breastfeeding prevalence. What could be the plausible explanations for this observation? Are the hospitals implementing any breastfeeding support programme as there is evidence suggesting that if the hospital environment is supportive of breastfeeding, caesarean sections have little impact on breastfeeding outcomes?

This could be explained as follows: “In our study, however, C-sections were not significantly associated with breastfeeding. One possible explanation could be that a large number of C-sections performed at hospitals in Beirut are elective- or not medically indicated. As a result, many mothers who deliver by C-sections in Beirut are healthy and able to initiate breastfeeding faster than mothers who have had a C-section for a medical reason and may be more ill”.

Page 11: “Breastfeeding promotion efforts may thus benefit from targeting the elevated C#section rates. For example, regulations that discourage the increased C#sections rates could prove to be a cost#effective way to increase breastfeeding.” The evidence from this study does not support this recommendation i.e. caesarean sections did not affect breastfeeding prevalence. The recommendation is misplaced and should be dropped.

Thank you for your observation; this recommendation was dropped from the manuscript.

Page 12: “In today’s ever#growing time constraints, our result emphasizes the value, for women’s health practitioners, of simply taking the time to answer a question. Given that the hotline issues addressed were not restricted to breastfeeding, it would be interesting to investigate whether the impact of a “breastfeeding hotline” would be greater. If so, a national hotline could prove to be a cheap and feasible way to raise breastfeeding rates.” I am not clear about these sentences. Rewording them may make the point clearer. There may not be the need for further investigating telephone support for breastfeeding mothers as there is a wealth of evidence that it could improve breastfeeding rates.

We feel a breastfeeding hotline could be an effective tool to increase breastfeeding rates in Lebanon. Although this has been proven in many settings around the world, it has yet to be systematically established within the Lebanese cultural context.
Limitations of the study
There is no section dedicated to this. I suggest the paragraph beginning with “The restriction of participants to healthy first-time mothers delivering in Beirut is one limitation of this study”.

We have dedicated a separate section to limitations of the study.

Page 12: The authors stated “Past experience with breastfeeding may affect breastfeeding behavior with subsequent children”. This is not relevant to the study population as none of them could have had a previous breastfeeding experience.

Past breastfeeding experience may mean exposure to breastfeeding via family or friends or even the mother having been breastfed herself. This has been further clarified in the text as “Past exposure to breastfeeding within the mother’s social networks may affect breastfeeding behavior; these associations were not captured in this study”.

Page 12: “As the present study was nested in a larger RCT, exclusive breastfeeding at 8-12 weeks postpartum was chosen as an outcome rather than the standard 6 months defined by the WHO”. It is not clear the points being made with this sentence. I will suggest the authors rephrase to clarify the point or delete it all together.

This point was further clarified in the text as “Exclusive BF at 8-12 weeks was chosen as an outcome rather than the standard 6 months defined by the WHO because the study was not originally designed to capture breastfeeding outcomes. Its results are nested within a larger RCT that aimed at assessing postpartum stress and depression rates at 8-12 weeks postpartum”.

Page 12: “As with any longitudinal study, the potential for recall bias is always present; however, the short recall period makes this unlikely to have caused significant change in results”. Recall bias is more a feature of case control studies and I am not clear how it could affect this study. I will be grateful for the authors’ clarification in this regard. Other possible limitations may be: loss to follow up as the outcomes for this group may be different. Also those who declined to participate may also have different outcomes.

It is important to remember that the potential for recall bias exists anytime one collects data retrospectively. The postpartum interviews included questions that required mothers to remember past information. For example, mothers were asked about their infant’s health in terms of number of pediatrician visits they’ve had since delivery. Responses entail having to remember information from the last 8-12 weeks. Although this is short-term recall and therefore less likely to cause recall bias,
we feel it is a potential limitation worth mentioning. Furthermore, the immediate postpartum period is usually a stressful and physically taxing time for mothers. We can speculate they would be more “prone” to forgetfulness during such times.

Conclusion:
As a general rule the conclusion should be based on the findings from the study being presented and should not include references from the extant literature. Some of these and some from the discussion could be put under “Implications for Practice” sub-heading under Discussion.

Thank you for your observation. The references were deleted from the conclusion section.

Tables:
Tables should display row and column totals and account for any missing data. There should consistency in the Tables e.g. in Table 1 categorisation of educational status is not the same as the one in Table 2. Table rows need to be properly spaced out to ensure the content of cells are legible.

The tables have been revised and all inconsistencies in categorization corrected.

Table 2 is confusing. The authors should consult the literature on how such data could better be presented.

Table 2 has been modified and is now presented with subheadings for enhanced readability.

The tables are too long. I will suggest the authors split them and categorise them under headings reflecting broad categories investigated e.g. maternal socio-demographic characteristics, baby characteristic etc. If the authors are clearer about which predictor variables they intend to investigate in the analysis, they could reduce the rather long list of variables, some of which overlap e.g. “held baby in the 1st 30 minutes” and “held baby in the 1st 2 h

The tables are presented in a clearer fashion, with inclusion of headings reflecting broad categories as suggested. Breastfeeding is influenced by a wide range of complex medical and psychosocial predictors. Although long, we feel it is important to include the list of variables mentioned to be able to accurately investigate the many possible breastfeeding predictors.

Grammar:
Grammar is acceptable in the main. However there a number of grammatical errors that need to be corrected. I have provided a list of a few.
• Page 2: “the proportion of mothers who maintain exclusive breastfeeding at 6
months of life” should be reworded e.g. “the proportion of mothers who exclusively breastfeed their babies up to 6 months” or something along this line may be more appropriate.

This was changed to “the proportion of mothers who exclusively breastfeed their babies up to 6 months remains low”

• Page 5: the sentence “chronic diseases requiring daily management such as cardiovascular” should read “chronic diseases requiring daily management such as cardiovascular diseases”….

This was corrected to “cardiovascular diseases”

• Page 9: “…….emotional support and the intervention arms were statistically associated with exclusive breastfeeding…” should be revised to read “…….emotional support and the intervention arms were significantly associated with exclusive breastfeeding…”

This was be corrected to “significantly associated”

• Acronyms e.g. RCT should be in their complete form when first used with the acronyms in parenthesis. Subsequent uses of such acronyms could then be without the full forms.

The acronym RCT was used after it was first introduced in its complete form as randomized controlled trial.

Discretionary revisions
The outcome variable selected was “exclusive breastfeeding at 8-12 weeks”. I think if the authors explore in addition the relationship between the predictor variables and any form of breastfeeding or mixed feeding there could be additional some important findings put out.

Thank you for your suggestion. It would indeed be interesting to study the association between any of the predictors and “all” breastfeeding. However, the aim of this study was to assess exclusive breastfeeding as most data available from Lebanon suggests mothers have high breastfeeding initiation rates and acceptable breastfeeding rates; it is exclusive breastfeeding that is the challenge for Lebanese mothers. We feel it is more important to determine predictors of exclusive breastfeeding, as this is truly where the gap in knowledge and practice is and it is where the public health needs lie.

On page 7: The authors reported “Most (67%) mothers were breastfeeding at 8#12 weeks postpartum. Only 27.4% of mothers were exclusively breastfeeding at that time, 39.6% were giving both breast milk and infant formula while 33% were giving infant formula only”. This is a further indication that they could have
studied the association of the predictor variables with any form of breastfeeding or mixed feeding.

Please refer to above

“c-section” should be in full.

The term C-section is commonly used in scientific literature. We feel either the complete or the abridged form is appropriate. We have used the term caesarian section

On page 8: “Women who identified their mothers as their main source of emotional support were significantly less likely to exclusively breastfeed than women who reported their husbands or others as their primary source of emotional support (21.1% of compared to 31.8%). I suggest the proportions here are accompanied by the relevant confidence interval or p value as the presented figures do not tell the reader if the differences are statistically significant or not.

The p-values were included in the text as such: “Education, stress, and whether the infant was the result of a planned pregnancy were factors marginally associated with breastfeeding (p-value= 0.090, 0.092 and 0.068, respectively). The results of the bivariate analysis are shown in Table 1. 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. Mothers whose age was between 20 and 24 were more likely to exclusively breastfeed than others (p-value= 0.003). Working mothers, and those whose monthly household income was more than 2 million Lebanese Pounds (around 1,333 US Dollars) were much less likely to exclusively breastfeed than their counterparts (p-value< 0.001 and p-value= 0.019, respectively). Mothers who had a preterm delivery and those who underwent a Caesarian-section were much less likely to breastfeed exclusively (p-value= 0.036 and 0.016, respectively).

Intention to exclusively breastfeed was strongly associated with exclusive breastfeeding (p-value< 0.001). Thirty percent of women who intended to exclusively breastfeed at the time of delivery were doing so, compared to 6.9% of those who did not intend to. A significantly higher proportion of mothers who did not report any health problems in their newborn breastfed exclusively compared to those who reported that their babies had health problems (p-value= 0.003). Women who identified their mothers as their main source of emotional support were significantly less likely to exclusively breastfeed than women who reported their husbands or others as their primary source of emotional support (21.1% of compared to 31.8% p-value= 0.012).

Under results: Generally I realised the authors placed undue emphasis on reporting bi-variate associations. I recommend this is reduced in favour of outcomes based on multi-variate analyses.
The methodology used to decide which variables to keep in the multiple regression model is a well-recognized statistical methodology. We systematically eliminated variables based on the bivariate analyses. The choice of 0.1 as a significance level is to be conservative and this is based on a biostatistician recommendations. Other statisticians consulted advised to have a $P < 0.2$ for the selection of variables from bivariate to multivariate. SPSS 17 was used for analysis.

Where outcomes were described as being more or less likely to be observed in one group compared with other groups e.g. Nonworking mothers were 4.44 more likely to breastfeed than working mothers, I will suggest the respective odds ratios with their respective 95% CI or $p$ values are provided in parenthesis.

The odds ratios and $p$-values have been integrated into the text.

Under discussion: “In this study, almost all mothers intended to exclusively breastfeed; similarly to neighboring countries such as Syria and Jordan”. This needs to be reworded to read “In this study, almost all mothers intended to exclusively breastfeed, similar to observations in neighboring countries such as Syria and Jordan”.

This sentence was re-worded to “In this study, almost all mothers intended to exclusively breastfeed, similar to observations in neighboring countries such as Syria and Jordan”.

“It is noteworthy, however, more than two thirds of the women who intended to exclusively breastfeed were not doing so at follow up” should read “It is noteworthy, however, that more than two thirds of the women who intended to exclusively breastfeed were not doing so at follow up”.

This was corrected in the text.

“……resulted in this change of heart may uncover important elements that could prove useful to proponents of breastfeeding” should probably read “……resulted in this change of heart may uncover important elements that could prove useful for the promotion of breastfeeding”.

“Proponents of breastfeeding” was changed to “promotion of breastfeeding”.

“Although mode of delivery was not found to be significant in the regression model”. Please rephrase this.

This was re-phrased to ‘Although mode of delivery was not found to be a significant breastfeeding predictor in the regression model’.

If the authors become clearer on a more concise list of variables the tables for
distributions of the predictor variables within the study population and the tables for odds ratios could be combined.

We feel the list of variables chosen is justified based upon known breastfeeding predictors in the literature and based upon the complexity of the breastfeeding ecosystem.

Minor point: I personally don’t find the use of the words “catalyst” and “catalyze” very helpful as they suggest to me improvements in practice would not be possible without modifications in the factors mentioned. This may not be wholly true as these factors may not be the only influences on breastfeeding prevalence.

Breastfeeding has multiple predictors such that no one factor is solely responsible for the behavior. The term catalyst, however, does not imply an all-or-non response; it merely suggests a change in the factor mentioned could make it easier to achieve the desired behavior.

3-Reviewer # 2: Aamer Imdad

Thank you for your suggestions and comments. Kindly find the responses below

Major comment
1) If you are describing the results of interventions from primary trial, please describe more details of primary study. Also describe that how many women were included from the primary study? I would recommend to keep this study to describe the baseline demographics and breastfeeding prevalence rates and describe the results of interventions in a separate paper, otherwise give more details about methods like sequence generation, allocation concealment and blinding etc.

Thank you for your observation. The original RCT-where the methodology has been described in more details- has been submitted for publication. The sampling method was further explained in the text as such: “This was a secondary analysis of data from a randomized control trial (RCT) assessing the impact of a 24-hour hotline service and postpartum support film on postpartum stress. Based on power calculations for the original RCT, a total of 751 primiparous women were contacted: 119 were excluded and 80 refused to participate. There were no significant differences between the socio-demographic characteristics of women who participated and those who refused (data not shown).

Of the 552 women interviewed at baseline, 18% were lost to follow up and 442 women were assessed postpartum. There were no significant differences between who were lost to follow up and those who remained in the study (data not shown).

Minor comments
1) Is there any epidemiology data on proportion of home and facility birth in
Lebanon? Is it assumed in this study that all the births occur in hospitals in Lebanon?

The latest data available on this comes from the Pan Arab Family Health Survey; this data has been added to the text as such: “Close to 100% of all births in Beirut are attended by a skilled birth attendant. All hospitals (26 private and 1 public) with maternity wards in the city and its close suburbs were considered eligible for enrollment. Twenty-three (22 private, 1 public) hospitals agreed to participate. None of these hospitals implement the WHO/UNICEF Baby-Friendly Hospital Initiative”.

2) Was a pilot done for the data collection sheet?

Yes, a pilot was conducted for the data collection sheet and revised accordingly.

3) How was the sample size calculated for this study?

The sample size was based on a power calculation for the original RCT, which was submitted for publication. We recruited the minimum number of subjects required for adequate statistical power. A total of 751 primiparous women were contacted: 119 were excluded and 80 refused to participate. There were no significant differences between the socio-demographic characteristics of women who participated and those who refused (data not shown). Of the 552 women interviewed at baseline, 18% were lost to follow up and 442 women were assessed postpartum.

To further clarify, kindly find the power calculations for the original RCT below:

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, 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 recruited for each arm.

4) Did authors assess the breastfeeding status at six months too? If not, why not as WHO recommends exclusive breastfeeding till 6 months of age.

No, this point was further clarified in the text as “Exclusive BF at 8-12 weeks was chosen as an outcome rather than the standard 6 months defined by the WHO because the study was not originally designed to capture breastfeeding outcomes. Its results are nested within a larger RCT that aimed at assessing postpartum stress and depression rates at 8-12 weeks postpartum.”
5) Please report the p values in results section when you are describing the numbers for different variables.

The p-values have been integrated in the text