Reviewer's report

Title: Determinants of Breastfeeding in an urban population in Lebanon: a longitudinal study

Version: 1 Date: 20 November 2012

Reviewer: Gabriel Agboado

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Determinants of Breastfeeding in an urban population in Lebanon: a longitudinal study

This study may be of interest to readers and policy makers in the developing economies particularly in areas rapid technological and economic development.

Major Compulsory Revisions

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.

Introduction

Definitions: The authors’ use of exclusive breastfeeding will need to be defined.

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 behaviour in addition the overall prevalence e.g. influence of religion and paediatrician gender, cultural beliefs.

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

Method:

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.

I find the data description inadequate. For instance which pieces of information were obtained at baseline and which was collected post-partum?
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 randomised controlled trial. The randomisation was not described.

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.

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. Dichotomization 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

Results:

Earlier in the report the author’s stated the data came from a large randomised 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.

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.
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 was annual income, monthly income or daily income.

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.

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.

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?

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.

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.

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”.

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.
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.

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.

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.

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.

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

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 hrs”.

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.

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

• 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.

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.

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.

“c-section” should be in full.

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.

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.

Where outcomes were described as being more or less likely to be observed in one group compared with other groups e.g. Non-working 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.

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”

“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”

“……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”

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

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.

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.

Declaration
I have no competing interest

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

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