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.

Method:
Randomisation 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].

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].

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].

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].

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

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].

Grammar:
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].

Consistency in reporting:
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].

Inconsistency in the following: PSS10 versus PSS, SSTAI versus STAI [Minor revision].

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].

Other points:
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]

Table 2: Household income – this should be “monthly household income” [Minor revision]

Table 2 – Valuable, Invaluable: the latter means “extremely useful”. I suggest this be changed to “not valuable” unless the intended meaning is “extremely useful” [Minor revision]

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

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

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].

**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