Reviewer's report

Title: Incentives as Connectors: Perceptions of women and peer supporters participating in a breastfeeding incentive intervention in a disadvantaged area of North West England

Version: 1  Date: 16 December 2011

Reviewer: Heather Whitford

Reviewer's report:

General Comments
This is an interesting paper which explores a topic of relevance with clear clinical application. The question is well defined and well justified. The importance of the subject and the originality of the approach have been established. In general the relevance of the results is discussed in an unbiased manner and the findings are related to relevant literature.
However the length of the paper and the complexity of the description of the methods make it hard to read. With attention to the aspects below, the paper could be significantly improved.

Major Compulsory Revisions
1. Are the methods appropriate and well described?
There are some aspects where greater clarity is needed:
1.1. On page 11 - 2 women interviewed were still in receipt of the programme, though on page 10 it says that they had all completed the programme at the time of interview.
1.2. The section on Design on page 7 refers to ‘the incentive intervention’, but the intervention is not described till the next page. Perhaps the order should be reconsidered, or signposts are needed to help the reader.
1.3. Given that the primary aim of the study was the qualitative evaluation, what information were the women given about the evaluation/interviews at the point of invitation to the incentive programme? When was the consent form signed?
1.4. Did all the peer supporters offer the incentives? Only 4 (of 9 peer supporters in total) took part in the focus group? How were the peer supporters selected to offer the incentives? Might the characteristics of these peer supporters (or the areas they served) have influenced the outcome of the intervention?
1.5. Of women who were recruited to the interviews, some were apparently ‘selected’ – on what basis were they selected?

2. Does the manuscript adhere to the relevant standards for reporting and data deposition?
The main problem with the paper is the clarity in the way the quantitative findings
are described. There is much greater clarity in the qualitative data.

2.1. Given that the issue of deprivation/disadvantage is highlighted in the abstract and the discussion, is any data available about the women who declined to take part in interviews (or who could not be contacted)? Did the women who were included in the interviews differ (in terms of socio-demographics) from those who were interviewed? What about the demographics of the women who did not complete the programme (and discontinued breastfeeding)?

2.2. Table 2 – one of the columns is highlighted – the reason for this needs explained.

2.3. The rates used in the text on page 13 do not correspond to the data in Table 2 because 2 rows have been added together and the total for ‘any breastfeeding’ is given in the text.

2.4. There is inconsistency in the style of dates in the text and in the tables. Is the use of quarters necessary? This gets very confusing at times. It might be better to call them period 1, 2 etc. Or ‘before’ and ‘after’ intervention?

2.5. Page 14 confusing paragraph – the order of description in the text is sometimes counterintuitive with the ‘after’ occasionally described before the ‘before’

2.6. In Table 3 - what is the difference between ‘Fully or partially participating’ (‘fully’ is not mentioned in the description in the text on page 14) and ‘Completed full programme of support and providing some breast milk at 6-8 weeks’ in Table 3? The description in the paragraph duplicates the information in the Table and is confusing.

2.7. Table 3 ‘any BF’ is reported for those ‘fully or partially participating’, while only the rate for ‘exclusive BF’ is reported for those who ‘completed full programme’. Comparisons are therefore hard to make.

3. Are the discussion and conclusions well balanced and adequately supported by the data?

3.1. Page 28 – there is a comment that ‘it was not the gifts per se that motivated women to breastfeed’ – perhaps this should be qualified by ‘in this group of women’ as these women may have been very motivated in the first place. No conclusions can be drawn about how the gifts may have motivated women who did not complete the programme (and were not interviewed).

3.2. Page 28 ‘the vital sign’?? - but this statistic is not revisited so the reader is left unclear about which percentage is meant.

3.3. Page 29 – this section of the discussion seems disconnected to the rest of the discussion. Can these conclusions be drawn from the findings? It is not clear that (for example) autonomy was enhanced in the ways suggested.

4. Are limitations of the work clearly stated?

4.1. More needs to be made of the major limitation of the paper (that the women who discontinued breastfeeding are not represented).

4.2. Sources of bias – might GT’s detailed knowledge of the programme have led
to bias in the analysis?

5. Do the title and abstract accurately convey what has been found?

5.1. The title and abstract both mention the issue of ‘disadvantage’; however the sociodemographics of participants are not reported.

5.2. The title is slightly misleading, as only the perception of women who ‘successfully’ completed the intervention are represented.

5.3. Similarly the title gives no indication that quantitative findings are also reported.

5.4. Page 27 – refers to ‘the first qualitative study’ – but quantitative findings are also reported.

Discretionary Revisions

- A flowchart to summarise recruitment/drop-outs would be helpful.
- Was there any discussion in the focus group about relationships with women who struggled with breastfeeding and who discontinued? Also regarding the process of stopping visiting/giving the incentives? This aspect would seem to be of great interest (particularly as the voices of these women are absent from the paper).

- Does the data lend itself more naturally to 2 papers?

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I have been PI on a CSO funded study (CZH/4/568) exploring predictors of infant feeding. I have also been in preliminary discussions with one of the authors of this paper (PH) regarding the potential for future collaboration.