Reviewer's report

Title: Women focussed development intervention reduces delays for accessing emergency obstetric care: a population based study in urban slums

Version: 1 Date: 23 September 2010

Reviewer: Pierre Fournier

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Research question:
The subject is relevant but the research question is unclear, according to the summary the purpose of the study is “to observe … Manoshi”. The conclusion is “the results supported the impact of Manoshi …”. How to conclude on the impact of an intervention after an observation?
The main objective is not clear and the methods used (including statistical analyses) are not coherent with what seems the implicit objective (impact or effects analysis).
This study is not a population based one: in the two groups, the cases are selected through medical records or registries. A population based study has population representative samples.

Statistical methods:
Statistical methods are too basics. Using only bivariate analysis even with some stratification does not allow to take in account the effects of many factors on the 1st delay (place of delivery, effective support from Manoshi program including financial support [we know that women who received financial support took less time to decide to seek care but we don’t know if it was different according to the case mix, the place of delivery …]).
The comparison of crude median 1st delay between the two groups is irrelevant given the major differences in cases mix, the medians should have been adjusted before being compared.

Data:
The way the two groups were selected is relevant but among these groups some women had an obstetrical complication some other not (82%/18%). We don’t what was these rates in the two groups and for some tables using only percentages (5, 6) we don’t know to which group or sub group it refers.
It is confusing to see that table 2 refers to 450 (225+225) women with obstetrical complication while in the text the authors says that “Most respondents (82%) experienced obstetric complications…”. 396 women had obstetric complications?
If the purpose of the study is to show that Manoshi had an effect on reducing the first delay, only obstetrical emergencies cases should be used as in other case the potential effect is not relevant.
If we assume that the implicit objective is an effect demonstration, the relevance of the data should be questioned. Both groups (home and DC delivery) benefited from the program. The main difference is that the delivery took place at home or in a setting supported by the program. It is expected that 1st delay is shorter when delivery is assisted by more qualified staff. This is not a discovery. Data show clearly that the case mix is different in the two groups. It should have been interesting to compare life threatening emergencies in the two groups depending on financial support provided by Manoshi.

Does the manuscript adhere to the relevant standards for reporting and data deposition?
No

Are the discussion and conclusions well balanced and adequately supported by the data?
No

Are limitations of the work clearly stated?
The authors refers to the limitations of a cross sectional study but as the objective is not clear it is difficult to say if these limitations are really taken in account.

If we stay in the observational area, a case-control approach should have been more appropriate.

Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
Most references are from empiric work done in Asia and in Africa, they are relevant. May be too many references come from reports for which access is not specified. Major work including synthesis internationally acknowledged are not cited. For example while the work is built around the concept of the three delays, the seminal paper introducing this concept is not cited (Thaddeus, S. & Maine, D. (1994). Too far to walk: maternal mortality in context. Social Science & Medicine, 38 (8), 1091-110). The Lancet papers on maternal mortality by The Lancet Maternal Survival Series steering group (2006) should also have been cited.

Do the title and abstract accurately convey what has been found?
No, the title and the abstract give the impression that the effects of the program is demonstrated and that is not the case.

Is the writing acceptable?
Yes

Recommendation:
This paper needs major compulsory revision or should be rejected.
In case of revision they are 2 options:
(1) making it only descriptive and using more qualitative material gathered during
interviews, showing the mechanisms by which the Manoshi program reduced the delays (better identification of complications and/or relevant financial support for the poorest household, etc…), the specific groups who benefited more of the program, the specific circumstances in which the program is most appropriate, etc…

(2) trying to be more analytic by using a relevant subset of data (only emergencies) and taking in account simultaneously all the factors influencing the 1st delay. In this case I can see a challenge not addressed by the authors. It is particularly difficult to quantify the first delay when it is reported by lay people because the beginning of the delay is self assessed and different depending to the type of complication. An hemorrhage could be easily identified but it is more difficult with obstructed labor (this category accounts for 60% of the DC delivery group). Usually a list of symptoms should be used for each type of complication to decide on a comparable basis when the beginning of the complication occurred.

The subject is highly relevant. In many settings in the world where women deliver without professional assistance, recognition of an obstetrical complication is a major issue. Any evidence on the way to reduce the risk incurred by these women is of importance. Given the very high relevance of the subject, the authors should be asked to revise their paper.

Declaration of competing interests:

I declare that I have no competing interests.