Reviewer's report

**Title:** Perinatal mortality in rural Burkina Faso: A prospective community-based cohort study.

**Version:** 1  **Date:** 4 May 2010

**Reviewer:** Sabine Gies

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In this secondary analysis of a prospective cohort Diallo et al present important data on perinatal mortality in a rural area in Burkina Faso and associated risk factors. Despite the relatively small sample size and the cluster design limiting statistical significance, the follow-up of such a cohort under the given field conditions is extremely demanding and study team must be congratulated for collecting and presenting this data. The paper is clearly written.

However, for a better understanding of the study context, some additional information on the methodology and analysis are required.

**Major**

1) **Methods - Selection of participants and sample size**

The sample of 900 women was enrolled from a total of 1162 identified pregnancies. Of these, 21 declined but it is not stated how many did not fulfil the inclusion criteria. Were the 241 women not enrolled different from those enrolled? Enrolment was over a one year period but how were numbers distributed over this period? Were the numbers related to village size?

2) **Methods - Type of intervention of the main study**

It would be helpful to have some more information regarding the peer-support for exclusive breast-feeding apparently implemented in 12 of the 24 clusters. Was this intervention individual or community-based? When did it start? By whom was it conducted?

3) **Analysis**

a) Although one would not expect exclusive breast feeding to have a direct impact on stillbirth, there could well be an impact of the intervention (or increased attention to pregnant women and delivery) on early neonatal death. The intervention arm should therefore be presented in Table 1. Also in Table 2, the allocation of villages to intervention arms would be helpful.

b) Including nulliparous women in the variable “history of previous perinatal death” seems not correct to me. While I agree that previous death is a high risk factor for PNM, this can only be assessed in parous women and therefore cannot be included in the same multivariable model. I suggest correcting the figures in Table 1 and removing the variable from Table 2 and maybe present a separate
model excluding nulliparae.

c) Were tests for interactions between variables performed, i.e. between the number of antenatal visits and season of birth, place of birth and distance to the nearest health centre?

4) Discussion

a) Paragraph 2 – Prospective studies are rare but there is one conducted on malaria prevention in rural Burkina Faso (Boromo) that also reports on miscarriage and stillbirth (Gies et al, Malaria Journal 2008).

b) Paragraphs 9 and 11 – I think that the issue of maternal malaria is not sufficiently broadly discussed and the last sentence of paragraph 11 does not appear correct to me. Women delivering during the dry season are very likely to have been exposed to malaria during the first pregnancy trimester which falls into the rainy or early post-rainy season known to be high malaria transmission periods. So the high risk of primi- and secundigravidae could still (partly) be explained by malaria, especially if malaria prevention was insufficient in the present study. Moreover, the Boromo study showed a significant effect of season of delivery on the uptake of antenatal services including IPTp-SP (Gies et al, AJTMH 2009). This should be taken into account in the discussion.

Minor

5) Table 1

The three administrative areas are not referred to in the methods section

6) Discussion – paragraph 3

Almost 30% of delivering women not having attended ANC is extremely low and in contrast with published health system statistics. ANC is free of charge in Burkina Faso since 2003 and in my experience, most women attend ANC at least once. Is there any explanation for this extremely low attendance in Banfora?

Discretionary

7) Introduction - paragraph 3 and Discussion – paragraph 6

Paucity of reliable data is deplored in the introduction but then WHO estimates are confirmed in the discussion, so apparently they were not that unreliable?

Level of interest: An article of importance in its field

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