Reviewer's report

Title: The effect of antenatal care on perinatal outcomes in Kwale district, Kenya: cohort study

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Reviewer: Anna Maria van Eijk

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General
By Brown et al.

This is a cohort study among pregnant women, followed until delivery. The authors assess factors associated with ANC visits, and the association between birth outcome and ANC visits. The manuscript is written in a pleasant style and gives the impression of a thorough analysis. However, there are some important limitations.

1) Results: If this is a cohort study, it would be pleasant if they could give a better impression of the flow. I suspect that none of the pregnant women was lost to follow up, but since this is not really described, I would prefer if this is explicitly mentioned in the results section. E.g. xx pregnant women were identified during the 4 monthly census in the yy households visited, and zz (or all of them) were followed until delivery. How frequently were these pregnant women visited? (Or: what is meant with “kept track”?) Was this monthly, weekly? Every time they were visited, was there a questionnaire conducted? Were there any benefits for the pregnant women to participate? How was the final number of ANC visits and pregnancy outcome ascertained? Were the ANC cards checked? (If yes, was there no information on gestational age by palpation on the card at the time of the visit? And on gravidity?) Was there also an interview conducted postpartum? How was the birth weight measured for the home deliveries? Were the birth weight measurements at home comparable with the birth weight measurement in the facilities (in method and outcome)? Was this done by the enumerators?

2) The following important pieces of information are missing: age, and gravidity of the participants, other services offered in the ANC such as hematinic supplementation, and trimester or gestational age at first visit. In particular gravidity and trimester of first visit would have been very important factors to add to the models.

It is very difficult to assess factors affecting birthweight/low birth weight if gravidity is missing. Primigravidae are more likely to have lower birth weight than multigravidae. This effect may become more pronounced if malaria is an important factor in the area; however, use of ITNs and IPT may reduce that effect. In some areas of Kenya, primigravidae are known to be more likely to visit an ANC and deliver in a health facility. If this is a cohort study, one would not expect this lack of important pieces of information. The authors need to comment on these limitations.

3) In addition, the definition of “good pregnancy outcome” (not an abortion, miscarriage or stillbirth), makes it possible that more ANC visits are associated with a good pregnancy outcome, simply because the pregnancy may have lasted longer when there was no abortion or miscarriage and women may have been more likely to be able to visit the ANC. So it is not clear what is measured here. It would be better to compare “good pregnancy outcome” with stillbirths only, and see if the relationship still holds. The working definition of an abortion or miscarriage would be helpful here. However, even among stillbirths, prematurity may be an important factor, so still you cannot exclude that “good pregnancy outcomes” are associated with ANC visits, simply because the pregnancy lasted longer. Again, gravidity may have been an important confounding factor here, but was not taken into account (e.g. women who had an experience of a normal delivery before, may be less likely to come for more than one or two visits to the ANC). The authors need to comment on this.

4) It is interesting to see there are differences between areas in birth outcome and low birth weight, but it would be helpful if was explored for an explanation, e.g. is there more malaria and less ITN/IPT coverage in one area compared to the other? What other sources of IPT/TT were there in the area except from the ANC? The authors need to comment on these limitations.
5) The authors have to be very careful with their conclusion: ANC visits may not be the cause of the “good birth outcome” but merely a marker for women who take good care of themselves and their pregnancies (as they mention in their discussion). So it is not clear if improving ANC visits may be indeed improving the “good birth outcome”, also because of the limitations described above (the definition of good birth outcome). This carefulness needs to be expressed in title and conclusion of the abstract.

1. Is the question posed by the authors new and well defined?
Not new, but well defined.
Questions: Determinants of attending ANC
Effect of attendance on behavioural and perinatal outcomes (live births and healthy birth weights)

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
No

3. Are the data sound and well controlled?
No

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes

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

6. Do the title and abstract accurately convey what has been found?
No: a more careful phrased title and conclusion in abstract is warranted (e.g. Antenatal care and perinatal outcome in Kwale district)

7. Is the writing acceptable?
Yes

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

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 declare that I have no competing interests