## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

## ARTICLE DETAILS

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| **TITLE (PROVISIONAL)** | Intrapartum and neonatal mortality among low risk women in midwife-led versus obstetrician-led care in the Amsterdam region of the Netherlands: a propensity score matched study |
| **AUTHORS** | Wiegerinck, Melanie; van der Goes, Birgit; Ravelli, Anita C.J.; van der Post, Joris; Buist, Fayette; Tamminga, Pieter; Mol, Ben |

## VERSION 1 – REVIEW

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| **REVIEWER** | Frank Chervenak<br>Weill Medical College of Cornell University<br>New York, NY, USA |
| **REVIEW RETURNED** | 04-Aug-2017 |

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| **GENERAL COMMENTS** | This paper represents an important contribution to the literature on the controversial subject of planned home birth. It is well-written.<br>Abstract and introduction are appropriate. Methods including statistical analysis are appropriate. Results are clearly stated in the text and tables.<br>The discussion follows the data presented, is not overstated, and is clinically relevant.<br>Limitations are clearly mentioned. |

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| **REVIEWER** | Beverley Lawton<br>University of Otago, Wellington<br>New Zealand |
| **REVIEW RETURNED** | 27-Aug-2017 |

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| **GENERAL COMMENTS** | This paper chooses to look at whether there is a difference in peri natal outcomes depending on which system of care term at the commencement of labour (midwifery led or specialist team).<br>It adds a valuable contribution to the present literature looking at pathways and systems of maternity care although the limitations need to be further clarified. This is a retrospective cohort design and as such it can only show an association and this should be clarified in the abstract.<br>comments<br>1. It would be helpful to understand why the authors chose labour and not the whole pregnancy continuum which would examine the care pathway from time of booking.<br><br>2. How do the authors know who was booked at the beginning of their care with the two options or how many had transferred?<br><br>3. How many of the midwifery led care were general practice care? Can these be separated out? Do general practitioners team with midwives.<br><br>4. Did you look at overall peri natal mortality at term for women |

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| | booking with the two options and why not?<br><br>5. What about neonatal encephalopathy as an adverse outcome. This was increased in the Wernham comparison between midwifery led care and medical led care. 1<br><br>6. Post-partum haemorrhage – a definition would be useful and also a look at the greater than 1000mls loss.<br><br>7. The 23 neonatal deaths – how many were admitted to ICU and how many were delivered by caesarean section.<br><br>8. For low NICU rates for midwifery care -does this reflect the availability . for example birthing units may be distant from a NICU and the same applies for epidural – is this equally available<br><br>9. 4% of births not in PRN database – do you know anything about these missing maternities? Is this a limitation<br><br>10. How did you account for smoking, maternal obesity etc. If these are not accounted for it's a serious limitation<br><br>11. Typo page 25 table 3 under midwife led care should read 9520? |

**VERSION 1 – AUTHOR RESPONSE**

Reviewer #1 ( Frank Chervenak )
"This paper represents an important contribution to the literature on the controversial subject of planned home birth. It is well-written. Abstract and introduction are appropriate. Methods including statistical analysis are appropriate. Results are clearly stated in the text and tables. The discussion follows the data presented, is not overstated, and is clinically relevant. Limitations are clearly mentioned."
>We would like to thank dr Chervenak for reviewing our paper. No alterations were made based on his comments.

Reviewer #2 (Beverley Lawton)
"This paper chooses to look at whether there is a difference in perinatal outcomes depending on which system of care term at the commencement of labour (midwifery led or specialist team). It adds a valuable contribution to the present literature looking at pathways and systems of maternity care although the limitations need to be further clarified.
This is a retrospective cohort design and as such it can only show an association and this should be clarified in the abstract."

> We thank dr Lawton for her comments. We propose the following modifications.
• We changed the conclusion of our abstract to 'Among low-risk women, midwife-led care at the onset of labour resulted in was associated with a statistically non-significant higher mortality rate'.
• We changed the conclusion of the discussion (last line) to 'While our findings were not statistically significant and do not allow for conclusions regarding causality, the results are concerning and warrant further evaluation of the underlying causes and replication.' (see page 18 line 326 of the revised manuscript)

Reviewer #2 comments that it would be helpful to understand why the authors chose labour and not the whole pregnancy continuum which would examine the care pathway from time of booking.

> We agree that it would be interesting to review the whole care pathway from time of booking, especially because we want to examine outcomes in different levels of care. However, the question on the best place of delivery given a low risk pregnancy at the start of labour should in our opinion be answered separately from the best care for pregnancy as a continuum. Secondary arguments that our registration systems are less accurate in discriminating between line of care during pregnancy. Also, we started this endeavour to replicate the study of Evers et al who found higher perinatal mortality in women starting labour midwife-led care. Therefore, we were interested in the specific research question of intrapartum and neonatal mortality in midwife-led versus obstetrician-led care.

Reviewer #2 wonders how the authors know who was booked at the beginning of their care with the two options or how many had transferred.
> Line of care at the onset of labour is registered in the Perinatal Database (PRN). We audited all mortality cases for line of care at birth and performed a validity check for the denominator. Based on a random sample of 100 records in the obstetrician-led care group without perinatal mortality, we performed a sensitivity analysis to account for the possible 12% misclassification in line of care at birth (had actually started labour in midwife-led care) and a possible 13% under reportage of risk factors, which did not lead to different conclusions. Mortality rates were then 0.62‰ in the midwife-led care group versus 0.25‰ in the obstetrician-led care group (OR 2.5; 95% CI 0.6-10.4). Appendix A describes our random sample results and additional analyses.
The referral rates were calculated based on the actual place of birth. We have made no changes to the manuscript.

Reviewer 2 would like us to elaborate on how many of the midwifery led care were general practice care and whether these can be separated out.
> Data from general practitioners as primary caregivers in maternity care were not included in our population. That information was not available at the time. Furthermore, it only accounts for an estimated 1% of births in the Netherlands. Also, care provided by a general practitioner was not part of our current research question. We clarified it in the methods section by adding ‘Data from the pregnancies with a general practitioner as the primary maternity care giver (without referral to specialist care) were not registered in the database and therefore not included.’ (see page 5 line 34 of the revised manuscript)

Reviewer 2 asks if we looked at overall perinatal mortality at term for women booking with the two options and why not.
> This is an important question. We did not include antepartum mortality. The primary motivation was a principal one, because our current research question focussed on mortality after the onset of labour. We added ‘For this study we focused on mortality after the onset of labour, meaning that we are unable to draw any conclusions regarding antepartum fetal death and total perinatal mortality. It would be interesting to review this in a separate study, as antepartum death is the largest group.’ (see page 15 line 258 of the revised manuscript)

Reviewer 2 suggests neonatal encephalopathy as an adverse outcome, as this was increased in the Wernham comparison between midwifery led care and medical led care.
> We thank the reviewer for the suggestion. Unfortunately, we were unable to reliably retrieve this information from the database and records. We have made no changes to the manuscript.

Reviewer 2 advises to add a definition of Post-partum haemorrhage and also to look at the greater than 1000mls loss.
> We agree with the need for a definition of Post-partum haemorrhage and added the following to our study: ‘hemorrhage was defined as reported blood loss ≥1000 ml’ (see page 7 line 84 of the revised manuscript)

Reviewer 2 would like us to further specify the 23 neonatal deaths in how many were admitted to ICU and how many were delivered by caesarean section.
> The following text was added ‘Seven children (22%) were born after caesarean section and six (19%) after instrumental delivery. Nine neonates (28%) were admitted to a NICU for at least 24 hours.’ (see page 12 line 209 of the revised manuscript)

Reviewer 2 wonders about the low NICU rates for midwifery care, and whether this reflects the availability (for example birthing units may be distant from a NICU and the same applies for epidural), or if this is equally available.
> We know that NICU admission is not a good outcome to measure morbidity when comparing different levels of care (see discussion line 307), because hospitals with NICU facilities also have a local medium care function which is not separately reported in the national register. Furthermore, even though distances and therefore travelling times in the Amsterdam region are relatively short, it is possible that availability is an issue.

Reviewer 2 comments on the 4% of births that are not in PRN database and asks if we know anything about these missing maternities and if this is a limitation.
> We know that in 2007 a total of 94% of all midwifery practices supplied data for the PRN database, 99% of the obstetric units, 100% of the neonatal units with NICU facilities and an estimated 68% of all paediatric facilities reporting admission between the first day of birth up to 28 days. These data registries are linked, resulting in a total coverage of 96.5% of births in 2007.
The missing 3.5% consists of midwifery practices that did not register to the national registry (including home- and midwife-led hospital births), 1% records of general practitioners (who did not register is these years) and records missing due to data registration problems. We do not believe that addition of these data would alter our findings. No adjustments were made to the manuscript.

Reviewer 2 asks how we accounted for smoking, maternal obesity etc, because she considers it a serious limitation if these are not accounted for.
> We thank the reviewer for addressing this important topic. Smoking and BMI were not yet available from our perinatal registry (PRN) and were therefore not included in our study. This is indeed a limitation that we now have addressed in our discussion. “Furthermore, certain important risk factors for perinatal mortality such as level of education, smoking during pregnancy and maternal body mass index, were not available from the registry and therefore not included in this study.”(line 239)

Reviewer 2 noticed a typo on page 25 table 3 under midwife led care where it should read 9520.
> Thank you. This typo has been corrected.

We thank the reviewers and editorial team for their comments, and we hope that our adjustments have made the manuscript suitable for publication. We look forward to your final decision on our revised manuscript.