Letter to the Editor

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Clarification of the methods and statistics in the study "Planned home birth and the association with neonatal hypoxic ischemic encephalopathy"

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To the Editor,

I have with true interest read the recently published paper "Planned home birth and the association with neonatal hypoxic ischemic encephalopathy" [1] in the *Journal of Perinatal Medicine*. However, to me, the study in its present form has caused some confusion, and I hope that you will be able to provide me with further clarification of selected issues that I will elaborate on below. In general, the study uses a very small sample size to study the relatively rare outcome of neonatal hypoxic ischemic encephalopathy (HIE). It is, thus, essential to make a thorough plan for analyses and draw cautious conclusions.

The main results from the study described in the abstract indicate a higher risk of HIE in infants born outside the hospital setting. The two odds ratios presented are 44 (for planned and unplanned home births) and 21 (for planned home birth), respectively, but with identical confidence intervals (CI), indicate that a least one of the calculations is incorrect. Further, the CI is 1.7–256.4, making it obvious that the result presented is determined with high uncertainty, increasing the risk of type 1 error.

In table 3, patient characteristics for infants born outside hospitals are presented. Three cases were not planned home births. Also, three births outside hospital were not attended by midwives. Two of these were unattended and one was attended by EMS. I assume EMS is "emergency medical staff", but I am not sure, as the abbreviation EMS is not explained in the paper. There seems to be a mismatch regarding exposure status (both planned and unplanned home births, and attended and unattended births), which makes it difficult to draw reliable and representative conclusions.

No information about the educational level of the attending midwives was provided (are they certified?). Moreover, I believe information about efficiency of transfer and initiation of treatment of the infants transferred from home to the hospital would have improved the quality of the study. Delays in transfer or initiation of treatment may affect the results negatively.

It is described that logistic regression was used to determine the adjusted odds ratios. However, it is stated that controls were matched on year of birth, geographical location, and gestational age. The correct regression model to use in a matched case-control study design is the conditional regression model and not logistic regression.

It is hard to identify the exact number of variables used in the adjusted model. The list of potential confounders mentioned in the methods section is long and the reader is left with the impression that (too) many confounders were included in a relatively small study, which increases the risk of a random result. Many of the mentioned confounders are properly correlated, making a strategy for the analysis necessary. The present sample size may generally allow four to five confounders to be added in the adjusted analysis to ensure a reproducible result [2]. Finally, a follow-up status on the infants, at least until discharged, would have improved the study.

To sum up, I believe there are a considerable number of methodological irregularities in the published paper and I wonder why this has not been pointed out in the review process.

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It is of paramount importance that we, as health professionals and researchers, collaborate together to provide safe clinical practice and conduct high-quality research to support women in their choices related to the birth process.

Author's statement
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References

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