Reviewer's report

Title: Risk-adjusted operative delivery rates and maternal-neonatal outcomes as measures of quality assessment in obstetric care: a multicenter prospective study.

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Reviewer: Jennifer Lutomski

Reviewer's report:

I would like to thank the authors for the opportunity to review this interesting paper on the value of measuring both caesarean and operative vaginal birth rates in regards to assessing quality of care. This is a topical discussion point in obstetrics, and determining the best methods to assess quality of care is needed. I agree with the authors that examining operative vaginal rates is a useful marker which is not always addressed. Developing a regionalised database for the prospective collection of data and performing regular audits on data quality were major assets of this study. As the authors aptly pointed out in their discussion, many obstetric research questions utilise retrospectively collected administrative databases which carry inherent biases. Nonetheless, I do have some comments.

MAJOR REVISIONS

I think the methods could be expanded in this manuscript. Better description of the prediction model would help clarify this work to the reader. I do not feel that it immediately comes across to the reader that adjustments are being made for obstetric volume and that is a critical point.

I find some of the comparisons in the mode of delivery to be less than ideal. I think the authors should state the tearing is a condition of a vaginal birth and bladder injuries are largely a result of caesarean delivery. In this regard, the authors should state that if a condition is inherent of a mode of delivery, then no comparative analyses were performed.

Are the authors able to distinguish emergency versus elective caesarean deliveries? I think this would warrant an important sub-analysis. In this context, I would also recommend an analysis inclusive and exclusive of twin births. Although the numbers are likely small, I think twin births are a residual bias in the analysis.

I think there is an over-reliance on p-values throughout this paper. Although the authors remark that this is a relatively small birth cohort (which is true in regards to other obstetric studies), statistically, this number is quite large. For instance, in Table 2, the minor PPH rate varies between 2.32 to 2.48 and this shown to be a significant difference. However, clinically-speaking this is not a large difference – and this is a slippery slope when attempting to distinguish quality of care between units (which in essence what the authors are trying to convey). I think all
the p-values should be removed from Table 2. In this regard, I think it would be clearer if the authors could include funnel plots (perhaps just of the composite rates) for the hospitals.

Perhaps I have missed this in the paper, but I also have a query regarding directionality in the case of neonatal outcomes. Could it be the hospitals with better/more advanced NICU services are the units with above-expected caesarean rates? I recognise that adjustments were made for neonatal complications; still, wouldn’t it be expected, for example, that infants delivered via caesarean have lower Apgar scores and higher admission rates to NICU? Infants at higher risk are more likely to be delivered via caesarean, and I don’t understand how this is a marker of quality of care. Residual confounding is potentially still an issue could obscure this association. Reassessment of neonatal conditions with justification of inclusion would be helpful. This point could also be explored in greater depth in the Discussion. I think this is an important point since the goal is for other obstetric units to monitor deliveries using methods applied by the authors.

Given that this is a cohort study, converting the ORs to RRs is methodologically robust. However, there are methods superior to the Zhang conversion. Did the authors attempt a log-binomial regression or a Poisson regression with robust variance? There may be an issue with convergence, however these methods are preferable.

In the discussion, the authors explain that the impact obstetric volume differed relative to other studies. I think there should be shown in the results section. This would be the added benefit of the funnels plots for readers; they would be able to quickly and easily assess the impact of obstetric volume. Moreover, it is easier to visualise inter-unit variation.

MINOR REVISIONS

Introduction, Line 82: I find the phrase “reflect efficient and appropriate care” misleading – I think I would prefer reflect “evidence-based intervention”.

I believe it should be very clear in the introduction if adjusted rates in other studies are solely adjusting for obstetric volume, maternal risk factors or both.

Introduction, Line 110: Instead of “our Region” the north-eastern region of Italy would be clearer for readers.

Methods, Line 115: 18-month (singular); Also, I am not clear when the study was carried out. Would you please add the start and end date?

Methods: Would the authors please specify if nearly all births are captured for the region? It appears so, but it would be interesting to know, for example, the home birth rate or if there are midwifery-led centres in the region. I think it would be also useful to clearly state that these are all hospitalised deliveries (or at least that is what I presume since they were all classified as Level I or Level III units).

Methods, Line 143: Would the authors cite the guideline used in Italy for PPH?
Some guidelines do distinguish between blood loss between vaginal and caesarean births.

Methods, Line 180: When is maternal BMI taken? This could have an impact on BMI figures. Typically, if recording pre-pregnancy BMI, <25kg/m2 or less is healthy weight and 25 to <30 is overweight and >=30kg/m2 is obese. BMI thresholds are slightly different in this paper, so a citation would be helpful.

Methods: What infants were included in this analysis, i.e. what are the gestational age and/or birthweight thresholds? What viability cut-off is used for this study? It seems that there was only a gestational age/birth weight restriction for admission to NICU. I do not understand why this was the case and think this restriction warrants further explanation in the methods.

Methods, Lines 210-213: Weren’t the data also adjusted for gestational age?

Results: There is too much information on Table 1, impeding readability. I would suggest splitting the table into two separate tables. Table 1 would be the incidence rates and Table 2 would be the risk ratios. Moreover, the p-values should be removed from the table and replaced with the 95% CIs. The p-values are essentially meaningless, and better interpretation of the data can be yielded with the CIs.

Results, Line 242: It would be helpful if the authors would more clearly state how the bivariate and multivariate analyses differed. Across what specific components?

DISCRETIONARY REVISIONS

I’m not sure the title does the manuscript justice.

The CI-ranges can be added to Figure 1.

Methods, Lines 182-183: The sentence structure switches which makes it a bit confusing. To keep the flow, it would be better to say parity (reference multiparous, nulliparous), gestations (reference singleton, twin)

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests