Reviewer’s report

Title: Childbirth assistance in Catalonia. An analysis of obstetric interventions in two groups of hospitals

Version: 3
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Reviewer: Louis Kollee

Reviewer’s report:

Discretionary revisions:
The manuscript is well-written and easy to read, but with quite long sentences and repeated statements. If required by the journal, it can be shortened. Throughout the manuscript, “births attended to” is used. Why not just “births”? Several references are still in press and many of them are in Spanish. Is this accepted by the journal?

Major compulsory revisions:
For births being recorded as normal, there should be a cephalic presentation of the fetus. However, in the other analyses the authors did not include cephalic or breech presentation, despite the fact that this may influence the decision for operative delivery. Also, under Limitations, it is stated that births surpassing 40 weeks of gestation cannot be recorded with the code “normal birth”. This is remarkable. Does it mean that the total number of “normal single vaginal births reported in MBDS 37-42 weeks of pregnancy” in the 3 phase analysis (figure 1) in fact only includes 37-40 weeks (and only cephalic position)? If so, is the analysis of this group of births really worthwhile?

The authors do not explain the differences in funding characteristics between private and public hospitals. For example, is funding based on admission and number of days in the hospital or also on interventions performed? If so, this could play a major role in some of the differences in practices found between both types of hospital.

Minor essential revisions:
Background section:
Paragraph 2, sentence 2 is not completely clear and should be revised.

The public network of Catalonia contains 43 hospitals with a mother and child service. However, In the Methodology section, it is stated that information was included from 44 state-assisted hospitals offering public service. This is confusing.

Table 1: the percentages of 24.0 and 10,41 normal births in private and public hospitals respectively is striking. This was not sufficiently discussed in the discussion section.
Results section:

Paragraph 2, sentence 2 (“This difference is not …”) is not completely clear and should be revised.

Table 2 a: “Hospitals with a higher volume of activity were found to have a raised rate of Caesarean section”. However, this is not sufficiently clear from the table. Is there any statistical significance?

The episiotomy rates were calculated in both the group of single births (phase 2) and “normal” single vaginal births (phase 3). This could be mentioned more clearly in the Methods section and the (confusing) findings (probably in part due to underreporting) could be discussed separately in the Discussion section.

Why was parity not included as a variable?

In the Discussion section, under Records, it is stated that “of the 60 hospitals included, 8 did not record …”. I do not understand the number of 60, since 64 hospitals were included in this study.

**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:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.