Reviewer’s report

Title: De-medicalization of birth by reducing the use of oxytocin for augmentation among first-time mothers - a prospective intervention study

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Reviewer: Anna Dencker

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De-medicalization of birth by reducing the use of oxytocin for augmentation among first-time mothers - a prospective intervention study

This is a very interesting and important subject to study but I would like to raise some questions, especially about methods and discussion. I have listed comments and questions below.

Title
* This is a complicated title. Is this really about de-medicalisation of birth?

Abstract
* Please coordinate hypotheses, aim and outcome measures between abstract and background.

Background
* As above. Please coordinate hypotheses, aim and outcome measures between abstract and background.

Methods
* It is very important to make sure that the group of women (Robson 1) studied before and after the intervention are comparable not only on background characteristics as shown in Table 1, but also that there has been no change in identifying the Robson 1 group. It is possible that the change in dystocia definition could affect a change in definition of spontaneous start or induced labour, for example how to care for women with broken water and no or weak contractions. Therefore, it is important to know how many of the primiparous women at the
clinic were defined as Robson 1 before and after, and how many were defined as Robson 2. Please add this information.

* A majority of primiparous women with spontaneous onset of labour had oxytocin treatment for dystocia before the intervention, which indicate a major overuse of oxytocin. Did the authors think that the change in dystocia definition should avoid most of this overuse?

* The definition of active labour and also the definition of dystocia seem consistent with the one used in active management of labour (AML). One component (probably the most important in AML to get good results) in AML is continuous support to the women in labour. I think it is relevant for this paper to add information about the clinic's guidelines of labour support and if it changed between the measure point. How was these definitions chosen (for active labour and dystocia)?

* What is the most common definition of active labour in Norway? Is there any national cooperation between midwives and doctors about definition of active labour?

* What is the most common definition of dystocia in Norway? Again, is there any national cooperation between midwives and doctors about definition of dystocia?

Statistical analyses

* Please explain p-chart on line 4.

* I am not convinced that predictors for operative vaginal birth (OVB) should be compared to spontaneous vaginal birth (SVB) + caesarean section (CS) together. SVB and CS are two very different outcomes.

* What is the point in doing a logistic regression adjusted for only one confounder (labour duration) when you had more known confounders (BMI, birth weight, epidural)? If the numbers are too small maybe you should refrain from the analysis? What happens if you try to use more confounders? And why use labour duration if only one confounder could be used? Is labour duration the most important confounder?

Results

* Please add description of the total number of primiparous women during the measure periods. How many of these were defined as Robson 1 or Robson 2?

* How did you choose the number 431?
I would recommend a more careful use of "dystocia". Surely not almost 70% of the women in the before-group had dystocia but dystocia according to the clinical guidelines?

More women "without dystocia" had oxytocin treatment after the intervention. A more careful use of "dystocia" would make this less confusing.

Fewer women had oxytocin treatment in the 1st stage of labour after the intervention. Why is this important?

Fewer women had oxytocin treatment before dilatation of 6 cm after the intervention. Why is this important?

In last paragraph it is said that you compare mode of delivery in all women augmented with oxytocin. Do you mean compared to those not augmented?

Discussion

In first paragraph: use of oxytocin should go down as hypothesised (1st hypothesis), if most staff follow clinical guidelines. But the increased use of oxytocin when "no dystocia" is maybe a natural effect of changed routines? This possibility is only briefly mentioned, please elaborate more on the effects of the changed guidelines for the total group of women.

The second hypothesis is quite complicated and not mentioned in the discussion. Can this study answer to the second hypothesis?

Were there any hypotheses about operative vaginal delivery and caesarean section before the study?

There was a moderate decrease in OVB after the intervention. How about OVB in similar settings during these periods? Can you compare with national numbers? Could other factors during this period affect the OVB rate?

There was a moderate increase in CS after the intervention. How about CS in similar settings during these periods? Can you compare with national numbers? Could other factors during this period affect the CS rate?

In the 2nd paragraph it is said that an RCT could not be performed in this setting, why is that, please explain what is meant.

Labour progression according to Zhang is mentioned. How come differences in progression in early or late labour were not taken into consideration?
Reference 26, Rossen et al. 2016, used a 4-hour partogram and analysed both Robson 1 and Robson 2 groups (and multiparous women). How can your results be compared to theirs? Please elaborate this much more in the discussion.

Could the higher rate of OVB in your study, compared to Kjaergaard and Bernitz, be associated with your definition of active labour at 1 cm (resulting in high rates of oxytocin)? Please comment on that in the discussion.

More discussion could be added about the problems with the definition used for active labour.

Conclusion

Conclusion could be changed after revision of discussion.

Table 3

I am not convinced that CS and SVD should form a comparison group together.

The text says that BMI was adjusted for but not the table?

Figure 1

This figure looks a little messy and is difficult to read.

Figure 2

The flow chart looks neat but I am not convinced that it shows the most important message. Maybe it just shows what happens if you use this definition of dystocia.

How many women during these periods were defined as Robson 1 and Robson 2? Could there have been a change in the proportions of these groups and how could that affect the results?

Definition of "dystocia" should be added in the figure.

Are the methods appropriate and well described?

If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?

If not, please specify which controls are required in your comments to the authors.

No
Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.
Unable to assess

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.
I am able to assess the statistics

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