Reviewer’s report

Title: A cluster-randomized field trial to reduce cesarean section rates with a multifaceted intervention in Shanghai, China

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Reviewer: Anna Locatelli

Reviewer's report:

The paper reports the effect of a multifaceted intervention intended to reduce CS in Shanghai with negative results. After this strong effort we hoped to see different results.

Methodological observations:
The type of assistance proposed to the women should be clearly expressed: i.e. the % of doula was different in the two groups but with such a low rate of doula and epidural how many women had a midwife in one-to-one relationship?

The intervention continued during the period of post-intervention group? Did the author could evaluate a possible temporal trend in some categories?

The Robson classification should be reported as in WHO document, with the same use of the 10 classes proposed (i.e. breech are separated from transverse lie..)

The results should be more detailed in the text because only reading the tables we can understand some main results, like the rate of CS in the main categories of Robson

Table 2 and 5: report also statistical comparisons between pre and post intervention

Table 5: row on operative delivery in patients with previous CS are useless, they are episodic cases

Regarding the interpretation of the data the main problem in Shangai seems to be the culture of delivery because the high rate of prelabor cesarean section in nulliparous, pluriparous and previous CS limit the effect of the intervention proposed and probably the effect could not be seen in such a short interval of time. If this culture is deep-rooted in the women and in the doctors only a longer time could demonstrate a beneficial effect of the intervention proposed. We should also emphasize that the interventions were mostly educational and education needs time.

The increase (induction) or absence of reduction (episiotomy) of the medical intervention should be discussed and justified

The authors report that in China rise in CS is related to reasons that could not be corrected in 6 months

Regarding the present study in particular:
Fear of labor pain, concerns about complications such as urinary incontinence and lower quality of sex life after vaginal delivery: we see a low rate of doula and epidural and high rate of episiotomy not
changed during the study, this result should be deeply discussed

The shortage of nurses/midwives and the large volume of deliveries often lead to more convenient and scheduled CS: in this field education should be for politicians!

The constrained doctor-patient relationship and insufficient training in vaginal delivery also exacerbated the situation: this problem regards academia

Higher financial incentives for CS versus vaginal delivery may lead to the preferred choice of CS: idem see previous points

I appreciate the effort of the authors but I think that their results should not dishearten clinicians that in other situation could benefit of this kind of interventions. The conclusion of the study should be different: that a multifaceted educational intervention affirmed but not performed in his essence (i.e. not transformed in a change of assistance) could not change the rate of CS where the high rate is mostly due to prelabor cesarean section

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

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Yes

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