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Letter to the Editor

Letter in response to original research article; oral misoprostol solution in comparison to vaginal misoprostol for induction of labour in a randomized controlled trial

Sir,

We read with great interest the original research article entitled oral misoprostol solution in comparison to vaginal misoprostol for induction of labour in a randomized controlled trial by Abbas et al published in your journal in September 2020 issue.[1] We want to congratulate the authors for this successful research article. It is indeed a great effort to shed light on the choice of preferred route and dose of administration of misorostol as a cervical ripening agent for induction of labour. However, we want to highlight a few points that are worth considering during interpretation of the trial results. All participants recruited in the study were primigravidae at term gestation. While term gestation starts from 37 weeks onwards, but in the inclusion criteria gestation range was mentioned from 36 to 42 weeks.

One point which needs clarification is whether primary health care staff in the labour room were blinded to the route of administration. Since routes of misoprostol were grossly different in the 2 study arms, it appears that there could have been chances of bias between the arms at the level of primary treating health care workers in the labour wards. In the methodology, it was clearly mentioned that participants in group 1 received 20 mcg misoprostol solution 2 hourly until adequate uterine contractions were achieved. In another place it was mentioned 200 micrograms dissolved in 200 ml of drinking water used as 20 ml/hour. We will be obliged if you could kindly clarify as to 20 ml in 1 hour or 2 hours. In the literature, it is mentioned that 3 patients were excluded from the arm with vaginal progesterone after randomization, out of which 2 were withdrawn and 1 developed chorioamnionitis. However, in the consort diagram, figure shows that 2 participants were excluded, out of whom 1 withdrawn and 1 developed chorioamnionitis. It will be great help if this dilemma can be solved as to whether the actual numbers of excluded patients were 2 or 3. It will be better to mention the criteria for diagnosis of chorioamnionitis used in the study (whether clinical only or sub-clinical chorioamnionitis also).

We appreciate the manner in which results were presented in tabular form with the tables so detailed yet simplified. In table 2, fifteen participants in the group which received vaginal misoprostol had to undergo caesarean section. Out of them 1 participant had an indication mentioned as others. It would be great help if authors can kindly mention the indication for caesarean in this 1 patient.

Use of Kaplan Meier analysis plots has made the interpretation of the results so lucid. However, figure 3 plot is not depicting what it was intended to show. Figure 3 is just a copy of the contents of figure 2 plot which is an analysis for time to active phase of labour. Figure 3 should be depicting the analysis for time to delivery in either arm. A mention has been made that there were no statistically significant differences between the incidence of meconium stained liquor, uterine hyperstimulation and fever. We would be highly obliged to know the frequency of participants who had the following outcomes in either arms and also the exact p value of the comparison.

We appreciate this research article which was a great initiative towards a commendable topic. We hope that our contributions will definitely add to the strength of the study and make it completely flawless for future references.

Manisha Meena, Avir Sarkar*

Department of Obstetrics and Gynecology, Post Graduate Institute of Medical Education & Research, Chandigarh, Chandigarh, India

***Correspondence to**
Dr. Avir Sarkar,
E-mail: avirsarkar93@gmail.com

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