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Is manual rotation really increases the spontaneous vaginal delivery rate? A response

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We read with interest the letter from Liu et al. about our publication in AJOG “Manual rotation of occiput posterior or transverse positions: a systematic review and meta-analysis of randomized controlled trials.” We agree with their call for more randomized trials and support their comments about the safety of manual rotation and its positive impact on episiotomy rates.

We would, however, like to clarify as briefly as possible some of the methodological issues they raised. Our initial question regarded the obstetric attitude providing the best effectiveness without impairing maternal and neonatal safety. We made clear in the methods section that an intention-to-treat analysis is a better approach to such clinical questions. We were fully aware this approach could lead to applying the intervention to controls or not using it in the rotation group (as in the studies by Broberg et al., Blanc et al. and Verhaeghe et al.). We do not consider this makes their studies ineligible for meta-analysis.

Liu et al. also point out that in the study by Yang et al., a U-shaped birth stool was systematically used in the rotation group and may thus be responsible for the effect observed on the spontaneous delivery rate. In the absence of any evidence or even any mechanistic hypothesis that postural strategies affect the management of occiput posterior positions (1,2), we see no reason to exclude this study because they used a birth stool,

Our purported data extraction error from the study by Yang et al. is equally baseless. The difference results from our inclusion of the cesarean deliveries in the total of childbirths, as in all the other studies in our meta-analysis. Including all births — both vaginal and cesareans — is methodologically necessary for a useful study.

Liu et al. also express concerns about the "overwhelming" weight of this study in our meta-analysis. Discrepancies in study weightings are inevitable under the basic principles of meta-analysis. The conclusion of Liu et al. regarding Yang data is also incorrect, because, again, when all childbirths are considered, the retrieval of Yang study led to a 1.08 risk ratio (95%CI 0.96-1.21). The power of the results would certainly suffer but this result was similar to the
global analysis. The inclusion of the Yang data, however, reduces heterogeneity within the meta-analysis and suggests a coherent effect. Because the key message would remain unchanged, we chose to keep the message to clinicians clear and straightforward.

References:
