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Does ultrasound determination of fetal occiput position improve labour outcome?

Sir,

We read with great interest the study conducted by Ramphul et al.,[1] recently published in *BJOG*. Despite its relative simplicity,[2] it remains unknown whether the accurate depiction of fetal occiput position before instrumental delivery reduces maternal and fetal complications.

In this large randomised study ($n = 514$), the authors demonstrated that the use of ultrasound prior to vaginal instrumental delivery is more accurate than digital examination, but with no detected significant improvement of labour outcome.[1]

We noted some flaws in the article. Firstly, among patient characteristics the fetal head station is not mentioned. Although it is unlikely that knowing the fetal head position prior to an instrumental delivery at a fetal head station of +3 cm or more would be beneficial, this may not be the case at levels between 0 and +2 cm, where operative vaginal delivery is technically more challenging and is reported to have a higher failure rate.[3] A stratification for fetal head station would have been very informative from our point of view.

Secondly, the study sample size was planned to identify a difference in the accuracy of diagnosis of occiput position, whereas it was not adequately powered to detect a difference in labour outcome. Ultrasound was found to be more accurate than clinical assessment (incorrect diagnosis in only 1.6 versus 20.2% in the digital examination group). Therefore, we find it strange to mention in the conclusions that 'A more integrated clinical skills-based approach is likely to be required to prevent adverse outcomes at instrumental delivery'. We would have expected the authors, in the light of their findings and their study design, to call for further larger multicentre trials adequately powered to detect differences in labour outcome, which their study was not designed to detect. We think that their conclusion is unexplained by their data and study design.

Lastly, the definition of the primary outcome of the study is questionable. The authors evaluated the accuracy of clinical or sonographic diagnosis of fetal position based on the actual position of the fetal head at delivery or on the markings of the instrument on the fetal head. Regarding the former aspect, the authors probably assumed the impossibility of fetal head rotation after their assessment and until labour, which we think is imprecise. In addition, the authors acknowledge that the assessment of the instrument markings on the neonatal head with the aim of confirming the accuracy of fetal position determination was at times debatable.

Based on the aforementioned criticisms we think that other large studies, like the recently launched multicentre randomised trial RISPOSTA, (http://clinicaltrials.gov/show/NCT01991665) investigating further the potential benefits of the use of ultrasound for determining fetal head position before instrumental delivery, in addition to meta-analysis with the present extremely valuable data, are desperately needed. ■

References

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Authors' reply

Sir,

We are grateful for the opportunity to reply to the letter by Drs Ghi and Youssef. Their comments allow us to further clarify the methodology and results of our trial of the ultrasound assessment of fetal head position versus standard care at instrumental delivery.[1] We collected a large volume of data, not all of which was presented in the article. The randomisation process was successful for baseline variables, and this includes fetal station, as can be seen in Table 1 below. We did not stratify by station, as station is highly subjective, and to do so could introduce bias. The majority of deliveries were mid-cavity

Table 1. Fetal station prior to instrument application

	Ultrasound *n* = 257 (%)	Standard care *n* = 257 (%)
Fetal head station above ischial spines	2 (0.8)	7 (2.7)
Fetal head station at 0–1 cm below ischial spines	199 (77.4)	180 (70.0)
Fetal head station at 2 cm below ischial spines	50 (19.5)	66 (25.7)
Fetal head station at 3 cm below ischial spines	3 (1.2)	2 (0.8)

(i.e. station at spines 0 to +1 cm), and this may well differ from practice in Italy.

With regards to our study outcomes, we chose incorrect diagnosis of the fetal head position as the primary outcome and powered the trial accordingly. We provided robust evidence that an ultrasound scan prior to instrumental delivery reduces the incidence of incorrect diagnosis, and furthermore that this does not delay delivery. We did not provide evidence that this approach reduces maternal or neonatal morbidity. We fully acknowledge that the trial was not powered for secondary outcome measures (maternal, neonatal and procedure-related complications), and that very large studies including several thousand women may have sufficient power to determine whether ultrasound assessment of fetal head position reduces morbidity. From the clinical perspective, however, we know that there are many factors that contribute to maternal and neonatal morbidity, and that the correct assessment of fetal head position is only one factor along the causal pathway.[2] We stand by our statement that 'a more integrated clinical skills-based approach is *likely* to be required to prevent adverse outcomes at instrumental delivery'. Rather than calling for the replication of our study, we recommended that attention be focused on stategies that not only enhance the accuracy of fetal assessment prior to instrumental delivery but also enhance the obstetrician's ability to deal with a correctly identified fetal malposition.

The definition of the primary outcome required a great deal of thought. We would emphasise that in both arms, the assessment (clinical examination alone or clinical examination and ultrasound scan) was carried out immediately prior to application of the instrument. Therefore, there was no time between assessment and delivery for the fetal head to rotate spontaneouly, unless this was performed intentionally with the chosen instrument and/or by manual rotation, both of which were documented and reported. The instrument markings were very informative, but it was sometimes challenging to differentiate between suboptimal instrument placement and incorrect diagnosis, particularly at vacuum delivery. We have performed further secondary analyses on the data set that we hope to publish in the near future. We very much look forward to reading the results of the RISPOSTA trial and are delighted that other groups are working in this challenging area of clinical practice. ■

References

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Prolapse surgery with or without stress incontinence for pelvic organ prolapse

Sir,

I read with interest the systematic review and meta-analysis of randomised controlled trials that compared prolapse surgery with or without stress incontinence surgery for pelvic organ prolapse.[1] Different continence and prolapse operations were pooled although they have their distinct mechanisms of action; which in turn may affect their outcome, alone as well as in combination, both in terms of effectiveness and adverse effects. Performing a separate meta-analysis for each possible combination of prolapse and continence surgery would have been more appropriate. The duration of follow up was not standardised, leading to pooling of studies with different follow-up durations. The inclusion of mixed incontinence and previous trial of conservative measures are aspects that should be looked at more specifically.

The support provided by mesh repair is different from that provided by fascial repair. The same applies to sacrocolpopexy, when the mesh used can be extended in front and/or behind the vagina to deal with anterior and/or posterior vaginal wall prolapse at the same time. Sacrospinous fixation was not mentioned and is known to be followed by a higher incidence of anterior vaginal wall prolapse, as a result of the exaggerated retroversion of the vagina.[2] There was no mention of surgery for uterine prolapse, such as vaginal hysterectomy or uterus-preserving surgery,[3] including