characterise the evolution of the contribution of each group to the overall CS rate. As stated in the recent World Health Organization statement, which recommends the use of TGCS at the local level, ‘this classification can help health care facilities to optimise the use of caesarean section by identifying, analysing and focusing interventions on specific groups of particular relevance for each health care facility’.

In our centre, as also reported in our national study and in most studies using TGCS in developed countries, the focus should be on group 1 (Nulliparous with single cephalic pregnancy, ≥37 weeks of gestation in spontaneous labour), group 2 (Nulliparous with single cephalic pregnancy, ≥37 weeks of gestation who either had labour induced or were delivered by CS before labour) and group 5 (All multiparous with at least one previous uterine scar, with single cephalic pregnancy, ≥37 weeks of gestation). We are convinced that the assessment of the CS rate using TGCS should be continuous and performed every year, to be effective. However, to take into account population changes over time, adjustment on maternal characteristics (especially maternal age and body mass index) as performed in our paper, is needed when possible.

Finally, we agree that audits of CS should be conducted, in addition to the analysis with the TGCS. This can be performed, as described by Nair et al., in their letter, only for deliveries by CS in the Robson subgroups identified or for all CS. In our centre, we review all caesarean deliveries performed over the last 24 hours at a daily meeting with all the medical staff. We also perform a weekly meeting to prospectively check the indications of planned CS to avoid the medically unjustified ones. These two types of clinical audit are complementary. Clinical audit of CS indications seems indeed an effective intervention to decrease rate of CS without adverse effects on maternal and neonatal outcomes as demonstrated by a recent multifaceted trial conducted in Quebec.1

References

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Re: Pelvic organ prolapse and incontinence 15–23 years after first delivery: a cross-sectional study

Sir,

We read with great interest the article by Volløyauga et al.1 on pelvic organ prolapse and incontinence 15–23 years after first delivery. We believe this is an important topic to investigate as the prevalence of pelvic floor dysfunction is high, has probably previously been under-reported and has a huge impact on the quality of women’s lives.2 The size of the study population and triangulation of data collection is to be commended. It also highlights pertinently that there is no significant difference in the pelvic floor dysfunction (PFD) outcomes between forceps and vacuum delivery.

However, we would like to draw your attention to two areas. First, the study excluded small babies (<2000 g), still-births and breech deliveries at the index pregnancy from the analysis. We believe there to be value in including these groups for comparison. For example, it would be interesting to compare the PFD outcomes between small babies delivered vaginally with those delivered via caesarean section. Likewise, still-births should be included, as many indications for operative vaginal delivery will be similar. Furthermore, if the numbers of vaginal breech deliveries were sufficient, the PFD outcomes could be compared where the delivery is carried out forceps or using the Mauriceau–Smellie–Veit manoeuvre.

The authors do not state why they excluded these groups.

Secondly, an opportunity was missed in analysing urinary incontinence, to distinguish between ‘urinary incontinence at urgency’ and ‘urinary incontinence at coughing, sneezing, laughing’ or mixed incontinence in a further sub-analysis. It is well known that operative vaginal deliveries can result in pelvic floor muscle and sphincter damage leading to stress incontinence3 yet there is very limited literature separating stress only, urge only, and mixed types of urinary incontinence when comparing vaginal and caesarean section. Analysis of the type of incontinence in terms of the woman’s mode of delivery, may have offered valuable insight into the origin and aetiology of the different types of urinary incontinence. The large sample size of this study may have allowed sufficient power to draw statistical conclusions. In the context of an increasing incidence of caesarean section, the results could be instrumental in
guiding future practice and advice given to women.

In conclusion, we believe that this is a well conducted and relevant study, but there may be some missed opportunities with regard to further information that could be gleaned from the available data.

References


Authors’ reply

Sir,
We thank Georgiou et al. for their interest in our recent paper published in BJOG.1 We acknowledge their comments on possible missed opportunities regarding further information that could be obtained from the available data. Our considerations are presented in the following paragraphs.

The reason why we excluded women with stillbirths and small babies (<2000 g) were mainly for ethical considerations. Small babies carry a higher risk of perinatal deaths or neurological problems, and we did not want to bother women with questions related to those births. Another reason for excluding small babies was that delivering a small baby probably carries a lower risk of pelvic floor injury and thus pelvic floor disorders than normal-sized or larger babies. Less than 3% of the babies delivered at Trondheim University Hospital in the actual time period weighed <2000 g.

Comparison of cephalic and breech vaginal delivery was not the aim of this paper, and the number of vaginal breech deliveries would have been too small to analyse for a difference between breech deliveries assisted by forceps and not.

A subgroup analysis on stress urinary incontinence (SUI) and urge urinary incontinence (UUI) was not a pre-specified analysis, and thus not included. The results of sub-group analyses of the prevalence of SUI and UUI are given here: For SUI we found: caesarean delivery (CD): 77/256 = 30.1%, normal vaginal delivery (NVD): 280/688 = 40.7%, operative vaginal delivery (OVD): 262/ 688 = 38.1%, forceps delivery (FD): 122/333 = 36.6%, vacuum delivery (VD): 123/297 = 41.4%. We found similar differences between groups as for the composite variable (UUI and/or SUI) and or previous surgery. For UUI we found: CD: 68/256 = 26.6%, NVD: 176/678 = 26.0%, OVD: 187/682 = 27.4%, FD: 87/332 = 26.2%, VD: 87/293 = 29.7%. There were no statistically significant differences between the groups for urge urinary incontinence, which implies that CD does not protect against UUI, as was also found in a review from 2007.2

In conclusion we agree with your comments. In particular, analysis of pelvic floor disorders after different modes of breech delivery and comparison to cephalic delivery is lacking and deserves future research.

References