argue for a patient-centered and shared decision-making approach to options counseling. Yet, statements such as “Because...the chance of morbidity is highly related to VBAC probability, this estimation also can be informative regarding other important health outcomes” are problematic. This implies that the calculator scores are clinically useful and are statistically-valid estimators of morbidity. This is entirely different from the estimation of likely success. The literature is replete with references of making LAC safer by limiting candidacy based on the calculator scores. We have also documented this in a pilot survey of 1400 practicing midwives in the United States (though we did not quantify the extent of the practice). Although the safety profile for people with high scores is particularly reassuring, the risks to those with lower scores are ambiguous at best. To use such data to “inform other important outcomes” is not ethically or clinically justified for persons with low predicted success. We would ask the authors to clarify this for readers.

Studies linking the calculator scores to morbidity are based on small numbers and composites conflating death, hypoxic encephalopathy, and transfer to neonatal intensive care. We do not minimize the significance of neonatal intensive care unit (NICU) transfer, but nationwide, the most common reason for NICU admission is transient tachypnea of the newborn. Absolute risks and long-term outcome differences between elective repeat cesarean delivery and LAC are uncertain and likely very small. Much larger numbers of low-scoring candidates should be studied before these scores are used in advising or managing patients regarding the morbidity risk, especially Black and Hispanic persons who scored systematically lower than Whites on the original calculator. The sensitivity and specificity, positive predictive power for harms, and the numbers needed to treat should be thoroughly evaluated across the range of possible scores. If racial disparities continue to be predicted (which is likely), this should be critically investigated with an eye on whether individual or systems of care limitations are reflected.

Enhancing information and choice after prior cesarean delivery

We thank Dr. Thornton and colleagues for their note regarding our recent article¹ and appreciate their recognition of our interest in making labor after cesarean delivery more accessible and equitable. We know that their goals are the same. The question then, is how to achieve that end. It also is well-established that some individuals who labor with prior cesarean delivery will have a repeat cesarean delivery. It also is well-established that this chance is strongly related to the chance of morbidity and that knowledge about this chance is important for some individuals to make an informed decision about the approach to delivery that is most concordant with their own preferences.²⁻³ With these facts in mind, it seems reasonable that a tool, if it were to be reliable and have good calibration, that provides information as to the chance of having a vaginal birth after cesarean delivery, has potential value to individuals. As with any potential intervention (be it a decision tool, a pharmacologic intervention, or a medical device), it is important that the tool is used appropriately. We have never advocated that a “cutoff” be used to determine the eligibility for planned labor after cesarean delivery; quite
Search for a predictive relationship between ultrasound thickness of the lower uterine segment and rupture of the uterus in women with a prior cesarean delivery does not make biological sense

TO THE EDITORS: We read with great interest the recent article titled “Evaluation of the usefulness of ultrasound measurement of the lower uterine segment before delivery of women with a prior cesarean: a randomized trial.” The authors conclude that “ultrasound measurements of the lower uterine segment (LUS) thickness did not result in a statistically significant lower frequency of maternal and perinatal adverse outcomes than standard management.” Although this conclusion is true, other studies have concluded that “LUS thickness measured by ultrasound during the third trimester of pregnancy is inversely correlated with uterine scar rupture/dehiscence at delivery”; a conclusion arrived at without demonstrating the practical utility of the method, which has too many false-positives, and above all, false-negative results.

However, we completely disagree with the final advice of this study. In fact, the authors recommended that “because this study was underpowered, further research should be encouraged.” We are aware that knowing about the risk of uterine rupture causes anxiety to the physicians about the potential adverse outcomes that could be a challenge to manage. Nevertheless, we believe that further studies on the subject are totally useless, as after a cesarean delivery, the muscle tissue of the LUS is partially replaced by fibrous tissue and not only by muscle tissue. It is known that the strength of muscle tissue is proportional to its thickness; this is not necessarily true of fibrous tissue, which can break despite its thickness. For this reason, we believe that studies relating the thickness of the LUS with any kind of rupture of the uterus have no biological, and consequently, no clinical sense.

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