conversely, the point of this tool is to amplify the opportunity for a person to gain information that can be used in person-centered decision making. Although Thornton et al express concerns about a decision tool such as this one, they do not suggest a better alternative. Providing no information certainly is not person-centered; if I am an advocate of the alternative - that is, providing information - it is hard to see a priori how its provision according to a provider’s gestalt is more likely to be free of bias or how a population-level estimate is more likely to be informative than an individualized one. This tool, at the very least, can be used to provide information to individuals that is more accurate and helpful than no information or a population average. Moreover, it can be tested to determine whether it enhances health outcomes. It is worth noting that given the structural underpinnings and biases that have been responsible for the disparities observed before any tool’s development, there is no reason to believe that not using a tool is more likely to eliminate bias in counseling or care. This fact only reinforces the urgency of achieving improved and equitable outcomes by adhering to the best practices for all people at all times.

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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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Reply: Search for a predictive relationship between ultrasound thickness of the lower uterine segment and rupture of the uterus in women with a prior cesarean does not make biological sense

We would like to thank Drs Ragusa and Svelato for their comments regarding our 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.”

We are happy to have the opportunity to stress the need for another randomized controlled trial (RCT). Drs Ragusa and Svelato state that further studies on the subject are “totally useless, because the strength of a muscle tissue is proportional to its thickness, but this is not necessarily true of a fibrous tissue, which can break despite its thickness.” Their comment, however, is simply an assertion made without evidence. More precisely, they cite a study from 1924 as evidence of the presence of fibrous tissue but seem to think that “this (that the strength of a muscle tissue is proportional to its thickness) is not necessarily true of a fibrous tissue” is affirmative evidence that further studies are useless. We respectfully disagree.

A high level of evidence is difficult to obtain in clinical research. According to the US Preventive Services Task Force, only properly powered and conducted RCTs ensure the highest level of evidence. Unfortunately, our RCT was underpowered, which did not allow for a formal conclusion. Identifying the women who have had a previous cesarean delivery and who are at a real risk for uterine rupture remains, as Drs Ragusa and Svelato agree, an important aim in obstetrical care, and a definitive conclusion on the usefulness of ultrasound or the lack thereof, for predicting this risk would be helpful for physicians and their patients. Certainly, the concern of all the authors (and their reviewers and editors) who have considered this topic cannot be brushed aside as being devoid of “biological, and consequently, clinical sense.”

Finally, even if our trial was positive, another trial would be necessary to confirm our results. As the US Federal Food, Drug, and Cosmetic Act, which provides the legal standard in the United States for establishing the efficacy necessary for drug approval by the US Food and Drug Administration, wisely states, “Substantial evidence” is based on positive findings from 2 or more adequate and well-controlled trials.

We definitely need more RCTs in clinical research.

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TO THE EDITORS: We read the article titled “Outcomes associated with antibiotic administration for isolated maternal fever in labor” by Bank et al with great interest. They have investigated the role of antibiotic therapy on maternal and fetal outcomes in pregnancies associated with fever but “without evidence of infection.” According to their results, postpartum