

advanced currently available features of ultrasound (eg, color Doppler, 3-dimensional imaging) may be the reason for this described lack of diagnostic benefit, which nonetheless has been documented elsewhere.³ We respectfully challenge the conclusion made in this manuscript, since it does not correspond with our own experience. ■

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REPLY

We thank Drs Levine and Fernandez for their interest in our recent article regarding accuracy of ultrasound for the diagnosis of placenta accreta. We agree that ultrasound is reasonably useful for the diagnosis of accreta and that it may be improved with newer technology. However, our purpose was to illustrate that the modality is imperfect and that there might be room for improvement. Additionally, we discussed that the pretest probability for accreta is strongly driven by clinical history, and that posttest probabilities after ultrasound examination may not significantly alter clinical decision making in certain high-risk patients (eg, those with placenta previa and >2 prior cesarean deliveries). ■

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Home birth study fails to identify credentials of midwives conducting home birth

TO THE EDITORS: Design errors in the article by Grünebaum et al,¹ “Early and total neonatal mortality in relation to birth setting in the United States” raise serious concerns about the accuracy and interpretability of conclusions.

When midwives transfer care to physicians during labor, usually because of obstetrical complications, birth outcomes are attributed to physicians on birth certificates. This may happen more often among midwives practicing in hospital, therefore spuriously reducing rates of neonatal mortality in the hospital midwife group, the reference group for this study. This might explain the two-fold increase in mortality in the physician group and account for the exceptionally low rate of mortality rates among the hospital midwife group (0.3/1000).

Evaluation of birthplace alone without consideration of midwives’ credentials confounds interpretation of findings. There is considerable variation in requirements for licensure of midwives between states. In some, midwives without

formal training can attend homebirths, as acknowledged by Grünebaum et al.² It is highly probable that the association between place of birth and neonatal mortality is confounded by differences in midwives’ training and practice. In contrast, sentinel studies of home birth have precise definitions of caregiver group.³

Another threat to the validity of conclusions from this study is the lack of adjustment for other confounders by, for example, maternal age, number of prenatal care visits and medical/obstetrical conditions but also access to primary obstetrical care and midwifery access to hospital privileges. Other leading epidemiological studies have addressed these issues previously.⁴ Table 1 of Grünebaum et al’s article¹ identifies important differences among exposure groups according to maternal age, race/ethnicity, birthweight and gestational age and the authors did not account for these differences.

Adjustment for confounders would still not make possible the attribution of outcomes to midwifery groups defined