Teaching Infrequently Used Skills: Vaginal Breech Delivery

To the Editor:

I enjoyed reading the editorial titled “Teaching Infrequently Used Skills: Vaginal Breech Delivery.”1 Despite the present emphasis on delivering breech babies by cesarean, vaginal delivery skills should be taught effectively to all residents who plan to deliver babies.

Perhaps you remember that I taught myself to perform external cephalic version for breech babies during the last trimester. Eventually I became skilled enough with versions so that about 80% of breech babies had been turned to cephalic presentation before the time of labor and delivery.2 Of course, this left a much smaller-than-average number of breech babies when labor started. Of these, I delivered 20% by cesarean for a number of specific reasons. This left a very small number of breech babies at term in labor, which could be used to give residents experience and training. Yet, in our small residency training program (1955 through 1981) every resident who completed his training was capable of performing an indicated vaginal breech delivery.

We accomplished this by simple methods. There were regular training sessions with the manikin. Then, in the unusual instance when a mother entered the hospital with her fetus in breech presentation, the resident and I would study the proper chapter in the obstetric textbook for several minutes. Then we would go to the manikin for some more practice. Finally, when we were scrubbing our hands before the delivery, I would inform the resident that he should perform the delivery carefully, as practiced. I also warned him, or her, that if a particular part of the delivery was not progressing as it should, I would nudge the resident discreetly aside to complete the procedure myself, during which time he could learn by observation. The next time he could apply this knowledge. By using these methods, it took only a few patients for the resident to learn the techniques of indicated safe vaginal breech delivery.

Brooks Ranney, MD
Box 590, Yankton, SD 57078

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Conversion From Vertex to Breech After Extra-amniotic Saline Installation

To the editor:

Extra-amniotic saline installation is a well-established method of inducing cervical ripening and dilation.1 The popularity of the procedure stems from its ease of use, low costs, effectiveness, and lack of major complications. Minor complications are uncommon as well and include bleeding, pain, accidental amniotomy, and, rarely, infections.1–3

We report our experience with extra-amniotic saline installation in a group of 563 women admitted for induction of labor. According to our policy, extra-amniotic saline installation is the preferred means of cervical ripening whenever the cervix is less than 2 cm dilated and less than 70% effaced. Fetal presentation and placental location are documented by ultrasonography before the procedure. Fetal heart rate and uterine activity are monitored with an external device throughout the procedure and for at least 2 hours afterward.

Route of delivery was vaginal in 425 (71%) women, instrumental in 6%, and cesarean in 24%. The main complications encountered were unbearable pain necessitating withdrawal of the catheter in 9 cases (1.6%), 3 cases of vaginal bleeding (0.5%), and altered presenta-
tion from vertex to breech in 7 cases (1.2%). The latter complication is as yet unpublished in the English literature. Although all ultrasound examinations were done within 30 minutes from catheter insertion, the exact timing of fetal rotation is unknown due to different rotation-to-detection intervals. Overall, 89 (16%) women in our group had an unengaged or free-floating vertex, but interestingly, all 7 women with altered presentation were among that group (statistically significant).

The mechanism for fetal rotation after balloon inflation has yet to be established, but we propose that the inflated balloon drives the presenting part upwards concomitantly with a sudden contraction caused by Ferguson reflex acting synergistically to cause a flip movement of the free-floating fetus.

We describe a heretofore unpublished complication of extra-amniotic saline installation insertion, which is probably unique to unengaged vertex presentations, and suggest reconsideration of the ripening method to avoid rotation to breech in cases of a free-floating fetal head.

Sharon Maslovitz, MD
Obstetrics and Gynecology Department, Lis Maternity Hospital, Sourasky Medical Center, Tel-Aviv, Weizman 6 st, Tel Aviv, Israel

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To the Editor:
Whitehead, Berg, and Chang make an important contribution in detailing pregnancy-related mortality due to cardiomyopathy. Their documentation of a racial disparity showing black women in the United States to be 6.4 times more likely to die from pregnancy-related cardiomyopathy than white women is both alarming and in urgent need of explanation.

We previously documented a high-incidence area of peripartum cardiomyopathy in Haiti (Fett JD, Dowell DL, Carraway RD, Sundstrom JB, Ansari AA. 100 cases of peripartum cardiomyopathy...and counting: what’s going on? Int J Cardiol 2004, in press). In comparison with the United States, with 1991–1997 pregnancy-related mortality due to cardiomyopathy of 0.88 per 100,000 live births, the ratio for the Hospital Albert Schweitzer District of Haiti is 32.3 deaths per 100,000 live births for a 4-year period from January 1, 2000 to December 31, 2003 (Warraich RS, Fett JD, Damasceno A, et al. Impact of pregnancy-related heart failure on humoral immunity: clinical relevance of G3-subclass immunoglobulins in peripartum cardiomyopathy, 2003, unpublished data).

This figure is based on 10 deaths due to peripartum cardiomyopathy over a 4-year time span, with 30,960 live births in a district where the incidence of peripartum cardiomyopathy is 1 case per 350 live births. Of these 10 deaths, 50% died within 42 days of delivery and 50% died between 43 days and 1 year postpartum. Whitehead et al's report identifies time of death from peripartum cardiomyopathy to be 2% while yet undelivered, 48% within 42 days of delivery, and 50% between 43 days and 1 year postpartum.

This would place our Haitian population cause-specific mortality ratio from pregnancy-related cardiomyopathy at 40 times the U.S. ratio for all races and 7.5 times the U.S. black women ratio. This discrepancy also merits continuing investigation but may accurately reflect the disparity in medical resources between the impoverished setting of peasant women in the Artibonite Valley of Haiti and the United States. Does the U.S. 6.4-fold disparity between black women and white women also reflect a disparity in medical resources?

James D. Fett, MD
Hôpital Albert Schweitzer, Department of Adult Medicine, Deschapelles, Haiti, c/o 611 Summer Avenue, Aberdeen, WA 98520; e-mail: jdfitts@tecline.com

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In Reply:

We thank Dr. Fett for his interest in our paper and his description of the prevalence of peripartum cardiomyopathy in Haiti. His description of the distribution of the interval between delivery and these deaths is similar to that which we documented in the United States. Cardiomyopathy is the only major cause of pregnancy-related death in which about half the deaths occur more than 42 days postpartum.

Dr. Fett’s numbers for the Hospital Albert Schweitzer District are facility-based estimates, which tend to overestimate the number of cases. Nonetheless, the cause-specific mortality ratio from peripartum cardiomyopathy in Haiti, about 40 times the U.S. ratio, is the same as the difference in maternal mortality from all causes. The 2000 World Health Organization estimates for maternal mortality show a ratio of 680 for Haiti versus 17 for the United States, a 40-fold difference.1

The black-white disparity in the United States is not specific to cardiomyopathy but holds true for all causes of maternal mortality.2 We agree this is an important disparity that we need to understand and eliminate, and we continue to investigate the causes of this gap both for cardiomyopathy and for all causes of maternal death.

Sara J. Whitehead, MD, MPH
Cynthia J. Berg, MD, MPH
Jeani Chang, MPH
CDC/HIV, P. O. Box 68, APO AP 96546; e-mail: swhitehead@cdc.gov

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ERRATA

In the response to letters by R.L. Brown and J.A. Decenzo (“Iatrogenic Endometriosis Caused by Uterine Morcellation During a Supracervical Hysterectomy” Obstet Gynecol 2004;103:583–4), the names of the authors were omitted. The authors of the response are Vicken Sepilian, MD, and Carl Della Badia, DO, Department of Obstetrics and Gynecology, Drexel University College of Medicine, Philadelphia, Pennsylvania.

In “Prevalence of Depression During Pregnancy: Systematic Review” by H.A. Bennett, A. Einarson, A. Taddio, G. Koren, and T.R. Einarson (Obstet Gynecol 2004;103:698–709), the column headings in Table 3 were printed incorrectly. Starting with “Instrument,” the headings should be shifted 1 column to the left, ending with the addition of “95% confidence interval” as the last heading.

In “Dysfunctional Labor and Myometrial Lactic Acidosis” by S. Quenby, S.J. Pierce, S. Brigham, and S. Wray (Obstet Gynecol 2004;103:718–23), an author’s name was printed incorrectly. “Sue J. Pierce, MBBS” should have read “S. J. Pierce, MbChB.” Also, the manuscript should have noted that Dr. Pierce’s work is funded by Action Research.

In “Microwave Endometrial Ablation: Three-Year Outcomes of a Multicenter Trial” by T.L. Anderson and J.M. Cooper (Obstet Gynecol 2004;103:99S), “excessive urinary bleeding” (in the abstract’s conclusion) should have read “excessive uterine bleeding.”