Singleton Vaginal Breech Delivery at Term: Still a Safe Option

To the Editor:
We read with interest the recent original research article by Alarab et al,1 which examines the obstetric and perinatal outcome of singleton vaginal breech deliveries at term, as well as the accompanying editorial comments by Queenan.2 Surprisingly, neither article mentioned the importance of epidural labor analgesia for safe and successful vaginal breech delivery in these parturients.

It is a routine practice at the University of California, San Diego, to encourage parturients wishing to undergo (and meeting criteria for) singleton vaginal breech delivery at term to receive early epidural analgesia (with low concentrations of local anesthetics and opioids to avoid any degree of motor block), which can be used later in the second stage of labor (if indicated) to provide adequate degree of perineal analgesia, relaxation, and/or anesthesia (accomplished with administration of higher concentrations of local anesthetics and opioids) to facilitate safe and successful vaginal breech delivery.

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REFERENCES
doi:10.1097/01.AOG.0000131627.26868.c8

In Reply:
We thank Drs. Kuczkowski and Simpson for expressing interest in our paper. The authors mention the importance of epidural analgesia for safe and successful vaginal breech delivery. We recognize that epidural anesthesia has gained increasing popularity throughout the world due to its safety and effectiveness in relieving pain and has replaced other methods of labor pain relief. However, controversies exist regarding its effect on the course of labor. Some studies have shown a prolonged mean duration of the second stage of labor,1–3 and increased rates of instrumental delivery and cesarean delivery with the use of epidural analgesia.4

In the National Maternity Hospital, Dublin, Ireland, where our study5 was conducted, the epidural service is available and offered to all patients who are in labor. In our study, 53% of patients who labored and 56% of those who delivered vaginally used epidural analgesia. We are not undermining the role of epidural analgesia. In fact, we adopt the same routine that the author mentioned and advise it especially in a high-risk patient for whom we anticipate an emergency cesarean delivery.

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doi:10.1097/01.AOG.0000131742.65394.9f
Singleton Vaginal Breech Delivery at Term: Still a Safe Option

To the Editor:
The retrospective review of singleton vaginal breech delivery at term by Alarab et al\(^1\) strikes a responsive chord. Following the report by Hannah et al\(^2\) and the American College of Obstetricians and Gynecologists (ACOG) Committee Opinion No. 265,\(^3\) many obstetricians in our region have felt obligated to perform a planned cesarean delivery of any breech-presenting fetus at term as soon as the diagnosis has been made.

The authors discuss their recommendations for a selective approach to a trial of labor. This concept evolved during the second half of the 20th century.\(^4\) Prospective randomized studies supported this conclusion in the early 1980s.\(^5,6\)

What is clear is that the connection between studies and actual practice is a distorted one. Virtually all persons involved, ie, patients and clinicians, identify the central problem to be the concept of what constitutes “safe” in the context of the uncertainty always present in the practice of medicine.

The authors state, “We present our findings not as an argument that breech presentation during labor and delivery is without risk but to suggest that with active involvement of experienced obstetricians... vaginal breech delivery can achieve comparable safety for the infant with spontaneous cephalic birth.”\(^1\) Given goals of limited resources and safe delivery for both mother and infant, a selective trial of labor has always been a viable approach.

Although selection is not 100% without risk, should it be abandoned in obstetrical practice? In my opinion vaginal breech delivery at term can still play a role in practice, albeit one limited more by unrealistic goals than by evidence. Let’s remember that the demands made upon us by patients, ie, the “perfect infant” originated with us. It’s time we set goals that can be realistically achieved on a daily basis.

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REFERENCES

Editor’s Note:
The authors declined to respond.
doi:10.1097/01.AOG.0000131628.62183.34

Teaching Infrequently Used Skills: Vaginal Breech Delivery

To the Editor:
We read with great interest Dr. Queenan’s editorial\(^1\) titled “Teaching Infrequently Used Skills: Vaginal Breech Delivery” in the March 2004 issue, and we agree that in our litigious society the route that appears to minimize professional liability will be taken and the number of these deliveries will decrease. As vaginal breech deliveries become less common, the level of expertise of physicians will also suffer. What will not change and may increase is the risk to the fetus when a breech vaginal delivery is unavoidable and the provider is inadequately trained. In addition to breech vaginal deliveries, forceps deliveries and other obstetric emergencies also present a dilemma with regard to adequate training and maintaining competency.

Simulation training allows for teaching and ensuring skill in today’s litigious climate in that there is no risk to the patient or the fetus, and models are available to simulate and practice obstetric procedures and emergencies. We have utilized an obstetric-training manikin at our institutions and designed a curriculum that both trains and evaluates the performance of our resident physicians on several obstetric procedures, including both forceps and vacuum deliveries, and emergencies, such as shoulder dystocia. We have documented significant improvement in both their performance and technique with our training and testing. Indeed training centers may implement these programs with minimal capital investment.\(^2\)
While we believe that simulation training is an excellent teaching method for common obstetric emergencies, it is important to evaluate the training and validate the approach. There is room for improvement in the development of realistic manikins and simulation scenarios. If simulation training is to be incorporated into more advanced training and higher-level competency evaluation, a more substantial investment in technology and dedicated centers will be required to train, validate, and ensure competency. We hope that the American College of Obstetricians and Gynecologists, maternal–fetal medicine subspecialists, and interested educators will take the lead and work toward developing centers of excellence in this rapidly growing field. We truly believe that the improvement in obstetric training and maintenance of skills that simulation laboratories can provide will improve care to our patients.

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REFERENCES

Editor’s Note:
The author declined to respond.
doi:10.1097/01.AOG.0000133282.34518.dc

Ethical Dimensions of Elective Primary Cesarean Delivery

To the Editor:
Minkoff et al1 concluded that beneficence-, justice-, and autonomy-based considerations do not support routinely recommending or offering elective cesarean delivery, since there is no evidence of clear benefit, and we do agree with their not recommending it. But it seems to us that offering it is still a medical duty regarding patient’s autonomy.

Although underlining the prominent role of autonomy, the authors refuse to base clinical judgment solely on the patient’s positive rights to self-determination, because it would reduce physicians to “technicians.” Patients are not experts in medical fields, so they cannot argue on clinical judgment. But they are experts in their own fields.2 Actually, patients want to have expert advice concerning clinical practice and then make their own choices and express their own preferences. The principle of autonomy is put into clinical practice through the informed consent process which encompasses 3 elements: 1) disclosure of information, 2) understanding of information, and 3) voluntary decision to authorize or refuse a clinical management.3

Consequently, with respect to autonomy, the physician must accurately present the medical facts to the patient and make management recommendations in accordance with good medical practice.4 The debate about performing elective cesarean delivery on request justifies per se that the option should be disclosed and then offered to patients. Minkoff et al think that only the reasonable alternatives should be offered to patients, but what is reasonable? Who decides what is reasonable? What about the reasonable interests of the fetus in vaginal versus cesarean delivery?

Finally, the whole of it sounds very much like paternalism. We definitely do not support the option of elective cesarean, but strongly suggest that not offering it disregards the principle of autonomy.

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doi:10.1097/01.AOG.0000130986.93923.d3
In Reply:

We greatly appreciate Dr. Tassy and colleagues’ interest in our article and are grateful for the opportunity to clarify our views on the important issue that they raise. Patient autonomy should be a guiding principle in the consent process, but its place should be correctly understood. The physician should identify medically reasonable alternatives for managing the patient’s condition or problem and present these alternatives to the patient. Medically reasonable means that the best available evidence supports a clinical judgment that clinical management is reliably expected to benefit the patient population clinically. Kennedy actually endorses this view of the physician as an expert in “clinical matters.” As Kennedy correctly puts it, the patient brings her expertise about her “experience, feelings, fears, hopes, and desires” to the informed consent process. Kennedy does not endorse the view that the patient brings clinical expertise, although Kennedy, again correctly, notes that patients are increasingly sophisticated about medical care. Sophistication, however, does not amount to expertise. In the informed consent process, the physician, as a matter of professional integrity (which is designed to protect, not paternalistically exploit, patients), should remain in control of what is regarded as medically reasonable and the patient should remain in control of the final decision about which clinical management is implemented. The best available evidence does not support the clinical judgment that elective primary cesarean will, on balance, benefit pregnant women and their children. That elective primary cesarean is a matter of current debate does not by itself establish that this form of clinical management should reliably be judged to be clinically beneficial, as Tassy et al suggest. Routinely offering elective primary cesarean delivery is therefore not required by the ethics of informed consent.

Medical paternalism involves interference with the patient’s autonomy for the clinical benefit of the patient. The exercise of expert clinical judgment does not interfere with, but enhances, the patient’s decision-making role in the informed consent process. Our position therefore does not constitute, much less “sound like,” paternalism.

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REFERENCES

Fatal Hydrops Fetalis Caused by Anti-D in a Mother With Partial D

To the Editor:

I read with interest the recent case report by Cannon et al, who described a fatal hemolytic disease of the fetus in a woman with partial Rh(D) category VI antigen. The authors conclude that, although severe hemolytic disease of the fetus and of the newborn (HDN) in patients with partial Rh(D) is rare, their case illustrates the need for a change in management of women with weak Rh(D).

However, partial Rh(D) is not a weak Rh(D) antigen. The D antigen on D-positive erythrocytes varies quantitatively and qualitatively. The distinction between the partial D antigen and weak D is that the partial D antigen differs from normal qualitatively, whereas weak D antigen differs from normal quantitatively. Individuals who lack part of the D antigen may, when exposed to a complete Rh(D) antigen, produce antibody to the missing epitopes. Tippett and Sanger divided these rare partial D antigens into 7 numbered D categories: I through VII. The partial D category I is obsolete.

There are numerous reports of HDN in the offspring in women with partial D antigen. Most had mild-to-moderate forms of HDN. The fatal cases had D category VI or IVa antigens. Because nonsensitized women who have partial Rh(D) antigen can develop Rh immunization, they should be regarded as Rh(D) negative for the purposes of prenatal management, and they are candidates for Rh immune globulin prophylaxis. Still, it seems that it is difficult to identify such women before sensitization because most women will be found to have an Rh(D)-positive phenotype.

On the contrary, because the risk of weak Rh(D) women for developing Rh immunization is so extremely low, it is generally accepted that weak Rh(D) women do not require Rh immune globulin prophylaxis. The consequence of unessential administration of Rh immune globulin prophylaxis in weak Rh(D) women might be transmission of infectious diseases.

Fatal Hydrops Fetalis Caused by Anti-D in a Mother With Partial D

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On the contrary, because the risk of weak Rh(D) women for developing Rh immunization is so extremely low, it is generally accepted that weak Rh(D) women do not require Rh immune globulin prophylaxis. The consequence of unessential administration of Rh immune globulin prophylaxis in weak Rh(D) women might be transmission of infectious diseases.
A distinctive circumstance is with weak partial Rh(D) antigen, which is an Rh(D) antigen that varies both quantitatively and qualitatively. Women with weak partial Rh(D) antigen should be regarded as Rh(D) negative for the purposes of prenatal management because an occurrence of HDN in a mother of the weak Rh(D) phenotype was described.2

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REFERENCES

doi:10.1097/01.AOG.0000130983.59168.be

In Reply:
Dr. Lurie correctly points out that a weak D antigen result may be due to a quantitative—not qualitative—difference of the antigen compared with a normal D antigen. As we stated,1 the fact that patients with weak D typing generally do not make anti-D suggests that the major epitopes of the D antigen are present, but with fewer antigen sites per red cell. In contrast, the concept of partial D (by definition) is related to the potential of patients with this type of D variant to make anti-D. As we pointed out, such patients may have normal or weak D typing.

Weak D typing results depend on the antigen and on reagent formulation. Patients in the past called weak D can show normal D typing results with current reagents. In fact, testing recommendations have changed; if the initial red cell typing is negative, testing for weak D is considered optional, as stated by Judd,2 referenced by both Dr. Lurie and us. The fact that the weak D test itself may be considered optional implies that we may consider patients with weak D tests as Rh negative (and, therefore, as Rh immune globulin candidates).

We agree that patients with a partial D phenotype should be treated as Rh negative for prenatal management. As Dr. Lurie states, it may be difficult to identify women with partial Rh(D) antigen prior to sensitization. We would, however, consider weak D results an indication of a potential partial D and choose not to perform a test for weak D (with Coombs reagent) if the initial direct test is negative. A test for weak D can be misleading, because a positive test in the Coombs phase does not prove a quantitative type weak D. However, it is apparent from the survey we cited3 that some laboratories come to this conclusion. An equivocal initial direct test for D also would lead us to evaluate for possible partial D antigen.

In Reply:
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doi:10.1097/01.AOG.0000131623.15664.c1

Fatal Hydrops Fetalis Caused by Anti-D in a Mother With Partial D

To the Editor:
We read with interest the article by Cannon et al.,1 which reviewed the topic of erythroblastosis fetalis from anti-D produced by patients who are D(+) positive or have partial D antigen. The confusing nomenclature and theoretical question of treatment prophylaxis have invited debate for decades. It was puzzling to us that the largest series2 in the obstetrical literature was not referenced within the recent case report.

At the 1982 meeting of the American Gynecological and Obstetrical Society, the outcomes of 5 affected patients were presented, and the last half of the discussion section addressed whether Rh immune globulin was indicated for the D(+) positive mother. The consensus of
the authors and formal discussants was that Rh immune globulin was not indicated based upon 1) the rarity of anti-D production by patients with D variants, 2) the paucity of reported perinatal deaths, and 3) the lack of scientific evidence that Rh immune globulin is efficacious for D-positive individuals. The one critical caveat to such a policy was clearly stated and involves Rh-negative women suffering a large fetomaternal hemorrhage. Such an event could cause a false positive D designation resulting in postpartum Rh immune globulin being withheld when, in fact, multiple vials of Rh immune globulin might be indicated.

So, what is new since 1982? Although not including pregnant patients, it is of concern that the transfusion literature has documented a 0.8–2.1% incidence of anti-D formation in patients with weak D phenotype transfused with D-positive blood. Nevertheless, Mayne and co-workers3 uncovered only 8 deliveries (1 in 114,000) involving anti-D production by patients with partial D. Although it is possible that future literature may identify patients with partial D antigen that benefit from Rh immune globulin, we continue to believe that it is not indicated based upon frequency and efficacy issues.

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REFERENCES

In Reply:
We thank Drs. Stedman and White for their comments. As information, the 5 patients reported in their 1983 article were among those included in a subsequent review by Mayne et al,1 which we did reference in our own brief case report.2

In the last 20 years, advancements in our understanding of the Rh system, improvements in reagents, and increased safety of immune globulin preparations have led to reassessment of routine policies. We would agree with others1,3 that patients with known partial D may benefit from Rh immune globulin, and we would not want to withhold Rh immune globulin prophylaxis from such patients. Our case report is a reminder that isoimmunization in a patient with partial D can cause hemolytic disease. Furthermore, the result can be devastating, reminding us of hemolytic disease seen more commonly in Rh-negative mothers in the days before Rh immune globulin was available.

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REFERENCES

doi:10.1097/01.AOG.0000131624.52535.3e

The Association Between Fetal Sex and Preterm Birth in Twin Pregnancies

To the Editor:
Tan et al,1 in a retrospective population-based cohort study, concluded that male sex was associated with an increased risk of preterm birth in twin gestations. We studied the gender of twins in relation to the gestational age at delivery in a single center in the years 1984–2000. After excluding intrauterine fetal death, structural abnormality, twin-to-twin transfusion syndrome, and incomplete data, 679 sets of twins were analyzed. The distribution was 216 (31.8%) female/female, 231 (34.0%) female/male, and 232 (34.2%) male/male twins. The preterm delivery rate at 34 weeks or less was 25.0%, 23.4%, and 24.1%, with a mean gestational age at delivery of 35.9 ± 3.1, 36.4 ± 3.1, and 36.2 ± 3.1 weeks in the 3 groups, respectively (P value not significant). Multiple logistic
Letters to the Editor

doi:10.1097/01.AOG.0000130985.62334.66

In Reply:

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Mark Walker, MD
Karen Fung Kee Fung, MD
Kitaw Demissie, MD, PhD
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doi:10.1097/01.AOG.0000131738.98566.a2

regression analysis was done to study factors that influenced the preterm delivery rate at 34 weeks or less. Variables included were advanced maternal age, multiparity, gender, small for gestational age (SGA), product of assisted reproductive technology (IVF), discordance, and obstetric complications. Factors that influenced the preterm delivery rate were discordance (odds ratio 2.81, 95% confidence interval 1.43–5.49, \( P = .03 \)), IVF (6.89, 3.44–13.79, \( P < .001 \)), SGA (0.13, 0.08–0.21, \( P < .001 \)), and multiparity (0.51, 0.34–0.77, \( P = .001 \)). The gender (any male) had an odds ratio of 0.74, 95% confidence interval 0.489–1.11, and \( P \) value was not significant.

We reanalyzed the data using the cut-off gestational ages used in their paper (Table 1). Excluding the 10 indicated preterm deliveries would be unlikely to affect our results.

The difference between our results and theirs may be due to the smaller number of twins studied. However, constitutional differences between the 2 populations can- not change the results. These findings were inconsistent with the results in our analysis of a large twin registry data (148,234 live-born twin pairs) in the United States (1995–1997),\(^1\) in which we found that the male-male twin pairs had the highest preterm birth rate, the female-female pairs were the intermediate, and the opposite-sex twin pairs had the lowest rate. As the authors have noted, the sample size in their study was small, and all of the results in their analysis were statistically insignificant. Differences in time period of the data, population characteristics, and health care systems between the 2 studies may have also accounted for some of the observed discrepancies.

We have hypothesized that fetal hormones may have played a role in the occurrence of preterm birth. However, the mechanisms of labor and preterm birth are complex and many factors are involved in the process, and the effect of fetal hormone can be offset or masked by other factors in the chain of actions leading to labor and preterm birth. A large study sample is needed to obtain stable results. The message obtained through our meticulous analysis of the large twin registry data from the United States should be widely disseminated to the medical society, because it may have important implications in clinical care for twin pregnancy and in scientific research on the mechanisms of labor and preterm labor.

Table 1. Gestational Age at Delivery and Birth Weight in Relation to Gender

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<thead>
<tr>
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<th>Female/Female I (n = 216)</th>
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<tbody>
<tr>
<td>Preterm delivery &lt; 28 wk [n (%)]</td>
<td>4 (1.9)</td>
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<td>Mean gestational age (wk)</td>
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<td>Mean birth weight (g)</td>
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NS = not significant.

REFERENCE

doi:10.1097/01.AOG.0000130985.62334.66

In Reply:

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Anwar H. Nassar, MD

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REFERENCE

doi:10.1097/01.AOG.0000131738.98566.a2

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NS = not significant.
Impact of Folic Acid Fortification in the United States

To the Editor:

Folic acid fortification has decreased the occurrence of neural tube defects in populations in the United States, Canada, Hungary, and other countries. This may also increase the red cell mass and, thus, lower the mean corpuscular riboflavin concentration. There is concordance of these values in the mother and her fetus that holds in deficiency states. This has been repeatedly studied and verified by statistical analysis (Kendall $\tau$ is 0.67 + 0.07 with 95% confidence). In populations where neural tube defects occur, severe deficiency of other B-complex vitamins is expected. Treatment with folic acid may give rise to other B-complex deficiencies and mask others, such as vitamin B12. What is of ominous significance is that the neural tube closes about 21 days after conception. Because of this and the cellular nutritional link between mother and fetus, “preconception nutritional treatment” has been recommended (Clarke HC. Cellular nutritional link between mother and fetus [letter]. Am J Obstet Gynecol 1984;148:121). But this should include other vitamins of the B-complex.

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REFERENCES

doi:10.1097/01.AOG.0000131626.34581.fc

In Reply:

We agree with Dr. Clarke that the preconception period should be one of optimal nutrition. Several studies have suggested the benefits to mother and fetus of vitamin supplementation. However, because utilization has been suboptimal, mandatory fortification of breads and grains began in the United States in 1998, and we believe the data show it to be a major success.

Before such mandatory fortification of grains with folic acid, there was much consideration of its effects on the nutritional milieu. Although the masking of vitamin B12 deficiency by folic acid fortification has been a theoretical concern, the problem from a public health concern has not, in fact, materialized. It is a greater potential problem in the elderly than in women of reproductive age. As we learn more about fetal programming, we will learn more about the effects of nutrition on gene expression and the development of birth defects. Nevertheless, it seems prudent to recommend that women of reproductive age take a daily multivitamin with 400 $\mu$g of folic acid.

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Effect of Raloxifene on Urinary Incontinence: a Randomized Controlled Trial

To the Editor:

Waetjen et al described an ancillary study of the Multiple Outcomes of Raloxifene (MORE) Trial in which urinary incontinence symptoms at baseline and 3-year follow-up were obtained from 963 women receiving either raloxifene or placebo.

The authors apparently compared the outcome variables between the treatment and placebo groups at 3-year follow-up using independent-samples tests. This may be appropriate for the primary condition under study, osteoporosis, because all women met World Health Organization criteria for osteoporosis at entry. However, it may not be appropriate for urinary incontinence. The samples at baseline and at follow-up are not independent and, moreover, urinary incontinence is not a static or necessarily progressive condition. Recent studies demonstrate incidence and regression as high as 20% per year in older American women. The number of women classified as “incontinent” in this study increased from 376 to 395 women in the raloxifene group and from 188 to 200 in the placebo group (this was not accounted for in the authors’ Table 3). Within these relatively low numbers (19 women in the raloxifene group and 12 in placebo), it seems likely that many more women experienced new onset incontinence (representing true incident cases), and of those women who reported incontinence at baseline, that many more experienced regression. These potential changes are lost when comparing only proportions of women classified as incontinent or continent.

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It would be important to determine the effect of raloxifene on specific rates of incidence and remission, not just by comparing the average number of women with incontinence at the 2 time-points. A more appropriate analysis would have been to perform paired-samples testing. Comparing the groups on 2 different occasions by independent samples testing does not address appropriately, and probably underestimates, the potential effect of the intervention.

Valuable information about the incidence and regression of urinary incontinence in postmenopausal women could be estimated from this study if the data were treated as paired data.

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REFERENCES
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In Reply:
With respect, Drs. Lowder and Weber are mistaken in their assumption that we compared “only proportions of women classified as incontinent or continent,” ignoring the individual incontinence trends of the subjects in the MORE trial. Rather, we computed changes in incontinence severity scores for each subject. Then, to make this long-tailed continuous outcome more tractable for analysis, we categorized the change scores as improved, no change, or worsened, and used proportional odds models to analyze treatment effects on these ordinal categories (Tables 3 and 4 in our study). This method of analysis thus clearly captures individual changes from baseline to year 3. In response to the interest of Drs. Lowder and Weber, the incidence of new incontinence among women who did not report it at baseline was 28% in the placebo group and 27% in the raloxifene group reported resolution of their symptoms at year 3.

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Subcutaneous Tumor Implantation After Laparoscopic Procedures in Women With Malignant Disease

To the Editor:
“Subcutaneous Tumor Implantation After Laparoscopic Procedures in Women with Malignant Disease” by Abu-Rustum et al1 reports the phenomenon of surgical inoculation as a means of tumor spread, a subject of interest for many years.2 Spread by “contact. . . has always excited a legitimate skepticism.”3 My personal experience with laparoscopic intervention is far less than that in the report and supports the authors’ conclusion about the rarity of laparoscopic implantation. But the risk of subcutaneous implantation of ovarian carcinoma in small punctures is higher than for other neoplasms.

A facet of tumor biology may be inferred from the observations common to the authors and others. In a personal experience with over 500 cases of ovarian carcinoma, I have never seen implantation in a conventional abdominal surgical incision. Before the use of cis-platinum, paracentesis to relieve respiratory embarrassment was frequent and implantation at the trocar site was common. I believe that the conventional abdominal surgical incision of 5–15 cm provokes some humoral or vascular reaction which prevents implantation of tumor cells, but this protection may not be induced by needle sticks or small trocar entries into the peritoneal cavity. Investigation of factors preventing tumor implantation in conventional surgical incisions would be worthwhile because those factors might be exploited for direct antitumor therapy.

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Editor’s Note:
The authors declined to respond.
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A Growing Concern: Inability to Diagnose Vulvovaginal Infections Correctly

To the Editor:
We commend Drs. Ledger and Monif1 for highlighting the importance of performing microscopy in the evaluation of vulvovaginal infections. The authors cite our previous work2 demonstrating that office-based testing is infrequently performed in the evaluation of women with vulvovaginal symptoms. However, Drs. Ledger and Monif are incorrect in their assumption that these incomplete evaluations occurred in our Vaginitis Clinic. In fact, this “dismal record” reflected the care delivered to women by their previous providers, before their visit to our center. Every woman evaluated in our Vaginitis Clinic with vulvovaginal complaints undergoes a comprehensive office-based evaluation, including pH measurement, amine testing, and microscopy of vaginal fluid.

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Work-Hour Limitations: Let’s Solve Our Own Problems

To the Editor:
We are senior residents in the United Kingdom. According to the Green Journal’s Deputy Editor, we are earning an exorbitant £80,000 (approximately $144,000) per annum in return for an education.1 We’d like to advise your readers about the training system on this side of the Atlantic Ocean.

We have been working as doctors in the United Kingdom for over 10 years. Our education and training is not yet over. It may take 3 further years’ training to get us to a consultant’s post. During these years, we’ve done 80 hours plus work each week in the hospital. We’d love to be trained properly in a 5 years’ training program akin to the North American model. The reality is that most of our time at work is all service and very little training. It is the residents (many time-expired) that hold our health service together while it bursts at the seams. In our spare time, we have to pass postgraduate examinations, research, audit, and publish if we are to move up the career ladder.

Eventually, however, most of us reach the dizzy heights of consultant and have our own patients and operating lists. What may surprise your readers is that we can also look forward to a starting salary of £54,340 (approximately $98,000) per annum! Little wonder that the brain drain from Britain continues unabated.

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Editor’s Note:
The authors declined to respond.
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In “The Risk of Pregnancy After Vasectomy” by D. J. Jamieson, MD, MPH, C. Costello, MPH, J. Trussell, PhD, S. D. Hillis, PhD, P. A. Marchbanks, PhD, and H. B. Peterson, MD, for the U.S. Collaborative Review of Sterilization Working Group (Obstet Gynecol 2004;103:848–50), a date from a data collection form was entered incorrectly, which resulted in a mistake in the point estimates and 95% confidence intervals for the 1-year failure rates. Although the overall conclusions do not change, the point estimate for the 1-year failure rate does. The incorrectly reported point estimate is 9.4 (95% CI 1.2, 17.5) and the correct point estimate is 7.4 (95% CI 0.2, 14.6). The 95% confidence intervals for the incorrect and correct estimates overlap and, thus, the incorrect and correct point estimates are within the same range. This error in data entry does not affect the 5-year failure rates, upon which we base our conclusions.

The article should be corrected as follows:

1. Abstract (Page 484, lines 13–14):
   Current: “...per 1,000 procedures (95% confidence interval) was 9.4 (1.2, 17.5) 1 year after vasectomy. ...”
   Corrected: “...per 1,000 procedures (95% confidence interval) was 7.4 (0.2, 14.6) 1 year after vasectomy. ...”

2. Results (Page 849, second column, line 10):
   Current: “...(95% confidence intervals) was 9.4 (1.2, 17.5). ...”
   Corrected: “...(95% confidence intervals) was 7.4 (0.2, 14.6). ...”

3. Results (Page 849, second column, line 17):
   Current: “...1,000 procedures would be 11.2 (2.3, 20.1) at 1 year after. ...”
   Corrected: “...1,000 procedures would be 9.2 (1.2, 17.3) at 1 year after. ...”

In “Rofecoxib Versus Magnesium Sulfate to Arrest Preterm Labor: A Randomized Trial” by J. McWhorter, S. J. Carlan, T. D. O’Leary, K. Richichi, and W. F. O’Brien (Obstet Gynecol 2004;103:923–30), the table column headings are reversed in Table 3. “Rofecoxib group (N = 105)” should be the first column heading and “Magnesium sulfate group (N = 109)” should be the second column heading.

The ACOG District II meeting was incorrectly listed as October 22–24, 2004. The correct dates are October 29–31, 2004.