

Sustained Relief of Leiomyoma Symptoms by Using Focused Ultrasound Surgery

To the Editor:

I was shocked and dismayed by the recent exchange in your journal regarding Dr. Parker and Dr. Stewart¹ about the recent article Dr. Stewart and colleagues published.² In his letter, Dr. Parker raised 10 important questions that were in need of addressing to assess the value and importance of the article. In reply, Dr. Stewart answered none of those queries.

Dr. Stewart states that a letter to the editor is not the proper forum for a detailed reanalysis of the complex data set presented in the manuscript. First, I feel sure that the data have already been analyzed; it would not have been appropriate to design the trial in such a way that the questions asked were not answered by Dr. Stewart or the sponsor at the conclusion of the study. Thus, a "detailed reanalysis" would in fact be simply reporting what was left out of the manuscript. Furthermore, a letter to the editor is precisely where such information should be transmitted, especially if it was omitted by error or intent in the original manuscript.

While I am sure that the manuscript underwent "rigorous peer review that is characteristic of *Obstetrics & Gynecology*," it is also clear that some key points were overlooked in that peer-review process. Dr. Parker's letter pointed out some of those key issues, and it would behoove the authors to take such questions seriously so that those of us treating patients can put this information to the most appropriate clinical use possible. Simply ignoring the questions merely suggests that the information might be deemed detrimental to the sponsors.

I am saddened that Dr. Stewart chose to ignore these simple, well-founded questions. I am even more distressed that this type of response was deemed acceptable to the journal editors.

Financial Disclosure

The author has no potential conflicts of interest to disclose.

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REFERENCES

1. Parker WH. Sustained relief of leiomyoma symptoms by using focused ultrasound surgery. *Obstet Gynecol* 2007; 110:1173.
2. Stewart EA, Gostout B, Rabinovici J, Kim HS, Regan I, Tempany CM. Sustained relief of leiomyoma symptoms by using focused ultrasound surgery. *Obstet Gynecol* 2007;110:279–87.

In Reply:

We are grateful that our report on the sustained efficacy of magnetic resonance-guided focused ultrasound surgery (MRgFUS) for uterine leiomyomas has stimulated much discussion including two prior letters to the editor, to which we have replied. We maintain that letters to the editor are not the appropriate forum for presenting detailed information from clinical studies. However, since Dr. Olive intimates that this belief suggests that we are hiding detrimental information, we will present additional information that may help physicians determine which women are appropriate candidates for MRgFUS. However, the more important message in this early experience is that sustained relief is possible with extensive treatment. The original questions are in italicized text and our reply follows.

1) *How many women with myomas, who presented for MRgFUS at any time, were candidates for treatment, and how many were deemed not to be candidates?* Only one study has this screen failure information available. Sixty percent of women (160 treated patients out of 264 women presenting for screening) were eligible for treatment. Of the 40% of women who were screen failures, the major category was inadequate level of symptoms on the Uterine Fibroid Symptoms Quality Of Life Questionnaire (20% or 21 of 104). Sixteen percent of women had leiomyomata that could not be treated, 10% had pathology other than a leiomyoma, and 6% had either bowel or scars in the treatment path. An additional 6% were referred

for hysteroscopic myomectomy, and 5% were claustrophobic. Thirteen percent withdrew after screening.

2) *How many women had treatment attempted?* All women who had attempted treatment were included in this report, as seen by the large number of patients with low nonperfused volume ratios.

3) *How many women were not treated because of proximity of bowel, bladder, ureters, nerves, or for technical reasons?* This is a rare reason for inability to treat as detailed in question 1 above.

4) *How many women were eligible for the higher nonperfused volume treatment (greater than 20%), but could not tolerate it or for whom it could not be technically performed?* The major limitation to treatment was not tolerability but the U.S. Food and Drug Administration-mandated treatment volumes which placed upper limits on treatment (a maximum of 33% and 50% of leiomyoma volume depending on the protocol).¹ However, the number of treatments halted prematurely decreased from 13% in the earlier protocols, where over half were stopped by the patients, to 3% in the latest protocol, where all premature terminations were due to inability to achieve appropriate temperature at the focus. All are reported.

5) *Since 33 women did not have gadolinium used for the magnetic resonance imaging, how was nonperfused volume calculated for the cohort, or were those women also excluded from analysis?* As reported in our article, this subgroup did not have gadolinium before treatment (p. 279), but did have gadolinium after treatment to calculate the nonperfused volume ratio (p. 282).² This would lead to an erroneous overestimation of the treatment effect, since all nonperfused areas would be attributed to treatment effect. Thus, these patients were included.

6) *Of the 359 treated women actually reported in the study, how many women were lost to follow-up?* Four were lost by 3 months, 10 by 6 months, 45 by 12 months, and 63 by 24 months.

7) *Of all the women considered for treatment (intent to treat), how many women had another type of leiomyoma treatment in the 24 months of follow-up?* An intention-to-treat analysis does not



Table 1. Alternative Procedures at 24 Months After Magnetic Resonance-Guided Focused Ultrasound Surgery

Nonperfused Volume Groups (%)	Average Nonperfused Volume (%)	Count	Alternative Treatment Within 24 Months [n (%)]
0–10	4	69	33 (48)
10–20	15	55	24 (44)
20–30	25	37	13 (35)
30–40	35	26	6 (23)
More than 40	53	29	5 (17)

include women “considered for treatment,” but only women assigned to treatment.³ An argument could be made for including the women who withdrew after screening, but since this information was not available for all groups, women undergoing treatment are reported.

8) *Of the 359 women actually treated, how many women had another type of leiomyoma treatment in the 24 months of follow-up?* As we reported in a previous letter, a substantial number of women from these early series underwent alternative treatments, but the incidence of alternative treatments is directly related to the extent of treatment as determined by the nonperfused volume ratio (Table 1).

9) *Of the 359 women treated, how many women had any type of complication or adverse outcome, and what were these?* The most common adverse events and the percentage of women reporting each were abdominal pain (33%), positional pain (16%), bladder or urinary catheter symptoms (14%), back or leg pain with sonications (13%), nausea or vomiting (11%), and abnormal vaginal discharge (11%).

10) *Were the 9% of women who became menopausal during the 24 months excluded from the leiomyoma symptom score analysis, in that their symptoms would likely abate as a result of menopause?* No. We are continuing to analyze and report new data on this technology and to pursue the option of a randomized trial of this novel technology compared with other treatment options. However, we face both the issue of no perceived need and no available funding for such studies. We hope to encourage Drs. Parker and Olive to become advocates for leiomyoma research to help address all the important questions in this field.

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REFERENCES

1. Fennessy FM, Tempny CM, McDannold NJ, So MJ, Hesley G, Gostout B, et al. Uterine leiomyomas: MR imaging-guided focused ultrasound surgery—results of different treatment protocols. *Radiology* 2007;243:885–93.
2. Stewart EA, Gostout B, Rabinovici J, Kim HS, Regan L, Tempny CM. Sustained relief of leiomyoma symptoms by using focused ultrasound surgery. *Obstet Gynecol* 2007;110:279–87.
3. Moher D, Schulz KF, Altman DG. The CONSORT statement: revised recommendations for improving the quality of reports of parallel-group randomised trials. *Lancet* 2001;357:1191–4.

External Cephalic Version for Breech Presentation With or Without Spinal Analgesia in Nulliparous Women at Term: A Randomized Controlled Trial

To the Editor:

We read with interest the scholarly executed randomized controlled trial about the use of spinal analgesia for external cephalic version in nulliparous women at term by Dr. Weiniger and colleagues.¹ This study evoked some validation to our own practice pattern where spinal analgesia is offered to all women with fetal breech presentation at 39 weeks of gestation. This practical solution offers immediate cesarean delivery if external cephalic version fails or significant complication occurs because analgesia is already administered.

However, the luster of the study was lost for two reasons. At first, no delivery indication other than fetal malpresentation (breech) was reported, for example, oligohydramnios. In the last paragraph of the Discussion section, the author mentioned that morbidly obese patients and patients with low

amniotic fluid volumes were excluded from the study analysis for valid reasons, although in the Material and Methods section, amniotic fluid volume was not described as exclusion criteria. An omission of these data could potentially alter final study statistics.

Secondly, it is vague why morbidly obese patients should be excluded from the study. Regardless of the lower success external cephalic version rates, these patients would benefit the most from successful external cephalic version while eliminating mounting risks related to major abdominal surgery and obesity. It is also unclear whether external cephalic version was offered to these patients at the first place or morbidly obese patients were deemed to be poor candidates for this type of surgery. An elimination of morbidly obese patients could introduce unwanted bias in data collection, regardless of the study claim that maternal weight at the time of delivery was not associated with external cephalic version success rate.

Otherwise, this is very good study that validates use of spinal analgesia as a major, and probably the most important variable, in addition to maternal compliance and motivation, for the success of external cephalic version at term.

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REFERENCE

1. Weiniger CF, Ginosar Y, Elchalal U, Sharon E, Nokrian M, Ezra Y. External cephalic version for breech presentation with or without spinal analgesia in nulliparous women at term. *Obstet Gynecol* 2007;110:1343–50.

In Reply:

We welcome the support of Dr. Predanic regarding use of spinal analgesia for external cephalic version in nulliparous



aras at term. The study recruits were candidates for external cephalic version, and the indication for performance of external cephalic version was breech position of the fetus. Subsequent delivery management was a separate procedure from the performance of external cephalic version.¹

Dr. Predanic observed that we did not specify amniotic fluid index below 5 as an exclusion criterion, and we are appreciative that he noticed this oversight which was referred to in our discussion. External cephalic version is performed infrequently among parturients with oligohydramnios in our population,² but may reduce the success rate as noted in our discussion.

Clearly, morbidly obese women have a great potential gain from successful external cephalic version. The latest confidential enquiry into maternal deaths highlights obesity as a contributing factor to death from sepsis after cesarean delivery.³ However, we elected to exclude this potentially confounding factor by not recruiting morbidly obese candidates (those weighing more than 40 kg/m²) in our random sample of nulliparas undergoing external cephalic version. Furthermore, this study was not designed to comment on maternal weight with statistical certainty; thus, our report of no association of maternal weight and external cephalic version among our study recruits is not authoritative.

We are not advocating withholding spinal analgesia for external cephalic version in morbidly obese women, and external cephalic version is performed in morbidly obese women in our institution. However, we wish to clarify that further study is needed to advise whether spinal analgesia has the same beneficial effect in nulliparas with morbid obesity.

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REFERENCES

1. Weiniger CF, Ginosar Y, Elchalal U, Sharon E, Nokrian M, Ezra Y. External cephalic version for breech presentation with or without spinal analgesia in nulliparous women at term. *Obstet Gynecol* 2007;110:1343–50.
2. Ben-Meir A, Elram T, Tsafirir A, Elchalal U, Ezra Y. The incidence of spontaneous version after failed external cephalic version. *Am J Obstet Gynecol* 2007;196:157.e1–3.
3. Lewis G. The confidential enquiry into maternal and child health (CEMACH). Saving mothers' lives: reviewing maternal deaths to make motherhood safer 2003–2005. The seventh confidential enquiry into maternal deaths in the United Kingdom. London: CEMACH; 2007.

Getting to Havarti: Moving Toward Patient Safety in Obstetrics

To the Editor:

Dr. Veltman's Current Commentary "Getting to Havarti" presents a good summary of how Reason's "Swiss cheese" model of accident occurrence can be applied to a reduction in perinatal injury.¹ I believe, however, his recommendations of "an obstetrician available in labor and delivery at all times . . ." and "having the whole team available in house . . . dedicated anesthesia services, operating room personnel and the availability of operating rooms to manage any emergency that may arise" represent, at best, unrealistic goals. Of course, the ideal hospital would have a sufficient number of continuous dedicated operating rooms fully staffed with *all* necessary personnel gowned and gloved, waiting for the unannounced patient to crash through the doors with the unpredicted prolapsed cord. Anything short of this does reflect some sort of practical compromise; the arbitrary 30-minute decision-to-incision cesarean delivery standard is perhaps the

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best known.² At rural facilities with less than 200 deliveries annually, by Dr. Veltman's own admission, an intrapartum asphyxia event leading to cerebral palsy may occur once every 48 years. Are there any data to suggest an in-house laborist at each of these facilities would change this rate? Despite the lack of evidence, I fear recommendations appearing in a respected peer-reviewed journal may become de facto "standards of care" with unintended consequences—witness the effect that the recommendation of the American College of Obstetricians and Gynecologists (ACOG) for "immediate" cesarean delivery capability had on national vaginal birth after cesarean (VBAC) rates.³ However, unlike a trial of labor, which has an available viable solution of scheduled repeat cesarean delivery, inability to meet this proposed recommendation leads to two choices for an affected institution: provide obstetric services at a local reduced standard of care and face liability for poor outcomes or close obstetric services and transfer patients out. In short, getting to Havarti may require a prolonged trip in labor to the nearest tertiary care center.

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REFERENCES

1. Veltman LL. Getting to Havarti: moving toward patient safety in obstetrics. *Obstet Gynecol* 2007;110:1146–50.
2. American Academy of Pediatrics, American College of Obstetricians and Gynecologists. Guidelines for perinatal care. 6th ed. Elk Grove Village (IL): AAP; Washington, DC: ACOG; 2007. p. 159.
3. Vaginal birth after previous cesarean delivery. ACOG Practice Bulletin No. 54. American College of Obstetricians and Gynecologists. *Obstet Gynecol* 2004;104:203–11.

In Reply:

The purpose of my Current Commentary was to suggest several areas that might be targeted to improve patient safety in obstetrics. I agree with Dr. Mirabello that rural hospitals may not be ready to embrace the concept of having a staff of obstetric hospitalists. But there are other things that were mentioned in the article that smaller labor and delivery units can do. They can work toward improving communications, conduct drills for the management of certain emergencies, develop protocols for the safe use of the medi-



cations used most often on labor and delivery, and conduct effective peer review. All of those activities will make the holes in the Swiss cheese smaller. I would also agree with Dr. Mirabello that, to my knowledge, there are no controlled studies proving that obstetric hospitalists improve outcomes. I would expect, however, with the popularity of the obstetric hospitalist movement nationally, such studies will be soon forthcoming. There is, in addition, a host of anecdotal data that all of us who have practiced obstetrics for a while have seen where a near miss or a potential catastrophe was averted by the presence of an obstetrician on the labor and delivery unit at just the right time. With respect to his comments about vaginal birth after cesarean delivery and the need for the team to be “immediately” available, I would only say that, in my opinion, this guideline¹ was based on sound evidence.² Finally, I am flattered that Dr. Mirabello thinks that this article might, in some way, lead to a change in the standard of care. I do agree with him that sometimes with a particularly difficult clinical situation, good judgment will demand transfer of the patient to a tertiary center. I do not see that as a negative.

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REFERENCES

1. Vaginal birth after previous cesarean delivery. ACOG Practice Bulletin No. 54.

American College of Obstetricians and Gynecologists. *Obstet Gynecol* 2004;104:203–11.

2. Leung AS, Leung EK, Paul RH. Uterine rupture after previous cesarean delivery: maternal and fetal consequences. *Am J Obstet Gynecol* 1993;169:945–50.

Abortion and Clostridial Toxic Shock Syndrome

To the Editor:

I found the use of the term “abortion” in the editorial by Soper¹ to be disconcerting. The same comment also applies to the article by Cohen et al.² Granted, Cohen et al do qualify abortion as medical and spontaneous in the title of the article.

The term abortion as a stand-alone noun is not descriptive, and in today’s political climate its use is unfortunate. A more precise and, in my opinion, a more politically correct term is “termination of pregnancy.” Examples of using this terminology include the American College of Obstetricians and Gynecologists (ACOG) Practice Bulletin Number 77³ and the American Medical Association (AMA) Council Report⁴ titled “Induced Termination of Pregnancy Before and After Roe v Wade.”

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The author has no potential conflicts of interest to disclose.

The reply authors chose not to respond.

A MEDLINE search using “pregnancy” and “termination” showed over 2,000 articles demonstrating that the expression “termination of pregnancy” is now accepted. Sagili and Divers⁵ suggest that the term “abortion” should not be used in clinical practice, and they offer more appropriate alternatives.

As *Obstetrics & Gynecology* is an ACOG publication, I suggest that the word “abortion” not be used in future.

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REFERENCES

1. Soper DE. Abortion and clostridial toxic shock syndrome. *Obstet Gynecol* 2007;110:970–1.
2. Cohen AL, Bhatnagar J, Reagan S, Zane SB, D’Angeli MA, Fischer M, et al. Toxic shock associated with *Clostridium sordellii* and *Clostridium perfringens* after medical and spontaneous abortion. *Obstet Gynecol* 2007;110:1027–33.
3. Screening for fetal chromosomal abnormalities. ACOG Practice Bulletin No. 77. American College of Obstetricians and Gynecologists. *Obstet Gynecol* 2007;109:217–26.
4. Induced termination of pregnancy before and after Roe v Wade. Trends in the mortality and morbidity of women. Council on Scientific Affairs, American Medical Association. *JAMA* 1992;268:3231–9.
5. Sagili H, Divers M. Modern management of miscarriage. *Obstet Gynaecol (Lond)* 2007;9:102–8.

