amniocentesis with all pregnancy losses before 24 weeks in the control group.

In our reply to Alfirevic and Tabor in May 2007, we again explained that no such comparison was made in the FASTER Trial (Eddleman KA, Malone FD. Pregnancy loss rates after midtrimester amniocentesis. Obstet Gynecol 2007;109:1204 [letter-reply]). Instead, only patients who successfully reached 15 weeks of gestation were included in the FASTER Trial. Otherwise, they could not have had both the first- and second-trimester aneuploidy screens specified by the trial protocol. Therefore, the claim that the FASTER Trial included pregnancy losses that occurred before 15 weeks of gestation in the control group is false. Instead, our amniocentesis study does indeed compare all amniocentesis patients with control patients who had viable pregnancies between 15 and 24 weeks of gestation. As such, we feel that the FASTER Trial provides amniocentesis and control cases that are sufficiently well matched to allow the valid comparisons that support our conclusion of an extremely low procedure-related loss rate for amniocentesis.

Fergal D. Malone, MD
Royal College of Surgeons in Ireland, Dublin, Ireland

Keith A. Eddleman, MD
Mount Sinai School of Medicine, New York, New York, on behalf of the FASTER Consortium

REFERENCE

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In Reply:

It is certainly reassuring to know that the control group in the First and Second Trimester Evaluation of Risk (FASTER) study, which showed such an extremely low amniocentesis-related pregnancy loss (0.06%),[1] was recruited from the cohort "who successfully reached 15 weeks" (Alfirevic Z, Tabor A. Pregnancy loss rates after midtrimester amniocentesis. Obstet Gynecol 2007; 109:1203–4 [letter]). We disagree, however, that this, per se, constitutes adequate matching for gestational age. The background risk of miscarriage is gestation-dependent,[2] and relatively small differences in the mean gestational age between two groups could introduce significant bias.

Our comment related to the gestational age as a matching criterion was referring to a lack of studies where each index case (amniocentesis) is matched with an appropriate number of controls still pregnant in the same gestational week, ideally with a priori sample size calculations.

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First- and Second-Trimester Evaluation of Risk for Down Syndrome

To the Editor:

In their cost-effectiveness study on prenatal screening for Down syndrome, Ball et al[1] put a price tag on an extra 21st chromosome. According to the authors, "The societal cost of raising and caring for an individual with Down syndrome is $762,748." This estimate stems from incremental direct costs (eg, extra expenses from inpatient hospital stays, outpatient medical visits, long-term care, and developmental services) and indirect costs (eg, lost productivity due to morbidity and early mortality) that were calculated for persons with Down Syndrome from California in 1988.[2] If taken literally, this estimate would suggest to expectant parents that the lifetime bill for raising a child with Down syndrome would be about an extra three quarters of a million dollars more than if they were to have a child with a normal karyotype.

This cost estimate is incomplete and, in its worst use, deceptive. Previous research and testimony support the notion that there are incremental cost benefits to having a child with Down syndrome. Some of these savings are tangible: mothers and fathers of children with Down syndrome incur savings from statistically lower rates of divorce compared with parents who do not have children with disabilities.[3] Other benefits have not–and probably cannot–be calculated: brothers and sisters of people with Down syndrome have distinctive emotional advantages,[4] and parents of children with intellectual disabilities report having increased sensitivity, tolerance, perspective, patience, and purpose, among other qualities.[5]

Until science and economics can accurately estimate, in financial terms, the full extent of the savings and expenses associated with Down syndrome, cost-benefit analyses have limited value. In the interim, placing dollar amounts on persons with disabilities remains a purposeless, and concerning, effort.

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Financial Disclosure
The author has no potential conflicts of interest to disclose.

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In Reply:

We thank Dr. Skotko for pointing out an issue regarding cost-effectiveness analyses of having a child with Down syndrome. He suggests that the cost estimate we utilized in the study may be an underestimate. This brings up several important methodologic issues that we will highlight below.

It is true that obtaining excellent

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cost estimates is more difficult in health care than almost any other industry. Because of reimbursements paid by medical insurance companies, rather than by consumers of medical care, we do not function in a strictly competitive market that would hopefully lead to an equilibrium between marginal costs and price. Thus, when we use prices, we often adjust by price-to-charge ratios which are simply industry-wide averages, not specifics. But, from a policy standpoint, one must make some estimate when attempting to examine cost-effectiveness. We based our estimate on the work by Waitzman et al[1] who, to the best of our knowledge, did the best cost work on this question.

Acknowledging that such cost estimates may vary from true costs, methodologically, we utilize sensitivity analysis to make sure findings from such studies are robust.[2] Simply, sensitivity analyses vary the inputs into cost-effectiveness models to determine how such inputs affect the outcomes of the model. We varied each of the cost inputs over theoretical ranges of their value. While this will change the absolute dollar value of the outcomes, in this case it did not change the order of what Down syndrome screening strategies were most cost-effective. Thus, as we reported, even changes of $50,000 to $100,000 in the cost of Down syndrome does not appear to affect the cost-effectiveness of the different screening strategies.

We hope that, as health care consumes an ever-larger proportion of the national budget, more attention is paid to determining actual health care costs. Until then, policy decisions still require the best available evidence, which we endeavored to provide in our analysis.

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Gestational Age at Cervical Length Measurement and Incidence of Preterm Birth

To the Editor:

Berghella et al[1] detailed the incidence of preterm birth based on the cervical length measurement and the gestational age at which it was measured. Their article presents useful tables for practitioners. Indeed, they present data that can assist in more precisely predicting the risk of preterm birth for each of our specific patients with a short cervix based on the cervical length and the gestational age. However, when looking closely at Tables 2, 3, and 4, the prediction model appears to break down at very short cervical lengths and very early gestational ages. For example, for a patient with a cervical length of 5 mm at 15 weeks of gestation, there is a 70.1% probability of birth by 28 weeks, but only a 67.9% probability by 32 weeks and only a 62.5% probability by 35 weeks. One would expect these probabilities to increase, not decrease. Could the authors please address this apparently illogical prediction in their tables?

Nathan S. Fox, MD
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REFERENCE

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In Reply:

We thank Dr. Fox for a very perceptive observation. The model predictions are indeed questionable for very short cervical lengths measured at early time points.

In Figure 1, there were few observations with a cervical length of 0 (and none in the early weeks of gestation). So, the statistical models have to extrapolate predictions in that early range. As Dr. Fox points out, these predictions are not very good.

All model predictions at the edges of the data range are doubtful. By "edges" we mean data points that are rare or extreme, specifically measurements of very short cervical lengths (less than 15 mm) at early time points (before the 20th week). In Tables 2 through 4, the predicted probabilities are quite inconsistent in the upper left-hand corners of the tables (the very short cervical lengths at early times). Reading to the right or to the bottom of the tables, the predictions become quite sensible–the probability of delivering before 28 weeks of gestation becomes smaller than the probability of delivering before 32 weeks, etc.

In retrospect, we should not have presented predictions for cervical lengths shorter than 15 mm (particularly for times less than 20 weeks) or greater than 50 mm (particularly for times greater than 24 weeks), since the data are so sparse in those ranges. We missed this when reviewing our results, and we are grateful to Dr. Fox for pointing it out. As he states, our findings can indeed ". . . assist in more precisely predicting the risk of preterm birth for each of our specific patients with a short cervix based on the cervical length and the gestational age," but they are best applied to cervical lengths between 15 mm and 50 mm.

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Financial Disclosure
The author has no potential conflicts of interest to disclose.

Sustained Relief of Leiomyoma Symptoms by Using Focused Ultrasound Surgery

To the Editor:

I read with interest the article by Stewart et al[1] in the August 2007 issue of *Obstetrics & Gynecology*. While I applaud the authors' efforts to increase our knowledge on focused ultrasound treatment of uterine leiomyomata, I am concerned with the presentation of the results from the Uterine Fibroid Symptom Quality of Life Questionnaire and the conclusions they draw from the study.

The Uterine Fibroid Symptom Quality of Life Questionnaire is a leiomyoma-specific symptom and

Financial Disclosure
The author has no potential conflicts of interest to disclose.

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