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[Three of the authors respond:]

Michael Klein argues against our conclusion¹ that planned cesarean section was safer and less expensive than planned vaginal birth during the period reported in the Term Breech Trial. He bases his arguments on other studies by us,^{2,3} which “showed no difference in outcome for the babies or the mothers” at 2-year follow-up. He also claims that by looking only at the duration of the Term Breech Trial we have “vastly underestimate[d] the real costs of elective cesarean for breech or any birth.” While we agree that a longer-term analysis might be useful, we disagree with these arguments.

The argument that our own studies show no difference at 2 years represents a misunderstanding of the results of those trials.^{2,3} The appropriate interpretation of those results is that the benefits of planned cesarean section are limited to reducing perinatal and neonatal mortality and serious neonatal morbidity during the first 6 weeks of life. These remain important benefits for the baby, the mother, the family and the health care providers.

Regarding the question of what will happen to the costs of planned cesarean section and planned vaginal birth after, say, 2 years, the answer is “we do not know.” Any argument that the costs will be higher is nothing more than speculation. For example, we agree with the assumption that most women will have more than one birth, but we do not know if breech

presentation will occur for the first birth, the last birth or a birth in between. Also, our experience from the Term Breech Trial has taught us that until actual resource utilization is measured in a controlled environment, it is not easy to predict what will happen. We thought that planned cesarean section would be more expensive than planned vaginal birth, but found that it was not. Furthermore, there was no single specific factor that explained why the costs of planned cesarean section were lower, which tells us that it is dangerous to try to predict (rather than measure) future costs.

Finally, we do not feel responsible for the headlines and content of what is published in the popular press. The Interpretation section of our paper discusses the study’s limitations and the consequent constraints on any conclusions drawn.

Amiram Gafni

Centre for Health Economics and Policy Analysis
Department of Clinical Epidemiology and Biostatistics
McMaster University
Hamilton, Ont.

Mary E. Hannah

Department of Obstetrics and Gynecology
Sunnybrook Health Sciences Centre
University of Toronto
Toronto, Ont.

Roberto Palencia

Maternal, Infant and Reproductive Health Research Unit
The Centre for Research in Women’s Health
University of Toronto
for the Term Breech Trial Steering Committee

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Multitherapy for diabetes

Julie Ménard and associates¹ used pravastatin or bezafibrate (or both) as a component of intensive multitherapy in their study of patients with type 2 diabetes mellitus. Although pravastatin is of questionable benefit (according to the ALLHAT-LLT study,² in which no cardiovascular, peripheral vascular, cerebrovascular or mortality benefits were found), my main concern here is with the use of fibrates.

Fibrates, including bezafibrate, are effective in lowering low-density lipoprotein cholesterol and triglycerides while raising high-density lipoprotein cholesterol, but there was no mortality benefit in a large bezafibrate trial.³ In addition, there have now been 3 trials with different fibrates (gemfibrozil,⁴ clofibrate⁵ and fenofibrate⁶) that ended with numerically more deaths in the group receiving fibrates than in the placebo group.

Fibrates lull patients and doctors into a false sense of accomplishment by bringing several blood lipid markers closer to guideline targets, thus reducing the urgency of more difficult dietary and lifestyle changes. However, with “over two-thirds of patients with diabetes [dying] of cardiovascular causes,”⁷ the established failure of fibrates to lower mortality should lead to an urgent call to stop their use and to examine the clinical efficacy of the lipid guidelines.

Eddie Vos

Sutton, Que.

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