Episiotomy and perineal tears: cause and effect

I would like to react to the editorial "Episiotomy and severe perineal trauma: of science and fiction" (*Can Med Assoc J* 1997;156:811-3), by Dr. Michael E. Helewa, concerning the article "Association between median episiotomy and severe perineal lacerations in primiparous women" (*Can Med Assoc J* 1997;156:797-802), by me and my associates. Although I appreciate the fact that Helewa recognized the original nature of our study, I must disagree with him about the causal relation between median episiotomy and severe perineal lacerations.

First, the classification of research designs used by Helewa is incorrect. There is no such thing as a cross-sectional study when evaluating the association between episiotomy and perineal tears. Almost all studies on the topic are true cohort studies, meaning that women either exposed to episiotomy or not are followed until birth to assess the incidence of perineal tears. Although most of these studies involve retrospective data collection, this does not invalidate the direction of the study. The distinction between cross-sectional and cohort studies is important when assessing a causal relation. The temporal principle (that cause must precede effect) can be supported by a cohort study but not by a cross-sectional (prevalence) study.

Second, Helewa mixes apples and bananas, namely median and mediolateral episiotomy. Most of the studies he refers to that imply that episiotomy is not associated with severe lacerations concern mediolateral episiotomy. In this context, his statement is correct: these studies showed either no association or a very small one. By contrast, he missed 9 out of 11 of the studies cited in our article that deal specifically with median episiotomy and severe perineal lacerations in primiparous women, all of which show a strong association. I challenge him to find a single study that does not show an association between median episiotomy and third- and fourth-degree tears.

I agree that a randomized controlled trial is the best design to demonstrate causality, but it is not the *sine qua non*. Is there any randomized trial in humans showing that smoking causes lung cancer?

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[The author responds:]

I am mystified by Dr. Labrecque's comments. The editorial does not negate the association between median episiotomy and severe perineal trauma. On the contrary, it promotes a causal relation, on the basis of evidence from the secondary analysis of Klein and colleagues'[1] randomized clinical trial. In that secondary analysis it was evident that patients who had an episiotomy accounted for the vast majority of patients suffering third- and fourth-degree tears, regardless of the group (restricted use or liberal use of episiotomy) to which they were originally allocated. However, I went a step further and posed the following question: Would adopting a policy of restricting episiotomy result in a reduction in the incidence of third- and fourth-degree tears? According to the evidence from the 2 major randomized clinical trials discussed in my editorial and from other minor randomized trials available in the literature, this desired effect might not materialize. In the 2 trials conducted in Canada and Argentina, there was no significant difference in incidence of perineal trauma between the restricted-episiotomy and the liberal-episiotomy groups.[2,3] The reasons for this observation are simple. First, a large number of physicians perform an episiotomy for perceived indications at the time of delivery, even if a selective or restrictive policy is advised. In Klein and colleagues'[2] randomized clinical trial, more than 50% of primigravid patients randomly assigned to the restricted-episiotomy group still underwent an episiotomy. Second, there are many risk factors other than episiotomy that may lead to severe perineal trauma. Labrecque and colleagues have shown that birth weight, forceps use and gestational age are independent risk factors associated with third- and fourth-degree tears. Others have suggested that shoulder dystocia or birth position are risk factors.

Labrecque criticizes the classification of the research designs presented in the editorial. This classification is not unique but has been published extensively in the past, especially by Thacker and Banta[4] and Woolley.[5] I agree with Labrecque that a cross-sectional study is a prevalence study. However, this study design was commonly used in the early articles on the topic. Authors attempted to establish a relation between episiotomy and severe perineal lacerations through the use of this design. A significant number of these early articles also failed to indicate the type of episiotomy (mediolateral or median) performed. In the cross-sectional studies in which the type of episiotomy was addressed, however, severe perineal trauma was more prevalent in women who sustained a median episiotomy.

Labrecque's comments downplay

the importance of randomized clinical trials in establishing causality. Cohort retrospective studies establish associations. Strong associations may imply causality. Still, the 2 are not the same. Fortunately, however, randomized clinical trials concerning episiotomy and its effect on trauma have been performed successfully, despite the inherent difficulties encountered in such a study design.

If Labrecque takes another look at the editorial his perceptions of what I presented may change. He may realize that the ideas promoted in my editorial are those of a friend, not a foe, to his good work.

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Controversies in spirometry

Dr. Benjamin Chan and colleagues have identified regional variations in spirometry use in Ontario physicians' offices ("Spirometry utilization in Ontario: practice patterns and policy implications," *Can Med Assoc J* 1997;156:169-76). Their observations are not surprising, considering that similar wide variations have been observed for other medical procedures.

Are these differences due to overutilization in high-rate areas, underutilization in low-rate areas or a combination of these factors? In the editorial "Spirometric testing: How much is enough?" (*Can Med Assoc J* 1997;156:202-4), Dr. Nicholas Anthonisen suggests that the overall use of spirometry in Ontario is either acceptable or too low. Chan and colleagues suggest, and Anthonisen states, that flow–volume (FV) loops (providing forced vital capacity [FVC], forced expiratory volume in 1 second [FEV_1], forced expiratory flow during the middle half of forced vital capacity [$FEF_{25\%-75\%}$] and other data) are being used excessively in comparison with simple spirograms (providing FVC and FEV_1). However, the data presented do not support these conclusions. We do not know whether FV loops were repeated for the same patients during a 1-year period or performed annually, on average. This issue is critical if one accepts Anthonisen's argument that FV loops should not be repeated more than once a year, which is arguable. I cannot reconcile his statement that "it is hard to imagine that as much as half of all flow studies could justifiably involve flow volume analysis" without any information on the number of studies carried out per patient. In the areas with the highest costs for spirometry, a mean of 5 spirometric tests per 100 population were performed during 1 year. This rate is certainly in line with the rate of asthma (3% to 5%) and of wheezing (up to 9%) in the population.[1]

The usefulness of FV loops versus simple spirograms is also discussed. The authors agree that spirometry is essential in diagnosis, assessment and follow-up of patients with obstructive lung disease. However, they question whether the FV loop, a powerful tool that provides additional information on small airway obstruction, is being overused. Anthonisen notes that FV loops, which provide information on small airway calibre, may be more sensitive than simple spirograms but that there is a wide range of normal values. Small airway obstruction, as measured by changes in $FEF_{25\%-75\%}$, may be seen in asthma and smokers before any changes in FEV_1, which measures air flow in larger airways.[2–4] The $FEF_{25\%-75\%}$ may be abnormal when the FEV_1 is normal. In this case, monitoring asthma with the use of simple spirograms may not provide necessary information. I doubt that many, if any, respirologists use simple spirometry rather than FV loops either in office settings or hospital laboratories.

The wide variation in normal values for $FEF_{25\%-75\%}$ can be taken into account through the use of well-recognized standardized reference ranges for $FEF_{25\%-75\%}$[5] and of FV loops to follow changes in small airway obstruction in response to treatment.

However, we must ensure that any tests are done for the benefit of the patient and not for purely economic reasons, especially if physicians with no special training in the area are performing high volumes of tests despite easy access to specialists. Such deviations in practice patterns are monitored by provincial health insurance plans, however, and physicians involved may be subject to audit.

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