

have been due to technical factors rather than neoplasia. This possibility was mentioned in our article where we noted the false-negative rate (poor sensitivity—no poor specificity!) inherent in Pap smear screening.

False-negative Pap smears may occur not only because of inadequate specimen collection techniques, as suggested by Dr. Baxley, but also because of problems associated with processing, screening, and interpreting the specimen.¹ The cumulative false-negative error rate, considering all stages of obtaining, processing, and interpreting a Pap smear, may be as high as 25 percent for precancerous lesions and up to 50 percent for malignant lesions.^{1,2} These inaccuracies in Pap smear screening, combined with the apparent change in the rate of development of cervical cancer, further support the need for annual cervical cytology screening.

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References

1. Koss LG. The Papanicolaou test for cervical cancer detection. *JAMA* 1989; 261:737-43.
2. Vander Graaf Y, Vooijs GP, Gaillard HL, et al. Screening errors in cervical cytology screening. *Acta Cytol* 1987; 31:434-8.

Rural Obstetric Care

To the Editor: I appreciate very much the vigorously supportive letter from Colonel Camp (April-June 1989). He was and continues to be a strong supporter of his residents, past and present.

I also read with interest and appreciation Dr. Brown's letter and accompanying references (April-June 1989). I strongly agree that Cesarean rates are too high and that shared care is an excellent method of improving them. Physicians are people, too, and subject to the same pressures as other professionals.

Currently, there is no disincentive to performing Cesarean sections, and some strong incentives to perform them. On average, they require less time than a vaginal delivery. Financially, they are much more rewarding. They can be scheduled not to interfere with one's plans, be they clinic or an evening out. If there is a bad outcome, the assumption is the physician did the ultimate—he or she performed surgery in an attempt to rescue the baby. If you do not do a Cesarean and there is a bad outcome, be prepared for a lot of hindsight inspection of the entire pre-intra-post-natal management. In my situation, referring a patient for a Cesarean causes me a loss of income. The loss does not prevent me from seeking consultation if needed, and it does not cause me to persist in hazardous labors, but it does remove one of the simple but potent incentives for a Cesarean section.

I view family practice at or near a crossroads. If family physicians continue to give up obstetrics, I think a critical mass of numbers, role models, and peers will soon

be gone, and it will remove obstetrics from family medicine. I believe it would be more efficient if we evolved a system wherein routine, low-risk care was provided entirely by family physicians, and obstetricians provided aggressive care for high-risk, complicated obstetrical patients, largely referred by family physicians. Of course, I am aggressively pro-family practice—I was trained by Dr. Camp. For your information, I have currently performed 94 vaginal deliveries (12 out of hospital [10 in clinic, 2 in cars]) and 2 Cesarean deliveries.

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Epidural Anesthesia

To the Editor: As family practitioners providing obstetrical care, we read with interest the study of epidural anesthesia in labor by Niehaus, Chaska, and Nesse (October-December 1988.) We share the authors' sentiment that natural childbirth should be encouraged, while appreciating that in certain circumstances epidural anesthesia may be of benefit to women in labor.

In interpreting their findings, the authors conclude that the "use of elective epidural anesthesia results in markedly increased odds of instrumental or operative delivery."^(p 238) We disagree that the study justifies such a strong causal influence.

The study's analysis reveals that much of the increased risk of forceps or Cesarean section delivery for women receiving epidural anesthesia is attributable to the greatly increased odds ratio for women administered epidural anesthesia in the *second* stage of labor. There was no significant increase in the risk of non-spontaneous delivery for the subgroup of nulliparous women receiving epidural anesthesia in stage one.

We question whether epidural anesthesia was truly "elective" when administered in the second stage of labor. In our clinical experience, initiating epidural anesthesia in the second stage of labor is reserved for situations of imminent or anticipated forceps or Cesarean section delivery. The predictably high association between epidural anesthesia and nonspontaneous delivery in this context, then, becomes one of effect-cause rather than cause-effect; that is, the use of forceps or Cesarean section (or anticipation of their use) is the primary decision that results in the selection of epidural anesthesia.

We suspect that the authors incorrectly assigned many patients to the "elective" epidural anesthesia cohort who had second stage epidural anesthesia administered under nonelective conditions, resulting in an erroneously inflated odds ratio for nonspontaneous delivery in this cohort. Alternatively, family physicians in the hospital under study may approach elective pain control in the second stage of labor very differently than those in our community.

We also have reservations about the authors' method of adjusting for confounding variables. In Tables 2 to 4 of their report, the authors list multiple variables such

as parity, fetal risk score, use of oxytocin, etc., that are strongly associated with the use of epidural anesthesia and also are plausible causes of nonspontaneous delivery.

The authors elect to control for these confounding variables by using stratified analysis. While stratified analysis is useful for evaluating the *individual* contribution of each confounding variable examined in isolation, this method is inadequate for analyzing the *simultaneous* contribution of all confounding variables. Stratified analysis does not, therefore, provide an odds ratio adjusted for the *cumulative* effect of all identified confounding variables.²

The authors cite Blake's article on stratified analysis to support their methodology,³ but Blake himself cautions in the conclusion to his article that "multiple regression is often superior to stratified analysis when there is a need to assess conjoint confounding by two or more variables." (p 225)

We are confused why the authors' presentation of their regression analysis is so cursory. While their report devotes considerable text and seven figures to stratified analysis, their presentation of multifactorial analysis is limited to the comment that "regression analysis for all these factors failed to eliminate the increased odds ratios for patients who received epidural anesthesia." (p 241)

Exactly *which* factors were included in the regression analysis? Was the analysis limited to only six variables described as showing effect modification on stratified analysis (an entirely different phenomenon than confounding), or were all variables associated with epidural use included in the regression analysis? Why are no actual numerical values provided for the adjusted odds ratio and *P* value calculated by regression analysis? And lastly, why does the methods section make no mention of the statistical model and instruments used for the regression analysis?

The decision by a woman and her physician to use epidural anesthesia in labor is often a difficult one. The published research on the impact of epidural anesthesia on labor outcomes remains clouded by conflicting conclusions, widely variant obstetrical practice patterns, and poor study designs. There has never been—and for ethical reasons, is unlikely ever to be—a randomized controlled trial of epidural anesthesia in labor that could accurately evaluate the independent contribution of this intervention to labor outcomes.

Unfortunately, because of the many methodological problems we have discussed, we believe the study by Niehaus and colleagues cannot meaningfully contribute to clarifying the risks and benefits of epidural anesthesia. It would be unfortunate if family physicians seeking guidance in this area interpreted this study as a compelling reason to withhold epidural anesthesia in instances in which its use might prove advantageous.

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References

1. Niehaus LS, Chaska BW, Nesse RE. The effects of epidural anesthesia on type of delivery. *J Am Bd Fam Pract* 1988; 1:238-44.
2. Newman TB, Browner WS, Hulley SB. Enhancing causal inference in observational studies. In: Hulley SB, Cummings SR, eds. *Designing clinical research: an epidemiologic approach*. Baltimore: Williams & Wilkins, 1988:98-109.
3. Blake RL. The use of stratified analysis to detect confounding and interaction in primary care research. *Fam Pract Res J* 1985; 4:219-25.

The above letter was referred to the authors of the article in question, who offer the following reply.

To the Editor: We are pleased by the continuing interest in our article, "The Effects of Epidural Anesthesia on the Type of Delivery." I believe some of the concerns raised by Doctors Grumbach and Abrams's letter have been answered in our response to letters published in a previous issue of this journal (April-June 1989). However, these correspondents raise two issues that need clarification.

Doctors Grumbach and Abrams's major concern is that the epidural blocks performed in our study were given because of the need to provide anesthesia for a planned procedure such as forceps delivery or Cesarean section. In the methods and study design section of our paper, we reported that the low-risk obstetrics patients studied received an epidural block electively for pain relief only. Those patients in which the epidural block was medically indicated were eliminated from the study.

The correspondents also expressed concern about the use of stratified analysis to identify effect modifiers. We compared obstetrical characteristics and demographics between low-risk patients who received an epidural block and those who did not. In examining the differences, we found effect modification present for 10 variables overall and found significant effect modification for six variables. We used regression analysis to examine these variables individually and in combination, and certain combinations (notably, nulliparous women who were not given a continuous epidural block) did decrease the odds ratio for instrumental delivery between patients receiving epidural block and those who did not. However, no combination of variables studied eliminated the use of epidural block as an independent risk factor for instrumental delivery.

We agree with Doctors Grumbach and Abrams that research on epidural block to date has not provided a comprehensive and clear answer to their question of whether epidural blocks *cause* instrumental deliveries. I suspect that no prudent authors doing retrospective work will be willing or able to provide them with this answer. However, our work does show that in low-risk obstetrics patients, the use of an epidural block is associated with an increased frequency of instrumental delivery. In addition, our work shows this increase is not