Correspondence

We will try to publish authors' responses in the same edition with readers' comments. Time constraints may prevent this in some cases. The problem is compounded in the case of a quarterly journal where continuity of comment and redress is difficult to achieve. When the redress appears 3 months after the comment, 6 months will have passed since the original article was published. Therefore, we would suggest to our readers that their correspondence about published papers be submitted as soon as possible after the article appears.

Epidural Anesthesia

To The Editor: In the article "The Effects of Epidural Anesthesia on Type of Delivery" (October–December 1988), the authors postulate that epidural anesthesia increases the frequency of instrumental and operative deliveries for low-risk obstetric patients under the care of family physicians. Compared with women without epidural anesthesia, women with epidural anesthesia in the retrospective cohort study had an almost tenfold greater risk of a forceps or operative delivery. Controlling for one or two potential confounders at a time by a stratified Mantel-Haenszel analysis, the odds ratio decreased somewhat but still maintained significance, except in the case of nulliparous women who elected to have epidural anesthesia during the first stage of labor. The abstract states that this multivariant analysis controlled for bias and did not change conclusions significantly. In actuality, the multivariant analysis controlled for the effect of confounding, not bias.

Bias is any systematic error in the design, conduct, or analysis of a study that results in a mistaken estimate of exposure risk. Biases in case ascertainment, exposure status, and selection of subjects may occur in any study attempting to estimate risk. Ascertainment, exposure, and misclassification biases are unlikely in this study, as it appears the authors took great care in selecting an appropriate cohort of patients and studied relatively objective outcomes obtained from the medical record. A significant bias that is a distinct possibility, however, is a surveillance bias.

A surveillance bias occurs when physicians know the exposure status of the patient. Because of that knowledge, they are often more likely to diagnose case status, resulting in a spurious association. Specifically, in this study, physicians caring for patients who requested epidural anesthesia may have been more likely to apply forceps or to suggest a Cesarean section to those patients, because previous studies and generally accepted clinical dictums have suggested that patients with epidural anesthesia may need such an interventionist approach in order to deliver successfully. The way to control for this potential bias is to blind physicians so that they do not know a patient's anesthesia status when they are suggesting a delivery method. It is important to emphasize again that in this study, multivariant analysis will not control for a surveillance bias or other biases that might have affected the results.

In addition, the authors could have used a stronger technique for controlling for the effects of multiple potential confounders at the same time, rather than adjusting the odds ratio for just one or two potential confounders at a time. That technique, logistic regression, would have controlled for multiple potential confounders at the same time, such as stage when the epidural anesthesia was administered, physician's estimate of pelvic adequacy, station at last-recorded examination, marital status, gestational age, socioeconomic status, and parity. Because the sample size is certainly adequate to conduct such an analysis, and because logistic regression programs are readily available on most mainframe computer statistical packages, such an analysis would have greatly aided readers in determining if there was an association between epidural anesthesia and forceps or operative deliveries or if this association was merely the product of a confounding effect.

Mark B. Mengel, M.D.
University of Oklahoma
Oklahoma City, OK

To the Editor: The conclusion reached by Niehaus, et al.[1] regarding increased risk of instrumental delivery with epidural anesthesia may not be justified on the basis of data presented. The two groups are shown to have differed significantly in demographic, labor, and obstetric risk factors. While the authors recognized the importance and controlled statistically for the differences, the error introduced by having dissimilar populations may have been too great to control by statistical methods alone. For example, 34.5 percent of women in the epidural group were high risk under the Morrison scale, but only 9.3 percent of women in the nonepidural group were so classified.

Moreover, the authors have failed to consider some important confounders; e.g., significantly more of the epidural group were married and middle-to-upper social status than the nonepidural group. It might be suspected that there were also more employed women in the epidural group. Smith, et al. reported that maternal employment was itself associated with at least a fivefold increased risk of operative delivery.[2] Previous method of delivery was not addressed in this study. The finding that oxytocin was used in 51.4 percent of the labors with epidural anesthesia versus 18.3 percent in those without may indicate a higher occurrence of dysfunc-