

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 multivariate analysis controlled for bias and did not change conclusions significantly. In actuality, the multivariate 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, multivariate 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-

tional labors in the epidural group. Not noted was the duration of stage 1 or whether oxytocin was begun before or after the epidural was in place.

The authors also fail to separate out one subgroup that may not be at increased risk. Presumably, one reason for instrumental delivery is abolition of the mother's "urge to push" by epidural anesthesia. In our experience, women who have undergone a previous spontaneous vaginal delivery (without epidural anesthesia) are able to push adequately if they are given proper cues to timing of efforts.

It may be tempting to extrapolate the findings reported in this study and conclude that epidural anesthesia causes instrumental delivery and therefore should be avoided. We believe that caution should be exercised before drawing such a conclusion for all women who may request epidural anesthesia. In his comment, Dr. Duhring asserts that, "women well trained in LeBoyer or Lamaze childbirth do not find the pain of labor overwhelming."

Circumstances such as prolonged labor or PitocinTM induction/augmentation can indeed overwhelm the psychic resources of even the most well-trained patient. To deny these women, especially those who have undergone a previous spontaneous delivery, the benefits of epidural anesthesia would be a grievous error.

Janice E. Daugherty, M.D.

Ronnie Horner, Ph.D.

East Carolina University School of Medicine
Greenville, NC

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. Smith MA, Brix KA, Heaton CJ. The influence of work on the outcome of low-risk pregnancies. *J Am Bd Fam Pract* 1988; 1:167-74.

The above letters were referred to the authors of the article in question, who offer the following reply:

To the Editor: On behalf of all the authors, I would like to thank Doctors Mengel, Daugherty, and Horner for their interest in our article and thoughtful comments.

Doctor Mengel's letter summarizes a definition of bias very nicely and specifically suggests that surveillance bias is present in our study. Of course, the presence of surveillance bias is a basic truth in any retrospective study and is readily acknowledged by our group. Doctor Mengel's letter goes on to suggest that a prospective study done in a blinded fashion is needed. While this has great merit, there are some practical limits. It is difficult to conceive how one could truly construct a double-blind study, given the equipment used for continuous epidural blocks. In addition, the vast majority of physicians believe that knowledge of the patient's anesthesia status is essential for proper medical care delivery. Doctor Mengel's letter concludes that

control of multiple confounders at the same time was needed. Indeed, the study did simultaneously control for all of the 10 variables listed and found that the effect of epidural anesthesia on the outcome of labor and delivery remained significant.

Doctors Daugherty and Horner's letter addresses the differences found in the patient cohort receiving epidural block versus the cohort that did not. It is important to note that subgroups were compared that were matched for risk. It is our belief that statistical methods were adequate to control for the differences and allowed us to select comparable subgroups from the total population. Daugherty and Horner's letter also points out other population factors not directly studied that may have a significant effect on labor outcome. One could not debate that the use of PitocinTM to augment labor is necessary due to the effect of epidural block. However, we acknowledge that there may be many modifiers beyond the 10 we listed in our article that adversely effect delivery type. We strongly agree with Doctors Daugherty and Horner that, while it may be tempting, one should not extrapolate the findings of our study and conclude that epidural anesthesia is the "cause" of instrumental delivery. However, as we stated in our article, ". . . epidural anesthesia during labor is associated with increased instrumental and operative deliveries . . ." (p 242)

Would selective application of epidural blocks in a yet-to-be-defined optimal subgroup of patients completely eliminate the adverse effect of the procedure? Can the present adverse effect of epidural anesthesia be completely eliminated by different labor management techniques? The answers to these important questions will need to be supplied to physicians by further studies of low-risk obstetric patients.

Robert E. Nesse, M.D.
Mayo Clinic
Rochester, MN

Medical Ethics

To the Editor: I was dismayed by many aspects of Dr. Pellegrino's article entitled "Medical Ethics: Entering the Post-Hippocratic Era" (October-December 1988) and would like to address what I believe are significant shortcomings in his paper and possible sources of misunderstanding.

In his "Deconstruction of the Hippocratic Precepts," Dr. Pellegrino sees in the preamble of the *Oath* a "sexist, elitist, monopolistic, and wholly inappropriate" concept of medicine. I would point out that, from an educational and socioeconomic standpoint, entrance into medical school is now as about as elitist as one could ever imagine. Giving this instruction to "my sons and those of my teacher" can be understood as the modern physician's obligation to teach medical students and residents. Furthermore, the injunction to reserve this teaching to those properly sworn and engaged can be