CORRESPONDENCE

Anesthesiology
2000; 92:623

In Reply:—I want to thank Dr. Malinow for his interest in our study.[1] Malinow discusses several concerns that, in his estimation, diminish the evidence that outcomes are similar following epidural or spinal anesthesia in severely preeclamptic patients.

The intent of publishing this retrospective study was to provide a series of anesthetic cases that are supportive, but by the nature of retrospective analysis, cannot be "convincing" support for the absolute safety of spinal and epidural anesthesia in the severely preeclamptic patient. Our study supports the contention that it is ethical to prospectively study epidural and spinal anesthesia in this high-risk patient population. We think that our study also supports the position that it is ethical to use spinal anesthesia in some circumstances. Are the two anesthetic techniques absolutely similar and interchangeable? Probably not, in all circumstances and in all patient populations.

Dr. Malinow questions the validity of comparing our epidural group with our spinal anesthesia group because of the uncontrolled nature of the anesthetic methods. As discussed in our report, the regional anesthetic methods were uncontrolled in this retrospective study. We would also agree that a large bolus of a relatively rapidly acting epidural local anesthetic might produce blood pressure reductions similar to those seen with spinal anesthesia. Whether a 10-ml bolus of epidural local anesthetic necessarily produces reductions in blood pressure similar to those produced by a single-shot spinal anesthetic with enough local anesthetic to produce an upper thoracic block is questionable. We provided data and discussion on this point, and specifically indicated in the Results section that "blood pressure response was similar when comparing spinal anesthetics to epidural anesthetics using some 0.5% bupivacaine or 3.0% 2-chloroprocaine."[1] Our intent was to indicate that those patients receiving slow-onset epidural bupivacaine had blood pressure reductions similar to those in patients receiving only rapidly acting 2-chloroprocaine epidural local anesthetic. The indication that blood pressure reductions were similar despite the choice of epidural local anesthetics was the support we could generate given the constraints of the study design.

Dr. Malinow's second point concerns laboring *versus* nonlaboring patients and the resulting reduction in blood pressure following the induction of spinal or epidural anesthesia. Dr. Malinow incorrectly asks why baseline blood pressures are similar in both laboring and nonlaboring patients, citing our study and the study by Wallace *et al.*[2] Although the prospective study by Wallace *et al.*[2] included laboring patients—one third of which received oxytocin for labor induction—the authors did not attempt to analyze blood pressure effects by anesthetic method or whether the patient was laboring. We excluded laboring patients from study in an attempt to simplify rather than complicate our retrospective analysis. In addition, the baseline blood pressures in the two studies are not strictly comparable. As discussed in our report, baseline blood pressures were the lowest recorded in the baseline period, whereas the baseline blood pressure reported by Wallace *et al.*[2] was the mean blood pressure during the baseline period. It is possible that severely preeclamptic patients who are in labor may respond differently to regional anesthesia than severely preeclamptic patients who are not in labor. This question awaits tightly controlled prospective analysis.

Finally, Dr. Malinow questions the potential confounding effects of magnesium therapy and the resulting blood pressure reductions that follow regional anesthesia induction. Sixty-five percent of our patients received magnesium therapy before induction of regional anesthesia. None of our patients were laboring, and, in 35% of cases, the obstetrician elected to delay instituting magnesium therapy until completion of the cesarean section. Whether blood pressure reductions after epidural or spinal anesthesia would remain similar if 100% of the patients received magnesium therapy before induction of regional anesthesia is unanswerable using our data. However, we reported that the proportion of patients receiving magnesium therapy were similar in both the spinal and the epidural anesthesia groups, and, whatever the effects of magnesium therapy, the proportion should have been similar in both groups. Wallace *et al.*[2] report that blood pressures were still similar during spinal and epidural anesthesia with 100% of the patients receiving magnesium therapy, which lends further support to this contention. We think that the results of our large retrospective study[1] and the smaller prospective study by Wallace *et al.*[2] support the contention that severely preeclamptic patients receiving spinal or epidural anesthesia for cesarean section will have similar blood pressure responses if magnesium therapy is also similar within the study groups.

In summary, I think that Dr. Malinow's concerns were adequately addressed in our report. Our study was a retrospective analysis, and clearly suffers the deficiencies of most retrospective studies. Currently, the anesthesia literature provides little guidance for assessing the relative risk and benefit of regional and general anesthesia for severely preeclamptic patients, and even less guidance for the relative merits of epidural *versus* spinal anesthesia. Further prospective studies are needed, and our retrospective study lends credence to the position that it is ethical to study spinal anesthesia, and that serious complications were absent in our series of patients. It is also ethical for the individual practitioner to use spinal anesthesia in selected cases. Definitive studies assessing the incidence of rare, life-threatening complications will probably not be performed. Our intent was to provide outcome data from a large clinical series in which potentially confounding factors were analyzed or discussed. We believe that our data analysis supports the contention that spinal anesthesia may be equally safe to epidural anesthesia for severely preeclamptic patients requiring cesarean section. Strict control of anesthetic method and analysis of variables such as labor, magnesium therapy, and intravenous fluids await prospective study.

David D. Hood, M.D.
Associate Professor of Anesthesiology
Wake Forest University School of Medicine
Medical Center Blvd
Winston-Salem, North Carolina 27012-9788
dhood@wfubmc.edu

References

1. Hood DD, Curry R: Spinal versus epidural anesthesia for cesarean section in severely preeclamptic patients: A retrospective survey. ANESTHESIOLOGY 1999; 90:1276-82

2. Wallace DH, Leveno KJ, Cunningham FG, Giesecke AH, Shearer VE, Sidawi JE: Randomized comparison of general and regional anesthesia for cesarean delivery in pregnancies complicated by severe preeclampsia. Obstet Gynecol 1995; 86:193-9

(Accepted for publication September 3, 1999.)