Anesthesiology
1998; 88:1688

In Reply:—Although I fully agree with the slogan that prevention is better than intervention (and that message is well embraced by the F_AO_2 parameter in the desaturation model in my article), the slogan for my conclusion "if V_E is thought to be 0, or near 0," then "a rescue option should be instituted aggressively and early," is that it is better to be safe than sorry. One must remember that my analysis probably underestimated the danger of severe hypoxemia by assuming complete alveolar denitrogenation (rarely achieved) and by ignoring the fact that concomitantly administered anesthetics may delay functional recovery (frequently true), result in loss of airway patency (often true), and adversely change physiologic variables (*e.g.*, decrease functional residual capacity and cardiac output). In addition, and very importantly, I presented mean data, which means that for half of the population the danger will be greater and for half of the population the danger will be less. In the final analysis every practitioner has to ask themselves, given the boundary conditions in my article ± 1 min, if they would be willing to simply wait for succinylcholine to wear off if there is no ventilation and no obvious likelihood there will be ventilation. A negative answer seems obvious. I would not want to try and justify injury to the patient on the basis of the myriad of arguments for increasing the margin of safety of 1 mg/kg succinylcholine brought forward by Dr. Bourke; the arguments are either not substantial (*e.g.*, minor discrepancies in succinylcholine dosing used in the literature), agreed on (decreasing the succinylcholine dosage shortens the duration of apnea), unrealistic (use 0.20–0.25 mg/kg of succinylcholine), or potentially dangerous (*some* patients might survive breathing spontaneously [diaphragmatic movement does not necessarily equate with respiration] with a single twitch height that is only 50% of control).

Jonathan L. Benumof, M.D.
Professor of Anesthesia
University of Calfornia, San Diego
San Diego, California

(Accepted for publication February 5, 1998.)

Anesthesiology
1998; 88:1688–9

Safety of Patient-controlled Intravenous Meperidine

To the Editor:—We would like to congratulate Sharma *et al.*[1] on the results of their study examining the relationship between cesarean section and epidural analgesia during labor in which they address the problem of "labor pain selection bias." This current study addressed many of the concerns that were raised with their previous study.[2]

We would like to express concern, however, regarding their conclusion that "we found meperidine PICA to be a safe and effective method of pain relief."[1] On what basis was this conclusion made? When the two groups were compared in terms of maternal satisfaction, pain scores in the first and second stages of labor, maternal desire for the same form of analgesia, and maternal sedation, the epidural group was significantly superior on all accounts. Perhaps they believed that there was a smaller crossover from the PCIA group to the epidural group, in contrast to their previous study; this suggested that the PCIA group had effective analgesia. Because the thrust of this study by caregivers and investigators was to keep patients in their original group, this may also have contributed to the sense that adequate analgesia was provided. The statistically significant difference between the two groups in respect to maternal satisfaction and desire for the same form of analgesia again cannot be ignored.

On the issue of safety, Chestnut in his accompanying editorial makes the following comment, "Maternal administration of high doses of opioids may result in substantial neonatal effects (*e.g.*, respiratory depression, prolonged neurobehavioral changes). These factors have been largely ignored in this debate."[3] Neonatal safety in this study was examined using Apgar scores, umbilical artery gases, naloxone requirements, and intensive care admission. There was a statistically significant difference between the groups in the use of naloxone. Differences for the other parameters of neonatal outcome failed to reach statistical significance, although there is no evidence presented that this study had the power to examine this question, and it would be wrong to draw firm conclusions. It would have been valuable if the authors had examined neonatal neurobehavioral changes in the two groups. There is a large body of evidence that meperidine used for labor analgesia causes significant neurobehavioral effects.[4–8] Many of these studies involved lower doses than those used in this study. The clinical significance of these more subtle changes in neonatal behavior are not clearly defined, but when considering neonatal safety, these effects should not be ignored.

In conclusion, we agree with the authors that "labor epidural analgesia in women at full term with uncomplicated pregnancies and in spontaneous active labor is not associated with increased numbers of cesarean delivery."[1] However, until there is further evidence as to the safety of PCIA meperidine, particularly on the neonate, we do not think it should be considered a safe and effective method of pain relief during labor.

Deborah Wilson
Clinical Fellow