surgical patients. Although the authors are to be congratulated for conducting such a difficult study, several points need both clarification and more extensive discussion before their conclusions can be widely adopted.

First, their technique of epidural anesthesia and analgesia is very poorly described. How much local anesthetic was administered? Why different agents? What was the extent of sensory analgesia? Was one or several doses administered to maintain sensory analgesia and, thus, more efficient nociceptive blockade during surgery? Which opiates were used for postoperative analgesia, and in what doses?

Second, the patients studied are poorly described. As pointed out in the editorial,[2] do the differences in morbidity between the two groups result from the epidural regimen or from other factors? What were the type and duration of the surgical procedures? (It is not sufficient to divide the material into thoracic, abdominal, and vascular procedures.) Why did the technique of general anesthesia vary in the control group? What was the diagnosis leading to surgery? What was the specific indication for ICU observation, which was apparently the reason for inclusion into the study? What was the cause of death in four patients?

Third, the discussion is insufficient for several reasons. The authors tend to confuse the subject by mixing results from studies using epidural local anesthetics and epidural opiates. This is an important point, as such a mixture is in stark contrast to the plentiful data demonstrating pronounced differences between epidural local anesthetics and opiates, and their physiological effects on postoperative endocrine-metabolic response, sympathetic blockade including changes in blood flow to the lower extremities, gastrointestinal motility, etc.[3,4] Furthermore, a more thorough discussion of published data is desirable. One obvious omission is a well-performed controlled study of 100 elderly patients following abdominal surgery, where the intraoperative use of epidural local anesthetics followed by 72 h treatment with epidural morphine 4 mg 12-hourly had *no* significant effect on a battery of carefully monitored postoperative morbidity parameters.[5]

Finally, the literature on postoperative morbidity in controlled studies comparing general anesthesia *versus* epidural local anesthetics (single dose or continuous) is quite extensive, and has been reviewed twice previously.[6,7]

In summary, although I completely agree with the basic concept of nociceptive blockade as a prerequisite to improved postoperative morbidity,[8] the demands for scientific documentation and clarification of this complex problem are enormous, and almost never fulfilled. Thus, the several "dark spots" in the article by Yeager *et al.* need to be clarified before any conclusion can be brought to bear on clinical practice.

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Epidural Anesthesia and Analgesia in High-risk Surgical Patients. II.

To the Editor:—There is a fatal flaw in the reasoning of Yeager *et al.*[1] which invalidates their conclusion that, in high-risk surgical patients, epidural/N_2O technique (group I) prevents the postoperative mortality seen following high dose narcotic/N_2O technique (group II).

They randomly assigned 53 patients into approximately equal numbers of group I and group II subjects. Postoperatively, there were no deaths in group I, while there were four deaths in group II. They concluded that "group I treatment exerted a significant beneficial effect on operative outcome." Furthermore, they were so convinced of the correctness of this conclusion that ". . . We felt it necessary to terminate the study . . . because the overall complication rate was strikingly higher in group II patients."

The flaw in their reasoning is their tacit acceptance of the logical fallacy known as the *post hoc* fallacy. (*Post hoc, ergo propter hoc,* or after this; therefore, because of this). In the *post hoc* fallacy, one concludes that, because a temporal relationship is present, a causal relationship exists as well. For example, if Event A precedes Event B, then clearly one cannot conclude therefore that Event A caused Event B. In the study of Yeager *et al.*, there is a statistically significant difference—four deaths in group II *versus* no deaths in group I. But this merely means that a statistically significant difference exists in mortality rates between the two groups—and nothing more. Differences which are statistically significant tell one nothing about the cause of that difference. Causality must be determined at its own level, on its own terms, by its own criteria.

The authors could have avoided the *post hoc* fallacy when interpreting these findings if they had reviewed the records of the four patients in group II who died, and tried to determine what the actual causes of death were. They could have consulted with the surgeons and the other physicians involved in the care of these patients. There are few anesthesia complications which lead to death 13–37 days postoperatively. It is more likely that the deaths were related to the high-risk status of the patients rather than to the anesthetic technique.

The take-home message of Yeager's study is "There were no deaths in group I and 5 in group II. Therefore, do not use group II technique." The logical approach presented here will likely have little impact following publication of the original article. More relevant is the old Russian proverb: "What is written down with a pen can not be chopped out with an axe."

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Epidural Anesthesia and Analgesia in High-risk Surgical Patients. III.

To the Editor:—Yeager *et al.* conclude that epidural anesthesia and postoperative analgesia, when compared with general anesthesia, exerted a significant beneficial effect on operative outcome in a group of high-risk surgical patients.[1] Unfortunately, confidence intervals for the differences between the two groups were not given, and highly significant differences cannot be assumed from this small study.

When comparing the efficacy of two treatments, estimation of the range of possible differences between the two provides more information than hypothesis testing. The confidence interval is a range of values that is likely to cover the true but unknown value. Both the *British Medical Journal* and the *Lancet* have encouraged the wider use of confidence intervals.[2,3] The use of confidence intervals and methods of calculation have been reviewed in both these journals.[4,5]

If confidence intervals are calculated for some of the data presented by Yeager *et al.*, the imprecision due to the limited sample size is shown. Four of 25 patients in the general anesthetic group died compared with none in the epidural group. While the best estimate of the difference in the percentage of patients dying is 16%, the 99% confidence interval (CI) ranges from −5% to +37%. The best estimate of the difference in the percentage of patients developing cardiovascular failure is 38%, the 99% CI ranges from 6–70%. The best estimate of the difference in the percentage of patients developing major infection is 33%, the 99% CI ranges from 3–63%. Finally, the difference in the mean cortisol excretion in the first 24 h is 36.6 μg/h, with 99% CI from −10.9 μg/h to +84.1 μg/h.

It is unfortunate that the authors felt it necessary to terminate their study after only 53 patients. Like Dr. McPeek, I am concerned that a difference was detected where, in fact, no difference exists.[6] I am unwilling to change my practice on the basis of such a small, imprecise study.