

## Effects of Anesthesia in Therapeutic Abortion

*To the Editor:*—Cullen, Margolis and Eger (ANESTHESIOLOGY 32: 108, 1970) have written of their prospective experience, relating blood loss during therapeutic abortion to the anesthetic procedure. I infer from their report that the study was neither randomized nor blind. Therefore, the differences in blood loss that these authors attribute to the several anesthetic agents may have resulted in part from observer bias, changes in procedure, or variation in measurement techniques followed throughout the period of study. Their results must be substantiated by a well-designed clinical trial before such data can be used as the basis for selection of anesthetic agents in abortion. Extrapolation from this study to other procedures on the gravid uterus is quite unwarranted.

Shnider's editorial in the same issue of ANESTHESIOLOGY (page 99) states: ". . . their carefully designed and controlled study conclusively demonstrates . . ." whereas the study makes no mention of the use of randomization, blind evaluation, or other standard precautions necessary to assure valid results. I feel that such editorial enthusiasm for a study that is only suggestive, and not definitive, is unwise. It blurs the standards of clinical science and further misleads the casual reader into an unwarranted sense of certainty, concerning the study itself.

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*To the Editor:*—The following information may answer some of Dr. Forrest's criticisms of the methodology in our study of blood loss during therapeutic abortion. The various anesthetics were not studied in successive "groups," as perhaps implied in the article. Initially, patients were given halothane, or nitrous oxide plus adjuvants, or fluroxene, in alternate fashion (i.e., every third patient received halothane). As the study progressed, however, we decided to investigate additional anesthetic techniques. In order to have comparable numbers of patients in all groups, we could no longer alternate techniques. We found no differences between our early results where tech-

niques were alternated and the results thereafter. There was no selection of patients with respect to anesthetic technique. Two patients were excluded from the study only because the operations were being performed for medical, rather than psychiatric, reasons.

Dr. Forrest suggests that observer bias possibly influenced our results. However, it is difficult to envision how bias might affect measurement of anesthetic or end-tidal CO<sub>2</sub> concentrations, or the measurement of blood in a graduate, or the rate of flow of oxytocin. As indicated in table 1, the patients in the various groups were comparable as to gestation, anesthesia time, and gravidity—measurements unlikely to be grossly influenced by observer bias. Suction time (i.e., operative time) was shorter with halothane, but other reports suggest this should have led to decreased blood loss rather than our converse finding.

The same surgeon (A. J. M.) participated in 80 per cent of the procedures. Initially, we attempted to keep him unaware of the anesthetic under study. However, his familiarity with anesthetic procedures precluded such "blindness." Surgical technique was uniform. Despite this, bias may have influenced the vigor of the surgical manipulation. If so, why didn't such a bias also cause the operator to prolong procedures done under halothane? As noted above and in our report, these operative times were shortest.

Finally, we agree with Dr. Forrest that our study might have been better constructed. However, our study is the only one that accurately relates obstetric blood loss to anesthetics and anesthetic dose at constant P<sub>ACO<sub>2</sub></sub> in a homogeneous patient population. In the absence of a study structured as suggested by Dr. Forrest, our data remain the best available. Should the practicing anethetist use anything less when deciding what anesthetic to choose for his patient?

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