Beneficial Effects of Halothane on Myocardial Ischemia

To the Editor—In their recent article, Dr. Verrier and his colleagues\(^1\) were kind enough to cite our earlier studies\(^2\) which demonstrated that halothane produced an increase in coronary vascular resistance. However, their own elegant studies have demonstrated that by using waterfall pressure rather than left ventricular end-diastolic pressure in the calculation of vascular resistance, halothane produces no change. Their studies may therefore reconcile conflicting data in previous investigations on this subject.\(^3-5\)

EZZAT ABOULEISH, M.D.
Professor of Anesthesiology
University of Pittsburgh
Director of Obstetric Anesthesia,
Magee-Womens Hospital

DALE MACMURDO, M.D.
Instructor of Anesthesiology
University of Pittsburgh

ALBERT L. CAFFO, PH.D.
Director, Nutrition and Trace Metals Laboratory
Children’s Hospital, Columbus, Ohio

A. HAROLD LUBIN, M.D.
Chairman, Pediatric Nutrition and Trace Metals Laboratory, College of Medicine
Ohio State University

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From our earlier study, we concluded that halothane jeopardized oxygen supply/demand relationships in the myocardium. Because this conclusion was at variance with our own clinical experience using halothane, we subsequently undertook an investigation which was published too recently to be cited in Dr. Verrier's manuscript. In a canine preparation with a one-stage ligation of the anterior descending artery, it was found that halothane 1 per cent inspired significantly improved supply/demand relationships in the ischemic in comparison with normal myocardium, despite producing a 42 per cent reduction in mean arterial pressure. Although the mechanism of this effect was not open to investigation, suspicion rested heavily upon the 15 per cent reduction in heart rate which occurred.

Thus, there appears to be common agreement that with a variety of different animal experimental models of myocardial ischemia, halothane has a beneficial effect, probably secondary to changes in systemic and myocardial hemodynamics and not by any specific mechanism. These data confirm our teaching that the clinical management of patients is based on maintaining oxygen supply to the myocardium while suppressing those factors which lead to an increase in oxygen demand. However, one would agree with the sentiments expressed by Dr. Merin that unfortunately these studies do not provide additional clinical guidance in managing patients with ischemic heart disease on the tolerance limits of change in cardiac filling pressures, systemic arterial pressure, heart rate, or any other hemodynamic factor.

G. Smith, M.D.
Professor of Anaesthesia
University of Leicester
Leicester, England

REFERENCES

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Dextran Is Not a Potent Local Anesthetic Adjuvant

To the Editor:—I read with interest the letter by R. E. Loder1 on the use of dextran as a local anesthetic adjuvant. The scientific literature does not support his contentions, however.

Based upon research conducted at the University of Washington, the Mason Clinic, and my own laboratory at the University of California, Davis, the following statements regarding dextran and regional anesthesia are more realistic:

1) Dextran does not form a macromolecular complex with local anesthetics.2,3

2) Dextran’s mechanism of action as a local anesthetic adjuvant appears to be a function of its pH. The more alkaline the dextran, the greater the prolongation of block.2

3) Pharmaceutical dextrans can vary markedly in pH. For example, Rheomacrodex® (Pharmacia), the most widely used dextran in the United States, has a pH of 4.5 to 5.5 in contrast to Dextraven® (Fisons) which has a neutral pH. This difference in pH explains some of the contradictory dextran studies that have appeared in the anesthesia literature.

4) The higher molecular weight dextrans are not more potent local anesthetic adjuvants. In a recent study, dextran 40 proved superior to dextran 75, 110, and 150 at prolonging bupivacaine’s duration of block in an experimental model. The result we attributed to the pH of the dextrans used in the study and not to their respective molecular weights.4

5) Finally, dextran is a rather mediocre and impractical local anesthetic adjuvant for clinical use. The maximum prolongation of block that we have been able to elicit experimentally is approximately 50 per cent using a dextran selected specifically on the basis of its alkalinity.

It is evident that the need for dextran as a local anesthesia adjuvant has diminished since Loder first advocated its use in 1960.5 The duration of block produced by the