devices and his proper place in the history of mechanical ventilation. With this aspect, there is no controversy. The problem of terminology remains, however. Some believe Dr. Barach should have called his technique "*constant* posisive-pressure breathing," since its objective was to maintain a constant mask pressure. They also believe that "*continuous* positive-pressure breathing" is the proper description for conditions during which mask pressure is always positive but not constant. A happy solution would be to retain CPPB to describe Dr. Barach's technique and his priority. The new term suggested to describe the end-expiratory positive-pressure technique now in vogue is "PEEP" (positive end-expiratory pressure). PEEP has the merits of accurate description and a pleasantly musical acronym.

More on Minipigs and Metabolism

To the Editor:—In a recent editorial,[1] Dr. B. L. Brown discussed at some length the implications of results presented in the same issue by Sawyer *et al.*[2] We find ourselves unable to agree with some of his interpretations.

In miniature swine, Sawyer did not show loss of halothane across the liver at high halothane concentrations, whereas at lower levels a considerable fraction of halothane was lost.

Dr. Brown interpreted these results as showing that "halothane acutely inhibits its own metabolism." Sawyer *et al.* did not demonstrate any such inhibition; indeed, they clearly noted that they were unable to discriminate between the alternatives of enzyme saturation and enzyme inhibition at high halothane concentrations. Failure to demonstrate loss of halothane at high halothane concentrations is probably due to the difficulties associated with distinguishing relatively small differences between two large values, even though the loss in absolute terms may well be similar to that which is readily estimated at lower halothane concentrations.

Dr. Brown noted that "if any criticism is to be made, it is that the hepatic halothane concentration differences observed were only assumed to be the results of metabolic extraction." This criticism is totally justified, as differentiation between hepatic storage and metabolic breakdown is crucial in the interpretation of this type of data. Sawyer and his co-workers assumed that the fraction of halothane removed was that which was metabolized. In the absence of any actual metabolic data this assumption is unwarranted. The statement made by Dr. Brown that halothane acutely inhibits its own metabolism is not supported by Sawyer's data or comments.

Comparative data on the metabolism and distribution of volatile anesthetics in miniature swine and humans are not available. In this instance it is hazardous, to say the least, to translate inconclusive data obtained in swine to man.

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References

1. Brown BR: Minipigs, microsomes, metabolism, and Maupassant. Anesthesiology 34:217–218, 1971
2. Sawyer DC, Eger EI II, Bahlman SH, *et al.*: Concentration dependence of hepatic halothane metabolism. Anesthesiology 34:230–235, 1971

To the Editor:—Drs. Foulkes, Topham and Winrow have erred in suggesting that the results obtained by Sawyer *et al.* might be ex-

plained by hepatic storage (uptake) rather than by metabolism of halothane. They apparently failed to understand that: "Equilibrium between blood and liver was approached from high to low concentrations to eliminate the effect of uptake" (ANESTHESIOLOGY 34: 233). We agree that at the higher concentrations there may have been uptake into the liver, which would give an overestimate of metabolism. Thus, if uptake (or storage) were to manifest itself as a concentration difference across the liver, it should have done so at the higher concentrations. As we reported, the fractional extraction of halothane was lowest at these concentrations. Moreover, as we decreased the partial pressure of halothane in the inflowing hepatic blood, we should expect to see a reversal of the halothane partial pressure difference between arterial blood and liver. Halothane should then move from liver to blood, thus causing the venous concentration of halothane to exceed that in arterial blood. Since we found the opposite, we conclude that "storage" cannot explain our data and, if anything, would only strengthen the conclusion we reached: the fractional metabolism of halothane increases as the partial pressure of halothane in the liver decreases. Our only error may be that the fractional differences we measured across the liver underestimate rather than overestimate the total halothane metabolism.

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Formulation of *d*-Tubocurarine

To the Editor:—Dowdy and her colleagues [1] have shown clearly that the antibacterial preservatives in the commercial preparations of *d*-tubocurarine have a marked negative inotropic action, and that *d*-tubocurarine by itself has a slight positive inotropic action. Because of the confusion that exists in the naming of solutions of *d*-tubocurarine in different countries, we should like to point out that the solution "Tubarine (miscible)," which is widely used in Great Britain, contains no antibacterial or antioxidant preservatives. In our experience, administration of *d*-tubocurarine, 30 mg, has been followed by highly significant decreases in blood pressure in both healthy and poor-risk adults.[2, 3]

Dowdy *et al.* do not say that an action on the heart of *d*-tubocurarine or preservatives is responsible for the clinical hypotension, but we think it worthwhile to point out that the marked hypotension which does occur with the dosage used in Great Britain must be due to histamine liberation, or, more probably, to ganglionic blockade, and has no connection with preservatives. This hypotension has not been observed in comparable studies with pancuronium.[2, 3]

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REFERENCES

1. Dowdy EG, Holland WC, Yamanaka I, *et al.*: Cardioactive properties of *d*-tubocurarine with and without preservatives. ANESTHESIOLOGY 34:256, 1971
2. McDowell SA, Clarke RSJ: A clinical comparison of pancuronium with *d*-tubocurarine. Anesthesia 24:581, 1969
3. Lyons SM, Clarke RSJ: Unpublished observations