

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 positive-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.

D. M. FOULKES, PH.D.
J. C. TOPHAM, B.Sc., PH.D.
M. J. WINROW, B.Sc., PH.D.
*Biochemical Pharmacology Section
Safety of Medicines Department
I.C.I. Pharmaceuticals Ltd.
Alderley Park, Macclesfield
Cheshire, England*

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-