30. Brody, T. M., Calvert, D. N., and Schneider, A. F.: J. Pharmacol. Exp. Ther. 131: 341, 1961.

To the Editor.—I was very interested in your editorial comments in ANESTHESIOLOGY, concerning halothane and liver damage.

The interest to me lies not so much in the possibility of liver damage being caused by halothane, but why these cases should be occurring in the U.S.A. and not in Britain.* No British anesthetists have reported any cases, and on a personal note, I have used halothane since 1956 with no abnormal findings in the liver postoperatively. (I was one of Dr. M. W. Johnston's Juniors, when he carried out his clinical trials.)

Which brings me back to why these cases occur in U.S.A. and not Britain. May I put forward a possible line of thought—and I would add that I have no concrete evidence to support my view—merely a theory. Could there be a build-up of CO_2 during anesthesia? —and could this, in association with halothane, cause the liver damage?

I have spoken to Dr. Raventós—he assures me that he has been unable to produce liver damage in his animals, no matter how high the CO_2 level—but would this of necessity hold in human beings?

I was able to observe methods in the U.S.A. at close quarters. I was privileged to be a Special Fellow in Anesthesiology at the Cleveland Clinic, Cleveland, Ohio, from February 1960–February 1961. One marked difference in technique which I noticed, was that we use much higher flows than our U.S.A. colleagues —*e.g.*, on semi-closed circuits we use 6 litres of O_2, 2 litres of N_2O or 5:3—usually a total flow of 8–9 litres (for adults—less for children.) This high flow is sufficient to wash out CO_2, even without an absorber. In U.S.A., people seemed to use a much lower flow—2:2 —but of course with an absorber. Suppose, however, the absorber was not acting efficiently, *e.g.*, soda lime stale—there would be a CO_2 build-up. It would be interesting to find out whether there was any CO_2 build-up in the cases reported.

I'm not for a moment implying any wrong or bad technique from the anesthesiologists concerned, but it seems so strange for cases to occur in U.S.A. and not here, yet we use the same halothane. So a feasibility must be technique, and you must agree that with a high flow and no absorber there is less chance of CO_2 build-up than with a low flow and inefficient absorber. A possible explanation of why cases on one side of the Atlantic only??

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* Since my original letter, reports have appeared in the *British Medical Journal.*

To the Editor.—I have been most interested in the article by G. L. Brody and R. B. Sweet and your own editorial comment on the subject of necrosis of the liver following anesthesia in which halothane is administered.

It occurs to me to inquire whether in the cases reviewed, halothane concentration was determined by monitoring the mixture actually delivered to the patient or whether it is merely inferred from the settings of the vaporizers used. In the latter case certain techniques can of course result in a substantially different concentration reaching the patient from that which is actually set on the vaporizer. I would not suggest, of course, that the concentration would be so grossly inaccurate as to cause obvious disparities at the time of the operation, but in matters like the present, where one is seeking the cause of an observed serious effect the academic details can be important.

My interest in this matter arises from the fact that my Company specializes in the development and manufacture of the Vaporizer for volatile anaesthetic agents. We are always striving to provide the practitioner with the most accurate control possible so that any indication that particular conditions require an especially sensitive control of the agent are of prime importance in planning and developing vaporizers of tomorrow.

J. A. JEPHCOTT
Longworth Scientific Instrument Co. Ltd. England