CORRESPONDENCE

CARDIAC ARREST AND RESUSCITATION

Sir,—In his review of the second edition of H. E. Stephenson's Cardiac Arrest and Resuscitation (Brit. j. Anaesth., 37, 73) Dr. Alick Hobbes saw fit to single out for adverse criticism a paper by Dr. Bullough and myself which was given some prominence in Dr. Stephenson's book. Apart from mis-spelling both our names, your reviewer calls it "The worst example . . . of a method of infant cardiorespiratory resuscitation . . . inadequate for pulmonary ventilation and useless for providing an effective circulation."

May I remind readers that:

(1) Our eight patients with apparent circulatory arrest ranged in age from 8 months to 9 years. (I later described an adult case, aged 35 years.)

(2) All the patients survived without adverse effect including the adult: suggesting that the method cannot be so inefficient, if properly carried out.

(3) Our paper preceded that by Kouwenhoven by three years and was, after all, the first paper describing a practical external method of dealing with cardiac arrest involving compression of the chest.

(4) Its main fault, perhaps, was that it came from the wrong side of the Atlantic. Hence it met only scepticism (from worse) and a total disinterestedness on the part of British anaesthetists. I wonder if elsewhere we should have found it as difficult to get a few animals on whom to confirm our theories.

ERNEST RAINER
Chislehurst

A copy of the above letter was forwarded to Dr. Hobbes, who replied as follows:

Sir,—I must apologize for the mis-spelling of the names of Mr. Rainer and Dr. Bullough in my review of the second edition of Cardiac Arrest and Resuscitation.

Mr. Rainer has misread my statement about his method of resuscitation, as the words "the worst example" clearly refer to the previous sentence concerning the lack of critical evaluation in the book being reviewed.

Mr. Rainer apparently agrees with me that his method of resuscitation is of historical interest, being an early description of a method of resuscitation that could be performed without thoracotomy.

Analysis of the illustration in their original article ("Respiratory and cardiac arrest during anaesthesia in children", E. H. Rainer and J. Bullough (1957), Brit. med. J., 2, 1024), shows that the pulmonary ventilation is inadequate (unless the lungs are ventilated by a second operator). And although lifting the legs will increase the central blood volume, how can the action of pressing the legs on to the chest 12 to 15 times a minute "squeeze blood out of the chest into the periphery" and so produce an effective circulation when the heart has, in fact, ceased to function?

An investigation of their method by Mr. Rainer and Dr. Bullough is required in order to allay the scepticism existing in the minds not only of British anaesthetists, but also of members in other specialties and in other countries, who are directly concerned with cardiorespiratory resuscitation.

ALICK HOBSES
Liverpool

EXTERNAL CARDIAC MASSAGE

Sir,—Following a correspondence with Professor T. Cecil Gray, a former Editor of the British Journal of Anaesthesia, and at his suggestion, we have thought it advisable to draw attention to the fact that our paper on "External cardiac massage in young children", published in the British Journal of Anaesthesia in July 1961, was submitted to this journal in April 1960, i.e. some months prior to the publication of Kouwenhoven's work.

Our work, which covered quite a period of time, as cardiac massage is so infrequently called for, was not based on anything read or heard, but was the result of inductive and deductive reasoning, together, no doubt, with some element of serendipity.

W. BRYCE MCKELVIE
Isle of Man

P. MCKELVIE
London

[The original paper, as submitted, was rejected after very careful consideration by the Journal's advisers on the ground that there was insufficient evidence to support the advocacy of a practice which might lead to delay in the institution of well established and effective methods of resuscitation. Following the publication of the paper by Kouwenhoven and his colleagues in the Journal of the American Medical Association of July 9, 1960, with its evidence from many dog experiments and reports of successful resuscitation in humans as early as July 1959, re-submission was invited.—Eds.]

HALOTHANE AND THE LIVER

Sir,—May one make a plea for reason in the halothane controversy? Let it be granted that the causal correlation between halothane and the reported liver-failures following its exhibition is "not proven". Let it be granted further that even if this relationship could be proven, the demonstrated incidence of significant liver damage in normal hospital population is low enough to be offset by the drug's many patent advantages. But then let us also state that the carping on the definition of hepatotoxicity is quite beside the point: the anaesthetist cares little whether it is the direct action of halothane on the liver cell or its effect on the circulation that brings about changes in liver function. If one drug produces such changes more often than another, the first drug is rightly regarded as more hepatotoxic than the second, regardless of the actual pathomechanism. Last year we have offered proof, based on the prospective study of 250 surgical patients (Audet et al., 1964), that halogenated anaesthetics (halothane and methoxyflurane) were associated with significantly greater bromsulphthalein (BSP) retention during surgery than was an intravenous anaesthetic (neuroleptanalgesic mixture; P=0.003). We could also show on...