

A CRITIQUE OF INTRAVENOUS ANESTHESIA IN WAR SURGERY

F. J. HALFORD, M.D., F.A.C.S.

Honolulu, T. H.

EVERY advance in anesthesia has been marked by its tragedies. So far as I know, a critical study of intravenous anesthesia in traumatic shock in human beings has not been published. The distribution of 1,800,000 ampules of such an anesthetic to a certain country is no more evidence of its virtues or its weaknesses than the number of words in a novel is a criterion of its literary value.

It would appear that not only are there dangers intrinsic in any anesthetic, but there are dangers inherent in its administration and in the immediate condition of the patient.

The latter point is probably the most important in all the well recognized anesthetics. A patient with interval attacks of gallbladder disease is not a comparable risk to an individual shocked psychically, wrecked physically, and drained of his vital fluids. All reports to date deal with the usage of the drug in routine civilian practice—none with the injuries of war.

While intravenous anesthesia would seem to be ideal for war injuries because of its compactness, ease of preparation and nonexplosive characteristics, it should be clearly recognized that under war conditions anesthetics cannot be given by highly skilled anesthesiologists, but have to be given by doctors, nurses and orderlies, to whom the art is strange and the toxicology and pharmacologic actions are unknown. With this heterogeneous mass of emergency anesthetists, it is necessary to choose an anesthetic involving the widest margin of safety for the patient.

One of the conditions under which these anesthetics must be given in time of war is the existence of the depletion of the patient's blood volume by hemorrhage or plasma loss, and the resultant inability of the patient to satisfy his oxygen requirements. This, however, is true of all anesthetics. This embarrassment might be further increased by the use of a drug whose volume requirements preempted the use of oxygen, as nitrous oxide, or by one which depressed respiration in an unalterable manner, as the barbiturates.

The basis of this criticism is drawn from the number of patients who succumbed during and immediately following the use of evipal and pentothal sodium anesthesia on the fateful seventh of December, 1941. I was fortunate enough to have been called as a civilian surgeon to a

military hospital in Honolulu on December 7. A number of patients were given evipal by competent anesthetists only to have respiratory failures, some of which ended in death.

After several such fatalities, pentothal sodium was used, and again respiratory failures occurred, and, as in the case of evipal, death ensued in enough cases to cause us to abandon it as too dangerous.

In several cases when as small an amount as 0.5 Gm. of pentothal sodium solution had been administered, there suddenly appeared a "cyanosis decolletage" which was the inevitable and irremedial predecessor of death.

The injuries which we were attending were all severe; the casualties received had not only been blasted by demolition bombs but machine-gunned. This list of wounds consisted mainly of traumatic amputations of one or more extremities often in the same patient, compound comminuted fractures, penetrating chest and abdominal wounds, head and jaw injuries. There were several cases in which both buttocks had been blasted away. Burns were present in only an extremely few of the wounded, although at Pearl Harbor 254 burn cases were treated.

Nor should it be forgotten that these wounded were healthy young adult males with (1) adequate lung capacities and excellent circulatory systems, plus (2) unbelievable morale despite the fact that all victims were totally unprepared in mind or spirit for such a treacherous assault.

That we were attending patients in severe shock there can be no denial. These patients were prepared with perhaps a minimum of plasma and whole blood transfusions, which substances are the keystones of the treatment of traumatic or surgical shock.

There was a definite lack of oxygen and equipment for administering continuous oxygen therapy. Likewise, there was confusion concerning the individual oxygen demands in that many of those chosen to be given the intravenous anesthetic seemed to have no oxygen poverty, and in truth were excellent risks according to usual surgical observations. These same patients were the ones who promptly had such alarming symptoms of respiratory depression that the anesthetic was stopped and artificial respiration was started—often to no avail. So consistent was the rapid onset of respiratory symptoms even at the beginning of the anesthesia that a "cause" must be found for such reactions.

It is my belief that this "cause" is contained in an unknown factor relating to anoxia. Furthermore, the type of anoxia found in traumatic shock in the human is entirely different from that sustained or observed in the experimental animal (usually under a barbiturate). No one has as yet defined the oxygen requirements in shock, nor has a method been found for a rapid and practical determination of these wants.

As Admiral Gordon-Taylor of the British Navy has so aptly said, "Spinal anesthesia is the ideal form of euthanasia in war surgery"—then let it be said that intravenous anesthesia is also an ideal method of euthanasia.

It was the consensus of all civilian surgeons concerned that, considering all the hazards of patient, anesthetist and anesthetic, open drop ether still retains the primacy!

The Army is in need of anesthetists. Particular interest at the moment is for trained men to be assigned to auxiliary surgical teams. When any qualified man is ready to apply for a commission he should write to Lt. Col. B. N. Carter (M. C.) U. S. A., 1818 H Street, Washington, D. C.