CORRESPONDENCE

TERMINOLOGY FOR DERIVED HAEMODYNAMIC CALCULATIONS

Sir,—I read with much interest the paper by Guggiari and colleagues (1985) "Use of nitroglycerine to produce controlled decrease in mean arterial pressure to less than 50 mm Hg". While commending them for their work, I would like to point out a misinterpretation of the terminology used for haemodynamic measurements in their paper.

Stroke volume (SV), as calculated by the authors, is in fact stroke volume index (SI). Similarly, systemic vascular resistance (SVR) is systemic vascular resistance index (SVRI). Since the calculations with cardiac output (which is really the systemic vascular resistance) do not correct for the variations in body size, cardiac index is substituted. As a result, systemic vascular resistance should be called systemic vascular resistance index (SVRI). In the same way pulmonary vascular resistance (PVR) as published should be called pulmonary vascular resistance index (PVRRI). Also, in their formula for SVR (which is SVRI), PAP (pulmonary artery pressure) should be mean PAP (PAP).

This confusion of terminology for derived cardiac calculations is really quite common in the published textbooks and literature. I hope this letter will bring it to the attention of future authors.

M. S. Dhamee
Milwaukee

REFERENCE


Sir,—I agree with Dr Dhamee concerning his remarks about the terminology used for the haemodynamic measurements. Nevertheless, the changes in the various indices are unaffected by this terminology and, thus, do not modify the interpretation of our findings.

M. Guggiari
Paris

REFERENCES


Sir,—In any clinical situation the benefits of a particular drug or technique must be balanced against the drawbacks. Our communication described the use of atracurium to facilitate tracheal intubation in circumstances where there is no ideal agent. Day-stay patients often require tracheal intubation, in order that the surgery may be performed safely, although surgery is often brief. This lies at the heart of the problem. A larger dose of relaxant improves intubating conditions, a benefit, but increases the duration of block, a drawback if surgery is brief (as difficulty in reversal may occur). A compromise has to be made and although I would agree that this compromise is not "ideal", it is, at least, fairly good. Dr Sosis argues that the use of suxamethonium in these circumstances, provides a better benefit:drawback profile. Although, unlike our report, he has not subjected this point of view to scientific analysis, he does cite work in support of his contention. I would not disagree that better intubating conditions may be achieved more rapidly by administering suxamethonium—a benefit. The drawback is a high incidence of muscle pains—a side effect not to be lightly dismissed (as anyone who has suffered from suxamethonium pains will attest to). Day-stay patients also tend to be just those in whom this unwanted effect is most pronounced—young, fit adults who are ambulant soon after its administration. Dr Sosis states "We have found that a small dose of non-depolarizing muscle relaxant minimizes the postoperative myalgia seen in young ambulatory patients who were intubated using suxamethonium". This anecdote is not, of course, an original observation. I have gone through my file of papers by researchers who have attempted to test this observation in a methodical and scientific manner. It is a very big file. Rather more than half the communications suggest that this method is, at best, unreliable.

Finally, your correspondent worries about the risks of gastric aspiration in outpatients. I believe it is quite insupportable to suggest that properly prepared outpatients, with no other risk factor such as hiatus hernia etc., be subjected to a "crash induction" technique—the natural conclusion to be drawn from Dr Sosis' comments.

R. M. Jones
London

RESPONSE TO INTRAOPERATIVE CONVERSATION

Sir,—We read with much interest the recent article by Bennett, Davis and Giannini (1985), in which non-verbal responses to intraoperative suggestions were reported. The authors' findings concur with the results of an accumulating amount of research in this area. However, in regard to Bennett and colleagues' findings, we would like to point to one of the problems that may arise in this kind of research, which makes the interpretation of data difficult if not unreliable.

In the reported study ear-pull suggestions were played to the patients..."approximately 5 min before beginning the reversal of anaesthesia..." The message lasted 3 min, so it seems reasonable to assume that the suggestion-patients were no longer in the deepest levels of anaesthesia, immediately before the reversal of anaesthesia. Anaesthetists generally attempt to allow the patient to lighten gradually as the surgical procedure comes to an end. Consequently, the depth of anaesthesia during this period is not comparable to the average depth of anaesthesia during the procedure. In addition, the possibility of tolerance to the anaesthetics used, with subsequent awareness, is ever present (Stevens, 1984).

For these reasons we hesitate to ascribe the authors' findings to intraoperative hearing, but instead would attribute their results to suggestion with subsequent amnesia, similar to the effect of post-hypnotic suggestions. Studies in which the effects of sounds during general anaesthesia are studied, must be conducted in such a way that sounds are only administered after anaesthesia has reached a sufficiently deep level (e.g. after the first incision as Bennett and colleague did, and not from intubation onwards) and must be stopped before any awakening from anaesthesia may be expected (e.g. at the beginning of the closing of the wound; that is at least some 20 min before the end of anaesthesia). Then, if postoperative signs of intra-operative hearing are found, there will be much less chance for normal auditory perception (and suggestion) with amnesia to have occurred.

The importance of a careful scrutiny of the depth of anaesthesia during the administration of sounds is further illustrated by our recent findings in volunteers, that prolonged inhalation of nitrous oxide may result in tolerance to the anaesthetic effects of the gas. In each of eight healthy, male volunteers, the minimal effective concentration of nitrous oxide was established, based upon continued absence of motor responses to repeated verbal commands. One week later this concentration of nitrous oxide was administered for a 3 h period; return of motor responses after at least 30 min of absence of such responses was considered a sign of tolerance to the anaesthetic effect of the gas. Four volunteers showed return of motor response within the 3 h of exposure, but two of these had been rather restless throughout the session. They may not have been deeply anaesthetized after all. The other two appeared calm, had continuously received 70 and 80% of nitrous oxide in oxygen, were clinically unconscious and nevertheless showed the demanded responses at some 140-150 min from the start of the experiment.

B. Bonke
J. Rupprecht
Rotterdam

REFERENCES


Sir,—Drs Bonke and Rupprecht (1985) raise two salient points with their letter concerning our recent paper (Bennett, Davis and Giannini, 1985). First, they correctly state that anaesthesia may have been lighter than is necessary for adequate surgical anaesthesia (e.g. incisonal stimulation) during presentations of the instructions to patients of the importance of touching their