Intraoperative awareness and the isolated forearm technique

Sir,—The correspondence columns of The Times newspaper (April 8, 18, 1995) is not the forum to air the obviously diverging views on the use of the isolated forearm technique (IFT) to detect intraoperative awakening and the significance of this level of consciousness to anaesthetic practice.

We were astounded to read that the IFT “has been shown to be ineffective” [1] and would be very interested to learn on what grounds these authors base such a statement. We maintain that the IFT is the only currently available, practical method of determining levels of consciousness during general anaesthesia involving neuromuscular blockers and we are not alone in that view. Jessop and Jones have described the IFT as the “nearest we have to a gold standard” against which all other methods of awareness detection must be compared [2].

There are abundant references in the anaesthetic literature attesting to the fallibility of clinical signs in paralysed patients, but we are aware of only two published studies which have raised doubts on the validity of the IFT [3, 4]. The study by Breckenridge and Aitkenhead [3] was published only as a abstract, was severely criticized in subsequent correspondence [5, 6] and was never published in full. The findings [3] indicated that for some 20 min before surgery started, patients made no response to commands with the IFT but subsequently “surgery proved impossible because of purposeful movements of the isolated arm”. The authors, ignoring these “purposeful movements”, deflated the tourniquet to enable surgery to continue. No further information was provided. The logical interpretation of these actions [6], which were not refuted, is that surgery was allowed to continue despite the “purposeful movements of the isolated arm” because the clinical signs indicated adequate anaesthesia. This interpretation is corroborated in a later review of awareness during anaesthesia by these same authors who stated that “there is a poor correlation between the clinical signs of light anaesthesia and response to command using the isolated forearm technique. The unparalysed arm may respond purposefully to surgical stimulation” [7]. We agree entirely with this statement, but unlike Breckenridge and Aitkenhead [3, 7], we believe an arm interfering with surgery or moving in response to command is an indicator of light anaesthesia, irrespective of other clinical signs.

The study by Miller and Watkinson [4] used the IFT in two groups of patients during anaesthesia supplemented with halothane (mean concentration between 0.5% and 0.6%): patients in the control group heard radio static and the experimental group heard commands and words. Patients in both groups were noted to move their isolated arms, but how it was decided that a hand movement in the radio static group had occurred, no attempt was made to verify, by direct communication, whether or not the patients were responsive and, as a consequence, the authors pointed out that “classification of a movement as intentional or otherwise verged upon the arbitrary”. Furthermore, a recently completed study involving two groups of patients during anaesthesia by these same authors who stated that “there is a poor correlation between the clinical signs of light anaesthesia and response to command using the isolated forearm technique. The unparalysed arm may respond purposefully to surgical stimulation” [7]. We agree entirely with this statement, but unlike Breckenridge and Aitkenhead [3, 7], we believe an arm interfering with surgery or moving in response to command is an indicator of light anaesthesia, irrespective of other clinical signs.

From the above brief discussion it is apparent that there is no published evidence to support the statement that the IFT “has been shown to be ineffective”. It is also worth stating that by judicious use of blockers and inflating and deflating the cuff appropriately there is no time limit on the IFT; 4 h is the longest case to date (transsection of oesophageal varices).

When the greatest fear for over half the patients (51.8%) admitted for elective surgery under general anaesthesia is “wakening up during the operation” [9] it is surely obvious that one of the prime responsibilities of our profession is to ensure the insensitivity of patients during such surgical operations. When using the IFT with some anaesthetic techniques it can be demonstrated that more than 50% of patients are awake [10–12] and many indicate that they are in pain [11]. We are deeply disturbed by the idea that some find it acceptable to operate on these patients simply because they (the patients) usually have no postoperative recall for this period of consciousness with pain [13, 14].

In general, we do not know the postoperative effects of performing surgery on patients, under so called general anaesthesia, who are capable of responding (whether in pain at the time or not) but without subsequent recall but there are case reports of adverse psychological effects on individual patients [15–17]. Until there is evidence to the contrary, the fact that patients do not have explicit postoperative recall for periods of intraoperative wakefulness or pain, or both, should not be used to condone anaesthetic techniques where there is a high risk of intraoperative responsiveness. Neonates in receipt of good benzodiazepine premedication and patients with Korsakoff’s psychosis do not remember surgery, yet few would advocate paralysis and ventilation with air–oxygen only for such patients.

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Sir.—The correspondence section of The Times newspaper is not a conventional forum to discuss issues pertaining to awareness during anaesthesia. However, our letter [1] was in response to an article in The Times which reported Russell and Wang's work [2] and, therefore, the letter was an appropriate forum for us to both question the suggestion made in the article that “half of the patients on every operating list show evidence of wakefulness,” and to answer Dr Wang's reported call for wider use of the isolated forearm technique (IFT) to detect awareness. Our original letter to The Times, in which we gave a detailed reply, had to be substantially reduced in length (with our permission) to allow publication. We are grateful for the opportunity to discuss some of the issues in more detail.

We understand, from personal communication with Drs Russell and Wang, that the original article in The Times reported research findings obtained using the IFT during nitrous oxide and opioid anaesthesia. This fact should have been made very clear in the original article, as it represents a very specific anaesthetic scenario, and certainly not “half the patients on every operating list.” We are well aware that the lay media may accidentally misreport details of scientific work. If this was indeed the case, it was the duty of the researchers concerned to put the record straight and to allay public fears in a public forum. We felt that, in the absence of such clarification from Russell and Wang, representatives of the anaesthetic research community should have said something. With regard to the increasing use of the IFT, it is not appropriate to publically advocate the widespread use of any clinical technique as a definitive solution to a medical condition without also describing the potential drawbacks and contraindications affecting its use.

The IFT was first described by Tunstall in 1977 [3]. During the subsequent 18 yr, the IFT has not become part of the routine practice of anaesthesia, and has been confined to research studies. Several workers have reported problems with detecting awareness with the IFT. One of Dr Russell's own studies [4] described a patient who felt surgical stimulation and heard commands to move the isolated arm, but was unable to do so, even though electrical nerve stimulation suggested that the arm was not paralysed. Two explanations were proposed by Dr Russell. The first was the block from ulnar nerve stimulation, and the second was due to the lack of confidence in the ability of the patient to understand and respond to commands. Therefore, the IFT in this case was not only revealing the ability to respond by unmasking the effects of competitive neuromuscular block, but was also reflecting impairment of cognition. Simple movements in response to the name of a patient would not constitute a taxing test of cognitive function [13] but a response to complex commands (e.g. squeeze my fingers three times) with a correct answer, would serve as both a test of cognition and a control for false positive responses. Russell and Wang acknowledged this distinction in data on a slide presented as part of a recent communication [14] where, with halothane anaesthesia, two patients exhibited IFT movement for simple commands but not for more complex commands.

The solution to routine prevention of awareness lies in real-time quantification of the depth of analgesia, which is not accomplished by use of the IFT. We stand by our statements in The Times [1], that is “The use of a tourniquet to protect a forearm from the paralysing drugs in the rest of the body so that the patient may signal awareness has been shown to be ineffective. Subjective monitoring described by Wang and Russell provides neither a diagnosis nor a solution. The suggestion that half of patients may be awake during surgery is not only frightening, it is wrong.” Surely, Russell and Wang do not believe the implication of their findings, that half of their patients are awake during anaesthesia.
Comparison between two methods of measuring cardiac output: misleading analyses?

Sir,—We are concerned about the correlation coefficient and regression analysis reported by Klotz and co-workers [1]. These workers used the Bland and Altman technique to calculate bias and confidence intervals, but failed to use these when constructing graphs and comparing the results with other work. In addition, at the bottom of the page 9 of their article acknowledge the appropriateness of the Bland and Altman technique, who could have been a victim of such an accident, suffered only some minor damage on his face and hands”.

On January 30, 1968, a tremendous explosion occurred with a lethal outcome during lung resection in the San Juan de Dios Hospital in Madrid, among them Sir,—We are concerned about the correlation coefficient and regression analysis reported by Klotz and co-workers [1]. These workers used the Bland and Altman technique to calculate bias and confidence intervals, but failed to use these when constructing graphs and comparing the results with other work. In addition, at the bottom of the page 9 of their article acknowledge the appropriateness of the Bland and Altman technique, who could have been a victim of such an accident, suffered only some minor damage on his face and hands”.

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2. Bland JM, Altman DG. The graph labelled figure 1 in the article of Klotz and colleagues is a simple plot of the measurements from the Doppler (DOP) against those from the thermodilution (TD) method. From the clustered points about the regression line, one would (falsely) assume that there is a strong agreement between the two methods. The correlation coefficient measures the strength of a relation between two variables and not the agreement between them [2]. A correlation coefficient of 0.84 shows that the two methods of measuring cardiac output are related, which is expected as both methods were specifically designed to measure this, but this value conceals a lack of agreement [2].

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**Increase in Mallampati score during pregnancy**

Sir,—Pilkington and colleagues are to be commended for their work which helps to elucidate the reasons for difficulty in tracheal intubation in obstetric practice [1]. However, the values quoted for the frequency of failed intubation in obstetric and non-obstetric cases are not comparable: the former relate to rapid sequence induction with cricoid pressure, the latter to elective induction of anaesthesia. Cricoid pressure can distort the larynx and even occlude it [2].

We disagree with their recommendations to increase the number of intubation attempts permitted in the “failed intubation protocol”. Failed tracheal intubation is not an admission of incompetence. As the authors point out, it is the avoidance of the complications of failed intubation which is the goal of management.

The upper airway in pregnancy tends to be oedematous as the author has postulated, and damaged more easily by trauma. This is especially true in pre-eclampsia and eclampsia [3, 4]. There is a danger of repeated attempts at tracheal intubation, especially blind, of causing airway narrowing to the point where a “can’t intubate, can’t ventilate” condition may change to “can’t intubate, can’t ventilate”. Traumatic intubation even when successful may also be associated with laryngeal obstruction after extubation, a danger which is less likely to be anticipated [5].

We suggest that failed intubation regimens should continue to include a limit on the number of intubation attempts, and ensure that, when appropriate, a more senior person is called for subsequent attempts.

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**Peripheral analgesic effect of morphine**

Sir,—It was interesting to read about the double-blind, randomized study carried out by Flory and colleagues on morphine added to interscalene brachial plexus block for analgesia after shoulder surgery [1]. We wish to make the following observations.

(a) The success of the interscalene block was achieved after both parenteral and neuraxial administration. Morphine administered in this study had no effect on the block, whereas for the relief of pain from the stellate ganglion, morphine does not seem to be effective while interpleural bupivacaine provides satisfactory pain relief for up to 6 h after laparoscopic cholecystectomy [7]. When injected around the stellate ganglion, morphine does not seem to be effective while interpleural bupivacaine provides satisfactory pain relief for up to 6 h after laparoscopic cholecystectomy [7].

(b) The analgesic supplements used in this study were either i.m. diclofenac or s.c. morphine, or both. The efficacy of s.c. and i.m. administration of analgesics is variable. This is not the case with the i.v. route of administration and it is well recognized that patient-controlled analgesia is a more reliable method of assessing postoperative opioid requirements. Supplementary analgesic requirements could have been assessed better in this way.

(c) All patients received 0.5 % bupivacaine 40 ml with adrenaline 1:200,000, which was seen to provide satisfactory analgesia for a mean of 11.2 h. It is possible that bupivacaine masked any analgesic effect of morphine in this study. A group of patients could have been given interscalene injection of morphine alone to evaluate its analgesic effect when deposited near major nerves.

The initial studies on morphine deposited around the free nerve endings were very encouraging. Intra-articular morphine, compared with bupivacaine, does not appear to provide significant analgesia after knee arthroscopy [2–5]; it does not afford any additional analgesic effect when added to bupivacaine in day-case arthroscopy and after biliary surgery when used for intercostal blocks [2, 3, 5, 6]. Morphine administered interpleurally is ineffective while interpleural bupivacaine provides satisfactory pain relief for up to 6 h after laparoscopic cholecystectomy [7].

Peripheral opioid receptor-mediated analgesia could be of short duration and of inferior quality than the centrally mediated effect achieved after both parenteral and neuraxial administration. Peripheral receptors may be less in number and have less affinity than those of the central nervous system.

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2. Ruwe PA, Klein I, Shields CL. The effect of intra-articular injection of morphine and bupivacaine on postarthroscopic

OF COURSE WE DO NOT IMPLY THAT ANY SHAME SHOULD BE ATTACHED TO FAILED INTUBATION—it happens to us all—but that is not the point. The point is that multicentre studies are needed, collating details of all failed intubations, including what training had been received by those involved, comparing values in different units, etc. Anaesthetists and obstetricians argued the need for this type of information more than 30 yr ago; belatedly, there is now government recognition of the need for clinical audit. Such studies may find no answer; on the other hand, this may be another insoluble problem which proves to have a straightforward solution, provided we maintain a creative, not a defeatist, frame of mind.

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Mivacurium and prolonged neuromuscular block

Sir,—Dr Davis in his recent letter [1] suggested that the time to full clinical recovery after administration of mivacurium to a patient who is either a homozygote for the atypical or silent gene or a heterozygote for the atypical and silent gene may be significantly longer than the 3 h quoted in Fox and Hunt’s report [2].

Two recent cases, in my own clinical practice, of prolonged neuromuscular block after administration of mivacurium do not confirm this. Both patients were healthy, young adults who received mivacurium 0.15 mg kg\(^{-1}\) in the course of day-case anaesthesia. In the first patient, 3 h elapsed and in the second patient 5.5 h before full return of neuromuscular function with normal train-of-four (TOF) response on peripheral nerve stimulation. Neostigmine was not given to the first patient, but 2.5 mg was administered to the second patient when the TOF ratio was visually estimated to have recovered to about 60%. Marked clinical improvement occurred shortly after allowing tracheal extubation to take place. Genotyping showed the first patient to be either homozygous for the atypical gene or heterozygous for the atypical and silent genes and the second patient to be homozygous for the atypical gene.

I agree with Dr Davis that the occurrence of prolonged apnoea after administration of mivacurium in day-case anaesthesia can be very inconvenient. It disrupts the operating list, requires the use of intensive care facilities and can cause considerable distress to the patient’s relatives.

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Thiopentone anaesthesia at Pearl Harbor

Sir,—We were delighted to read the historical article by Bennett [1]. The criticism of the use of thiopentone after the attack on Pearl Harbor by the Japanese on December 7, 1941 [2] has haunted military anaesthetists to the present day. It is a great relief to learn that the rumoured death rate was grossly exaggerated. One can only speculate on Dr Halford’s motives but it is interesting that a surgeon’s opinions could have had such an adverse effect on the practice of anaesthesia.

Two contemporary accounts by Dunnill [3] and Raffan [4] described the use of thiopentone as the sole anaesthetic agent in 200 cases. Raffan was serving in the Middle East and Dunnill in a tropical country where there was a shortage in the supply of nitrous oxide. Both anaesthetists described simple improvised apparatus for the administration of the drug. Thiopentone was used in very ill patients and those suffering from shock, but neither author reported serious morbidity or mortality. We were aware of these articles when we wrote our article [5] on the use of total i.v. anaesthesia (TIVA). Nevertheless, the use of TIVA in the field has mirrored the lack of popularity of this technique in normal anaesthetic practice.

Anaesthesia for war injuries is practised in situations where sophisticated apparatus is unavailable and nitrous oxide unobtainable. Nitrous oxide cylinders are heavy to transport and difficult to refill after use. TIVA with modern drugs offers an alternative to draw-over anaesthetic methods. The immediate future of TIVA is with propofol and the short-acting opioids [6], or propofol and ketamine [7]. Some basic principles remain essential: knowledge of the pharmacology of the drugs used, familiarity with their use, meticulous attention to the correction of hypovolaemia and the prevention of hypoxia.

We hope that the shadow cast 52 years ago over the use of TIVA in military anaesthesia has been lifted.

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Maximum dose of lignocaine for brachial plexus block

Sir,—The article entitled “Maximum recommended doses of lignocaine are not toxic” [1] is interesting to those who are enthusiastic in giving axillary brachial plexus block. The authors

claimed that doses of lignocaine up to 900 mg with adrenaline, approximately twice the maximum recommended dose, could be administered in transarterial axillary brachial plexus block without systemic toxicity.

Maximum recommended doses for local anaesthetics, stated in most anaesthetic textbooks, are used to minimize the risk of administration of excessive amounts of drugs that may result in systemic toxicity. Systemic toxic effects are related to the therapeutic index of local anaesthetics and the rate of vascular absorption, metabolism and excretion. One of the factors which influences the rate of absorption is the site of injection [2]. Normally, vascular absorption of local anaesthetics from the neurovascular sheath is a slow process [3]. Cockings, Moore and Lewis [4] used mepivacaine 750 mg, approximately twice the recommended maximum dose, for transarterial axillary brachial plexus block with a very low risk of mild systemic toxicity. A much higher dose of mepivacaine has been used previously in brachial plexus block. It is not certain if the use of similar amounts of mepivacaine or lignocaine by other techniques of axillary brachial plexus block still has a low incidence of systemic toxicity. Perivascular axillary block, with a frequency of 6% of unintended injections of local anaesthetics outside the neurovascular sheath [5], certainly has a higher risk of systemic toxicity if excessive amounts of local anaesthetic are given.

It is not known if the maximum dose of other local anaesthetics can also be increased in transarterial axillary brachial plexus block. Bupivacaine in brachial plexus block should be used cautiously. Administration of more than 1.5 mg kg⁻¹ of bupivacaine through the blocks could result in peak venous plasma bupivacaine concentrations close to those observed in cardiac complications, although peak venous plasma concentrations were still lower than the neurotoxic limit [6]. Moreover, systemic toxicity with bupivacaine has occurred after axillary brachial plexus block with "normal doses" of bupivacaine. Exceptionally rapid absorption of local anaesthetic after a successful axillary brachial plexus block with bupivacaine 3 mg kg⁻¹ resulting in a potentially toxic plasma bupivacaine level has been reported previously [3]. In two other cases of systemic toxicity immediately after axillary block with bupivacaine 150 mg, rapid diffusion of the drug into a relatively empty axillary vein [7] and flow of local anaesthetic into a punctured axillary vein [8] were implicated as the causes of the rapid development of toxic effects.

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Sir,—We thank Dr Sun for his valuable remarks on our article. These remarks underline our findings that the site of administration of local anaesthetic should be taken into account when the toxic dose is determined.

Dr Sun warns also about the toxicity of bupivacaine. We performed our studies with lignocaine and the results apply solely to this drug. Bupivacaine is known to be more toxic than lignocaine. We agree with Dr Sun that one should be very cautious when using bupivacaine as the local anaesthetic in transarterial plexus block. This is one reason why we never use bupivacaine alone for this procedure.