Absence of memory for intraoperative information

Sir,—I wish to comment on the article by Russell and Wang on absence of memory for intraoperative information during surgery under adequate general anaesthesia.1

Their study seemed to have been motivated by their belief that the brain can only process information during anaesthesia if anaesthesia is inadequate enough to result in the patient being conscious. Using the isolated forearm technique (IFT), they showed that there was no evidence for explicit recall, or implicit memory changes in patients, for times at which these patients were unresponsive to verbal command.

I would agree that changes in implicit memory are more likely to occur at times when the anaesthetized patient is responsive to verbal command, but their assumption that an observed response when using the IFT automatically indicates a state of consciousness is, I believe, not warranted.

As they themselves quoted, with some general anaesthetic techniques more than 70% of patients may be capable of responding to commands during surgery (using IFT), with no postoperative explicit recall of intraoperative events. It is still not necessarily the case however, that the patient was conscious, even if postoperative changes in implicit memory are demonstrated in these patients.

All this implies is that the brain has processed information in some way and created a response.

The authors seem to believe that IFT acts as a reliable “consciousness monitor” or indicator of “adequate” anaesthesia, but they disregard the possibility that responses observed while using this technique might also reflect pre-conscious information processing.

In other words, whereas patients might respond appropriately, they might still not be conscious.

The major fault with this study is that in attempting to disprove the notion of pre-conscious information processing, they make use of a clinical tool (IFT) which, much of the time, may in fact measure exactly what they wish to disprove.

Until when, and if, it is demonstrated that this type of anaesthesia results in significant psychological morbidity, there is no reason to suppose that it is not “adequate”.

M. B. HOOPER
Department of Anaesthesia
Townsville General Hospital
Townsville, Queensland
Australia


Sir,—Dr Hooper speculates on our motives for conducting this study. The background is the voluminous literature apparently demonstrating implicit learning during general anaesthesia. Typically, these studies involve the use of neuromuscular blocking agents and absence of systematic intraoperative monitoring of levels of consciousness. Given the poor relationship between intraoperative cognitive function and postoperative explicit recall, it is quite possible that in many of these studies intraoperative information was presented to patients who were wakeful. Our study demonstrated that while level of consciousness is controlled such that there is insufficient higher cortical function to allow response to command, verbal learning (whether explicit or implicit) is precluded.

Dr Hooper’s use of the terms “conscious” and “unconscious” is misleading, because it assumes two discrete states. The evidence generated by interest in memory and awareness during anaesthesia over the past decade demonstrates that we have a continuum, which can be delineated in terms of various experimental paradigms.

Dr Hooper’s letter also implies the common misconception that absence of postoperative recall is synonymous with intraoperative oblivion, and therefore “adequate anaesthesia”. Readers who have clinical experience of patients with dense amnestic states such as Korsakoff’s psychosis need no persuading of the mistake in this assumption. Memory function in such patients is irreversibly impaired such that memory of moment-to-moment experience is not maintained beyond a few minutes. However, it would be wrong to characterize them as having impairment of consciousness: all other cognitive functions may be normal, they are commonly alert and have a clear sensorium. The relevance of this for anaesthetists is that many drugs used in general anaesthesia have precisely these types of effects on memory function, albeit in a reversible manner.

We are unclear as to what Dr Hooper means by “pre-conscious processing”. Normally this term refers to perceptual processes which occur without the involvement of higher cortical function. We are certainly not attempting to disprove the existence of such processes. However, it is incorrect to suggest that specific motor responding to a verbal command is purely a function of pre-conscious processing. Response to command, particularly if that command involves conditional clauses such as “if you are in pain, squeeze your fingers twice” clearly involves operation of higher cortical centres. It seems that Dr Hooper is attempting to use the term “pre-conscious processing” as a semantic device with which to dismiss the significance of high levels of intraoperative cognitive processing.

M. WANG
Clinical Psychology Unit
University of Hull
Hull Royal Infirmary
Hull


Sevoflurane or halothane for paediatric endoscopy

Sir,—We were interested in the conclusion of Meretoja and co-workers in their article comparing halothane with sevoflurane for paediatric endoscopic procedures.1 We have recently undertaken an audit of sevoflurane, as a new drug, in this anaesthetic department and found that in a study of 344 adults and children, only two patients complained of nausea or vomited in the recovery area after administration of sevoflurane. An analysis of the patients in our audit by the ages used in the study of Meretoja and colleagues showed that of 17 patients aged 3–11 months, 92 patients aged 1–5 yr and 14 patients aged 6–15 yr who received sevoflurane for induction and maintenance of anaesthesia, only two patients in the middle age group vomited. A total of 20 patients aged 3–11 months, 243 patients aged 1–5 yr and 66 patients aged 5–16 yr (with 15 adult patients) also received sevoflurane for maintenance of anaesthesia with no additional patients suffering nausea and vomiting. All paediatric patients are kept in the recovery area for a minimum of 1 h in recovery after anaesthesia and for a minimum of an additional 1 h before discharge.

The observation that a relatively short administration of sevoflurane may confer antiemetic properties, similar to those of propofol,2 in the postoperative period has been confirmed by
other authors and our audit.3-5 A recent study comparing propofol and sevoflurane found that the incidence of nausea and vomiting was higher in patients receiving sevoflurane than in those receiving propofol, but this finding was not present in earlier work from this group.5,6 We also noted an emergence phenomenon similar to that reported by Johannesson, Floren and Lindahl; 19 of 329 children in our audit were observed to be “distressed”, “restless” or “agitated” in the recovery area.7 There were no long-term adverse sequelae.

We would support the conclusion of Meretoja’s group that further studies are indicated to confirm (and quantify) any antiepileptic properties of this new volatile agent.1

A. FLORMAN
J. BARSON
J. CLARKE
S. BOLVIN
Department of Anaesthesia
The Geelong Hospital
Geelong, Victoria, Australia

4. Bryson HM, Fulton BR, Faulds D. Propofol. An update of its effects of sevoflurane versus propofol in the induction and maintenance of anaesthesia that should be avoided, as there may be a difference in the sensory level of analgesia between the two groups was not statistically significant. However, if the difference in the level of maximal spread of analgesia is true for his comment on the single patient in whom the analgesia level was restricted to T12.

Spinal anaesthesia: effect of the site of injection on the spread of analgesia

Sirs—I read the article by Veering and co-workers1 with much interest. In their study they examined the effect of the site of injection on analgesia levels after spinal injection of 0.5% hyperbaric bupivacaine solution. The patients received 3 ml of 0.5% hyperbaric bupivacaine for spinal anaesthesia either at the L3–4 (group 1) or L4–5 (group 2) interspace. The highest analgesia levels in their study did not differ between groups.

In my opinion, the results of their study are not as disappointing as the authors claim. Admittedly, the results of their study showed that the difference in the level of maximal spread of analgesia between the two groups was not statistically significant. However, if we examine the results of their study more closely, we note an interesting difference; in group 1 (L3–4), six of 15 patients developed the highest level of analgesia to the fifth thoracic dermatome or higher, compared with only two of 15 patients in group 2 (L4–5). Although this may be a biased approach, it is nevertheless a high sensory level of spinal anaesthesia that should be avoided, as patients are at increased risk of developing hypotension. Therefore, it is probable that the small statistically insignificant difference in mean vertical decrease in SAP occurred in this group of patients.

On the other hand, one patient in group 2 developed the highest level of analgesia to the T12 dermatome, which was probably insufficient for blocking the pain caused by bladder distension during surgery.

It is a pity that the authors did not include an additional higher level of subarachnoid puncture in their study. There may have been a difference in the sensory level of analgesia between injection of the local anesthetic solution at the L4–5 interspace compared with injection at the L2–3 interspace.

In my opinion, when we perform spinal anaesthesia we should try to choose a higher approach to the subarachnoid space (preferably L2–3) and give less local anesthetics, which we considered better control of spread of analgesia in decreasing the number of patients with too high and too low analgesic levels after spinal anaesthesia.

M. KAMENIK
Department of Anaesthesiology, Intensive Care, and Pain Management
Maribor Teaching Hospital,
Ljubljanska, Maribor, Slovenia


Sirs—Thank you for the opportunity to respond to the comments of Dr Kamenik on our article.1 As noted, the highest levels of analgesia after injection via the L3–4 and L4–5 interspaces did not differ. In his comments Dr Kamenik draws attention to the fact that six of 15 patients who received injection via the L3–4 interspace had the highest analgesia levels extending to T5 or higher compared with only two of 15 patients who received the injection via the L4–5 interspace. Furthermore, Dr Kamenik relates the higher incidence of high thoracic analgesia levels to the slightly larger, although not statistically significant, decrease in systolic arterial pressure in patients who received the injection via the L3–4 interspace.

The question which must be addressed in this respect is whether or not we might have missed a true difference in the levels of analgesia associated with injection via the L3–4 and L4–5 interspaces. As noted in the article, the study was based on a power analysis of earlier observations, and aimed at an 80% probability of detecting a difference of two segments, which we considered clinically relevant. This includes a 20% probability of missing a true difference of two segments, which is generally considered acceptable from a statistical point of view. Of course, the probability of missing a true difference of one segment is considerably greater. In this respect the comments of Dr Kamenik may be justified. However, as his comments are based on isolated observations, these are highly speculative and should be tested prospectively, possibly by a study aimed at the number of patients reaching analgesia levels in the high thoracic region. The same is true for his comment on the single patient in whom the analgesia level was restricted to T12.

We did not include a group of patients receiving injection via the L2–3 interspace because an earlier study revealed no difference in analgesia levels after injection via the L2–3 and L3–4 interspaces.2 In addition, the injection sites were selected on anatomical considerations, as described previously and discussed in our article. Briefly, hyperbaric solutions, under the influence of gravity, migrate preferentially to the lower levels of the subarachnoid space (i.e. below L3 to T4 in the lumbarosacral concavity and above L3 to L4 in the thoracic concavity).3-4 As such it is expected that injection of a hyperbaric solution via the L2–3 interspace with the patient in the sitting position during injection and until 2 min thereafter, followed by horizontal supine positioning, would result in higher analgesia levels than injection via the L4–5 interspace.

Overall, inter-individual variation in analgesia levels after subarachnoid injection of hyperbaric bupivacaine solutions is impressive, especially in elderly patients. Any modifications in the spinal procedure that could reduce this variability and prevent excessive cephalad spread of the local anesthetic solutions, and thereby result in a more predictable and safer spinal block, would be welcome. The suggestion of Dr Kamenik to reduce the dose of local anesthetic may deserve investigation.

B. TH. VEERING
P. M. TER RIET
A. G. L. BURM
R. STEINSTRA
J. W. VAN KLEEF
Department of Anaesthesiology
Leiden University Medical Centre
Leiden, The Netherlands

1. Veering BTh, ter Riet PM, Burm AGL, Stienstra R, van Kleef JW. Spinal anaesthesia with hyperbaric 0.5% bupivacaine

British Journal of Anaesthesia

144

Cuff deflation for easier weaning from ventilation

Sir,—We read the editorial by Shneerson1 with great interest and agree that much work needs to be done to assess the relative importance of each of the new approaches during the weaning process. Shneerson stated that cuff deflation is probably the single most underrated and important step in improving upper airway function and should have a central place in the preparatory phase of weaning. The reason cited is that deflation of the cuff improves the co-ordination of the laryngeal and pharyngeal muscles by reinstating the respiratory function of the larynx and by allowing patients to speak.

The above explanation is logical; however, another immediate advantage of deflating the cuff is reduced work of breathing, allowing easier weaning. During quiet breathing, intrapleural pressure at the base of the lungs varies between −2.5 mm Hg and −6 mm Hg.2 However, if an adult breathes through a size 9.0 mm internal diameter (id) tracheal tube, peak intrapleural pressure developed during normal inspiration is approximately −15 mm Hg,3 indicative of increased work load of breathing. A size 8.0 or 9.0 mm id tracheostomy tube should also produce changes of similar magnitude. Although these changes seem modest, in patients with weaning problems they could be clinically important. Even a small reduction in work load of breathing could be crucial in determining the outcome in such patients.

Deflation of the cuff allows the patient to breathe both through the lumen and also from around the tube. This leads immediately to a significant decrease in the resistance and work load of breathing. The fact that patients are able to phonate is evidence of the higher resistance offered by the tracheostomy tube compared with the rest of the upper airway. Speech would not be possible if the resistance of the tracheostomy tube was much less than that offered by the upper airway, as this would allow the air to escape through the tracheostomy tube, taking the path of least resistance.

Sleep deprivation in intensive care during the recovery phase is widespread and deserves further attention. Attempts should be made to establish a normal sleep–wake cycle. Generally patients in intensive care units are receiving constant rate infusions of sedatives and analgesics. Perhaps administration of sedatives to follow the circadian rhythm may help, so that during the night patients are well sedated and during the day, almost awake. Whether patients would benefit from such a strategy is open to question and needs further investigation.

J. R. ALLSOP
Department of Obstetrics and Gynaecology
Ipswich Hospital NHS Trust
Ipswich

1. Wheatley T, Veitch PS. Recent advances in prophylaxis against deep vein thrombosis. *British Journal of Anaesthesia* 1997; 78: 118–120.

Prophylaxis against deep vein thrombosis

Sir,—Wheatley and Veitch in their editorial on recent advances in prophylaxis against deep vein thrombosis stated that women receiving the combined oral contraceptive pill (OCP) should discontinue it 6 weeks before elective surgery.1 While it is accepted that combined OCP users are at increased risk of venous thromboembolism, in the presence of other risk factors there is insufficient evidence to support a policy of routinely stopping the combined OCP before surgery.2 A burden of responsibility is put on the person advocating stopping the combined OCP to arrange suitable alternative contraception. Failure to do so may present the anaesthetist with an unexpected pregnancy, the effects of anaesthesia and surgery on an undiagnosed pregnancy and termination of pregnancy with its physical and emotional sequelae. The Royal College of Obstetricians and Gynaecologists Working Party report on thromboprophylaxis made three recommendations.3

(1) Patients receiving combined oral contraception should have a risk assessment profile performed, as with any patient undergoing gynaecological surgery.

(2) The combined oral contraceptive need not be ceased nor thromboprophylaxis commenced in women undergoing uncomplicated minor and intermediate procedures.

(3) Patients receiving combined oral contraception who are assessed as moderate risk should receive one of a variety of prophylactic measures. Whether it is necessary to cease oral contraception before surgery is unclear, but if this is done, adequate counselling regarding pregnancy risk or alternative contraception (e.g. progestogen only pill) should be provided. The situation with regard to the patient’s additional risk factors should be considered on an individual basis.

Wheatley and Veitch also stated that hormone replacement therapy (HRT) has not been shown to increase the risk of thromboembolic disease. This is not accurate. Three recent studies have shown an increased risk of venous thromboembolism in users of HRT.4–6 The risk of a blood clot increases from 1:10 000 per year in those not receiving HRT to 3:10 000 per year in those who are, so the effect of replacement treatment seems to be small. Assessing risk based on age and other risk factors still pertains.

J. R. ALLSOP
Department of Obstetrics and Gynaecology
Ipswich Hospital NHS Trust
Ipswich


Sir,—I thank Drs Bapat and Verghees for their comments. I agree that cuff deflation reduces the work of breathing and can enable patients to breathe spontaneously at an earlier stage in the weaning process. This benefit of cuff deflation is distinct from its use in improving upper airway function and is only of value when the patient is not attached to the ventilator. In contrast, upper airway coordination can be improved both when the patient is breathing spontaneously and while receiving ventilatory support. Cuff deflation can be used to achieve this, even before withdrawal of ventilatory support is attempted or between initial trials of this. These two benefits of cuff deflation, reduction of work of breathing and improvement in upper airway function, are therefore complementary and assist the weaning process in different ways.

J. SHNEERSON
Respiratory Support and Sleep Centre
Papworth Hospital
Cambridge

Sir,—Dr Allsop is correct in highlighting an area of continuing controversy regarding thromboembolic prophylaxis for women taking the oral contraceptive pill (OCP). The report of the Royal College of Obstetricians and Gynaecologists Working Party does not consider taking the OCP to be an independent risk factor for thromboembolic disease in patients undergoing surgery, but in combination with pre-existing risk factors it admits that there is uncertainty as to whether the OCP should be stopped before surgery. In contrast, the European Consensus Statement regarded any woman receiving the OCP as being at an increased risk and therefore warranted either cessation of this medication 4–6 weeks before surgery or some type of prophylaxis. Obviously any woman who ceases to take the OCP must be advised to use adequate alternative contraception if she is to avoid the unquantified risk of unwanted/undiagnosed pregnancy and termination.

The three recent studies showing an increased risk of thromboembolic disease in women receiving hormone replacement therapy (HRT) were all published in the same edition of the Lancet after this editorial was written, and represent some of the first evidence that HRT may be associated with such a risk.

T. J. WHEATLEY
Department of Surgery
Leicester General Hospital
Leicester

Intraocular pressure and suxamethonium

Sir,—After reading the case report of the use of rocuronium in a pregnant patient with an open eye injury by Gaiser and Seem,1 I feel that it is necessary to comment on their recommendations concerning the avoidance of suxamethonium.

As in many situations there is a balance of risks to be sought before choosing any particular anaesthetic technique. In this case the authors decided that the risks of damage to the injured eye in using suxamethonium outweighed the risks of using rocuronium in a pregnant patient with a potential full stomach and a known increased incidence of difficult intubation.2 But is this the correct balance of risks? Rocuronium removes the safety net that suxamethonium has of potentially wearing off, should intubation be difficult or impossible, and what is the evidence that suxamethonium causes problems in open eye injury? It is true that suxamethonium causes an increase in intraocular pressure (IOP) of 10 mm Hg,3 but not using suxamethonium does not imply that suxamethonium causes an increase in IOP of 40 mm Hg.4 Blindling increases IOP by 5 mm Hg5 and forcible eyelid closure may cause an increase of up to 70 mm Hg6 events likely to have occurred in any person who has suffered a severe eye injury.

In addition, as long ago as 1957 thiotepone was shown to abolish the increase in IOP associated with suxamethonium, and Edmondson and colleagues demonstrated in 1988 that there were no significant increases in IOP compared with baseline values when suxamethonium and thiopentone were used as part of a rapid sequence induction and intubation.

More recently, and more significantly, Moreno, Kloess and Carlson11 using an anterior and posterior trauma model in the cat eye found that there were no cases of vitreous extrusion in 38 separate administrations of suxamethonium. From this they concluded that suxamethonium, when indicated, should be considered in patients with suspected or known open globes.

Have there been cases of vitreous extrusion after suxamethonium reported in the literature? Very few can be found. In 1957 Lincoff, Breinin and DeVoe7 reported several cases (by personal communication) of prompt expulsion of vitreous when suxamethonium was used to try and forestall impending vitreous extrusion; an unfair situation in which to put all the blame on suxamethonium. The only other report of expulsion of intraocular contents after suxamethonium that I could find was a letter by Rich and colleagues in 1986,12 but no anaesthetic details were given. This contrasts sharply with the combined 900 cases of penetrating eye injury reported by Libonati, Leahy and Ellison13 and Donlon14 where the use of suxamethonium caused no additional damage to the eye.

In conclusion, until we have a non-depolarizing neuromuscular blocking agent with the same potency and short duration of action as suxamethonium, it will always have a place in the balance of risks for a patient with an open eye injury.

L. EDMONDS
Anesthetic Department
Wansbeck General Hospital
Ashington, Northumberland


