Correspondence

Radiation exposure of nurses on an intensive care unit

There is an awareness of radiation as a possible occupational hazard and radiological investigations are frequently performed outside the main hospital radiology suite. When performing mobile X-ray examinations, all personnel should wear appropriate shielding and, whenever patient safety permits, distance themselves as far as possible from the source of radiation. The levels of scattered radiation from different mobile X-ray units have been shown to differ and the intensity of scatter of some types of beam does not decrease as rapidly as with others [1].

Exposure of groups of health care workers, e.g. orthopaedic surgeons [2] and anaesthetists [3], has been studied but the degree of exposure of nursing staff on an adult intensive care unit (ICU) is not known. Nursing staff working in an intensive care environment are repeatedly exposed to scattered radiation, chiefly from frequent diagnostic chest X-rays. It is generally assumed that the doses are very low, and consequently intensive care nurses are not included in radiation protection personnel monitoring services.

In a prospective study, we monitored nursing staff on an eight-bedded general adult ICU to determine occupational exposure to radiation. Five film badge dosimeters were placed in circulation over a continuous 8-week period. The film badge employs a small piece of photographic film as the radiation detector, which becomes blackened when exposed to radiation. They have a minimum detectable dose of 0.05 mSv. The badges were worn by the nursing staff at all times, and at the end of each shift they were transferred over to the next shift nurses. The usual precautions were taken to avoid exposure from scattered radiation, which, in our unit, largely comprises ensuring a minimum ‘safe’ distance of 2 m. It was possible to show that the period of study was typical as regards the use of X-rays.

The badges indicated negligible cumulative exposure over the 8-week period. Three badges were below detectable limits; the other two badges recorded 0.05 mSv. With current working patterns of intensive care nurses and the fact that the badges were exchanged between nurses throughout three shifts per day, this can be extrapolated to indicate negligible exposure for any individual nurse over the 8-week period.

Over the course of a year, an intensive care nurse would not exceed the recommended maximum whole body dose limits set by the Ionizing Radiation Regulations of 20 mSv yr$^{-1}$ for a non-classified radiation worker [4]. Radiographers typically receive an effective whole body dose of about 0.3 mSv yr$^{-1}$. For comparison, the typical patient dose from a chest X-ray is 0.02 mSv. Adherence to standard protective measures precludes most exposure to machine-produced radiation. The minimal exposures reported do not justify the regular use of dosimeters. The intensive care unit is believed to use more X-rays than other wards, so exposure elsewhere should also be undetectable.

We conclude that ICU nurses can provide quality care to their patients without concerns over the detrimental effects of radiation exposure, provided that the basic principles of radiation protection are followed.

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References

Saturation during upper GI endoscopy

I was interested to read Wang et al’s recent article (Wang et al. Anaesthesia 2000; 55: 654–8). Although other authors have reported that sedation increases the incidence of desaturation [1, 2], we showed that the experience

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(and, by inference, skill) of the endoscopist was at least if not more important. We [3] have noted that desaturation tended to occur during periods of struggling and retching, so comparing different endoscopists with no assessment of patient tolerance cannot therefore give a valid comparison of sedation with no sedation.

The effect of sedation on desaturation is most likely to manifest itself the longer the procedure continues. Not surprisingly, there are big reported differences in this respect — approximately 3 min in our study, 6 min in the study by Wang et al. and 12 min in the study by Lieberman. I think it is likely that if endoscopy lasts less than about 4–5 min, then operator skill is the most important influence on desaturation, with sedation having little effect unless the procedure lasts longer than this.

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References

Pulse oximetry – to bleep or not to bleep?

Standards of minimal monitoring during anaesthesia are well established [1] and include the provision of a pulse oximeter for all patients. The benefits of pulse oximetry are well known. In the Australian Incident Monitoring Study (AIMS), the pulse oximeter ranked first in picking up monitor-detected events [2]. Findlay and colleagues demonstrated that pulse oximetry combined with non-invasive blood pressure measurement, ECG and clinical observation detected 90% of critical incidents [3].

There are no recommendations on how pulse oximetry should actually be used. In addition to a visual display, pulse oximeters can provide a continuous auditory signal with a variable pitch according to the patient’s oxygen saturation. Monitors vary in the auditory default setting and the auditory information, when available, is not always used.

We observed the routine practice of 25 consultant anaesthetists. All used a pulse oximeter in the operating theatre. Seventy-two per cent used the pulse oximeter with a variable pitch signal. More consultants utilised the variable pitch tone when the monitors were configured with the variable pitch tone on (12/12) compared to when the monitors defaulted to the variable tone switched off (6/13). This was significantly different (p = 0.0052).

The advantages of auditory alarms over visual alarms have been documented [4]. The AIMS study suggested that the use of a variable pitch tone rather than the bleep of an ECG would have improved the detection rate of critical incidents by the pulse oximeter from 27% to over 40% [2]. In a scenario based simulator study, Craven and McIndoe demonstrated that anaesthetists using a variable tone pulse oximeter recognised an episode of oxygen desaturation significantly quicker than anaesthetists using a fixed tone [5].

The use of a pulse oximeter with a variable pitch tone may improve patient safety, and our data suggest that ensuring that pulse oximeters default to the variable tone automatically switched on significantly improves the use of this facility.

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Tec 6 vaporiser

I read with interest the letter regarding the warming effect of an operational Tec 6 vaporiser (Chandler & Poulton, Anaesthesia 2000; 55: 1031–2). As they correctly observe, the operation and maintenance manual clearly points out the fact that the Tec 6 gets warm. I feel that it is relevant to point out that elsewhere in the manual it also clearly states that the Tec 6 vaporiser should be positioned on the right-hand edge of the back bar, with its power lead running in the groove underneath the vaporiser and then taken around the outer edge of the anaesthetic machine. The reason for this is to prevent the wire interfering with the interlock system, which can result in it being impossible to turn on any vaporiser. The figure in the letter showed a Tec 6 vaporiser in the middle position, though the lead was not visible.

Positioning the Tec 6 vaporiser as recommended by the manufacturers

References
may avoid warming any drugs that have been kept in this handy recess, as well as avoiding the potential for a real critical incident.

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**Bad connection**

We wish to report an unexpected problem with the use of Clave® and three-way taps. In our institution, it is common to anaesthetise children for radiotherapy. Most of them are done repeatedly on a day case basis using a propofol infusion via a Portacath. The port is usually accessed at the beginning of the week, and the open end of the access line is usually fitted with a Clave® (ICU Medical Inc.) or a Single Extension with a Clave® (Kimal plc). Both are valved, needle-less ports that may be used to inject drugs without disconnection or the use of a needle.

We were in the habit of using three-way taps (Connecta® Plus 3, Becton Dickinson) connected to the distal end of the propofol infusion and then luer-locked on to the Clave. This enables us to have easy access to the venous line in case we need to inject any other drugs. Our assumption, reinforced verbally by the representative from Kimal, was that if a regular syringe could be used to inject into the Clave® we could also use a three-way tap.

We found that on multiple occasions, the syringe driver would alert us to an occlusion message. Sometimes, this would be rectified after reapplying the luer-lock of the three-way tap.

We tried to analyse the reason for the infusion failure by re-examining the Clave/three-way tap connection.

As shown in Fig. 1, the Clave® in an explosion diagram has a silicon sheath covering the actual injection lumen. With a luer-slip Terumo® syringe and most other luer-lock/slip connectors, the sheath folds down on itself to expose the injection lumen of the port to the syringe, which forms a lock over it. The three-way tap on the other hand has a wider internal diameter than the syringe (Fig. 2). This seems to cause the plastic sheath of the Clave® to kink on itself, thereby blocking the injection lumen. The result is an occlusion alarm on the syringe driver.

With the three-way taps, it is possible occasionally for the sheath not to kink. Most times, however, the tightening of the luer lock causes the Clave® to occlude the three-way tap.

This highlights the importance of ascertaining compatibility of different systems before using them. We initially assumed that the three-way taps would work with Claves. If syringes work well, why don’t the taps? Indeed, the luer-lock appears to fit well and tight. However, the actual infusion cannot be started.

We are constantly reminded of the need to check and recheck anaesthetic apparatus. This should apply to intravenous equipment as well. The importance of this, to avoid the potential for a
stormy and possibly dangerous anaesthetic, cannot be overstressed.

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The Flexiblade 1

I feel strongly that the recent paper on the Flexiblade laryngoscope (Perera et al. Anaesthesia 2000; 55: 890–3) should not be allowed to go unchallenged. Their conclusion that ‘the Flexiblade can be very useful in the management of the difficult airway’ is not supported in any way by their work. Firstly, they have not assessed the Flexiblade in patients with difficult airways, and secondly they have not compared it with the standard Macintosh blade to see if the view obtained is better or worse.

They state that the blade, when the lever is not depressed, behaves like a normal Macintosh laryngoscope (the same assumption was made when first assessing the McCoy but this was later shown not to be the case [1]). In this series, the Flexiblade without the lever depressed gave a 9.5% incidence of grade 3 laryngoscopy – not at all similar to the performance of the Macintosh blade.

It may be that the Flexiblade proves to be useful in difficult intubation and difficult airway management, but there is no evidence for that yet. All that can be said on the strength of this paper is that the Flexiblade works better when the blade is flexed than in the neutral position and that the incidence of difficult laryngoscopy when using this blade is 1.5%. That is similar to the incidence with the Macintosh in other studies.

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Reference

The Flexiblade 2

In their evaluation of the Flexiblade laryngoscope (Perera et al. Anaesthesia 2000; 55: 890–3), Dr Perera and colleagues conclude that it ‘can be very useful in the management of the difficult airway because it significantly improves the view of the vocal cords’. We believe that this conclusion is not supported by the data presented, and may lead clinicians to use the device before it is fully evaluated. The study results only demonstrate that the view of the larynx obtained with the Flexiblade in the flexed position is better than when used in the unflexed position.

The study conclusions are based on the stated assumption that ‘when the Flexiblade lever is not depressed it behaves like a normal Macintosh laryngoscope’. Unfortunately, the authors present no data to support this assumption. They have not made a direct comparison of the two blades and have not referenced a study that has demonstrated their equivalence. There is circumstantial evidence to suggest that this assumption may not be correct. Firstly, our personal experience of the two laryngoscopes during direct comparison is that the unflexed Flexiblade exposes less of the larynx than a Macintosh blade. The exposure, however, becomes similar when the Flexiblade is flexed. Second, direct visual comparison of the two blades shows that the Flexiblade in the neutral position is considerably straighter than the Macintosh blade. Lastly, in unselected populations, the incidence of Cormack and Lehane grade 2 views is around 30% and grade 3 views between 1% and 5% [1–3]. In their study, Perera and colleagues excluded those patients with poor mouth opening or restricted neck movement, and therefore the proportion of grade 2 and 3 laryngoscopies would be expected to be reduced. However, when using the Flexiblade in the neutral position they found grade 2 views in 36% and grade 3 views in 10%. These observations suggest that the unflexed Flexiblade may produce a poorer view of the larynx than the Macintosh blade. The authors also state ‘the Flexiblade has considerable advantages (over the McCoy blade) in terms of improving laryngeal view’. Again, since no direct comparison has been done, this conclusion is not justified.

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Reference
made between the baseline views obtained with the two blades, this statement is speculation.

Before the Flexiblade can be recommended as a useful tool in the management of the difficult airway, it should be directly compared with the Macintosh and/or the McCoy laryngoscopes. Further evaluation may also reveal unexpected problems related to its use. Figure 3 shows a Flexiblade that broke during routine cleaning after five uses. The broken fragment was extremely sharp and could have caused considerable damage had the breakage occurred during use.

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References

\textbf{A reply}

We are grateful for the interest and the comments made by Drs Frerk, Gupta and Cook. We would agree that we did not set out to compare the Flexiblade with the Macintosh blade. What we compared were the views obtained with the neutral and the depressed blade, i.e. the neutral position acted as the control.

We showed that the views of the laryngeal inlet were improved and therefore thought the Flexiblade could be useful in the management of a difficult airway, which is not unreasonable. It would be difficult to test the suggestion that we study difficult airways as they only occur in less than 1% of the surgical patient population. We could use simulated difficult airways, but the major problem would be to obtain the same starting point.

We also note the comment on the incidence of difficult laryngoscopy. However, the main difference between the Macintosh blade and Flexiblade is that the ‘flexi’ end does enable the user to improve the view in a fashion similar to the McCoy laryngoscope.

\textbf{Use of the laryngeal mask is contraindicated during cholecystectomy}

Georgiou and colleagues reported the use of the laryngeal mask in a patient with myasthenia gravis undergoing laparoscopic cholecystectomy (Georgiou \textit{et al.} \textit{Anaesthesia} 2000; \textbf{55}: 821–2). I believe that the patient was at an increased risk of pulmonary aspiration of gastric contents and the use of the laryngeal mask is contraindicated in such a patient. Gastric acid secretion is likely to increase during biliary tract surgery \cite{1,2} and regurgitation of bile-stained gastric fluid is frequently observed during cholecystectomy \cite{3}, particularly when cholangiography is performed \cite{4}. Distension of the abdomen by gas insufflation further increases the risk of regurgitation and pulmonary aspiration. In fact, pulmonary aspiration has been reported in patients undergoing cholecystectomy in whom the laryngeal mask was used \cite{5,6}.

Georgiou and colleagues also stated that they maintained anaesthesia by a combination of thoracic epidural with light general anaesthesia. The risk of regurgitation or vomiting increases when general anaesthesia is not sufficiently deep.

Although the authors attempted to reduce the risk of regurgitation, prevention of pulmonary aspiration of gastric contents (particularly of bile) by tracheal intubation outweighs the advantages of the use of the laryngeal mask in patients with myasthenia gravis. There are several methods without using any neuromuscular blocking agents to obtain adequate muscle relaxation for relatively smooth tracheal intubation.

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\textbf{A reply}

Airway protection is certainly more efficient when tracheal intubation is used. There are, however, many reports of uneventful use of laryngeal mask airways in laparoscopic gynaecological and cholecystectomy operations, both of which involve distension of the abdomen by gas insufflation \cite{1}. In the present case, we took care to keep intraperitoneal pressure at 9–10 cmH$_2$O to avoid over-distension. The use of spontaneous ventilation may also be associated with a lower risk of pulmonary aspiration as there is no positive ventilatory pressure driving air into the stomach \cite{2}. The presence of a nasogastric tube, while reducing the potential protection of the upper oesophageal sphincter, permitted frequent suction and removal of gastric acid and bile.

Administration of ‘light’ general anaesthesia does not imply insufficient
depth as the patient had adequate analgesia provided by the thoracic epidural. Therefore, the risk of regurgitation and pulmonary aspiration was not expected to increase in connection with the depth of anaesthesia.

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References

A blocked catheter mount

I should like to report an unusual case of airway obstruction whereby a faulty flexible catheter mount (Intersurgical) caused total obstruction to gas flow.

A 49-year-old female with significant gastro-oesophageal reflux, but otherwise healthy, presented for laparoscopic cholecystectomy. She was the last patient on an afternoon operating list. The anaesthetic machine, breathing system and equipment had been checked at the start of the list and had been functioning satisfactorily. She was pre-oxygenated and good movement of the reservoir bag had been observed. After an uneventful rapid sequence induction and intubation, we were unable to ventilate her lungs and carbon dioxide was undetectable on capnography. With cricoid pressure maintained, the tracheal tube was removed and facemask ventilation performed. The lungs were easy to ventilate and we confirmed carbon dioxide in her exhaled gases. She was re-intubated with a different tracheal tube and again ventilation was impossible. This tracheal tube was also removed, but on this occasion facemask ventilation also was impossible. Whilst elements of the breathing system were then isolated in sequence, ventilation with a self-inflating bag with a non-rebreathing valve was successful. After a short time, during which the patient suffered a brief episode of desaturation, the catheter mount was found to be almost totally occluded by a disc of plastic (Fig. 4). The patient was allowed to awaken and the operation postponed. She suffered no ill effects from this experience.

This sequence of events as described above is explainable in retrospect. After the first extubation, the faulty catheter mount was omitted from the breathing system along with the first tracheal tube. Unfortunately, it was retained as part of the breathing system after the second extubation. Visual check of the catheter

Figure 4

Figure 5
mount failed to identify the defect (Fig. 5). Existing practice was to include the catheter mount as part of the breathing system only after intubation. Hence, pre-anaesthetic checks had failed to detect any potential problems of this nature. Both Intersurgical and the Medical Devices Agency were contacted with reference to the fault.

This case has a number of valuable learning points. Pre-oxygenation does ‘buy’ time to problem-solve, when presented with sudden and unexpected difficulties. The old adage of ‘if in doubt, take it out’ allowed a further period of oxygenation. An alternative method of ventilation should always be immediately available. Most importantly, however, only the most pedantic and stringent checking of all equipment will prevent anaesthetic accidents.

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A reply
We are grateful for the opportunity to reply to Dr Thomas’ letter.

The occlusion of the catheter mount connector was caused by a fault during manufacture. The cores, which form the hole through the component, failed to mate fully when the injection mould tool closed and plastic was injected. This was the first reported occurrence of an occluded component made with this mould tool, which has been in use for 3 years, and has moulded 4 million connectors in the last 12 months. Our quality systems are designed to prevent faults such as this from occurring, and we have taken urgent corrective and preventive action.

We have carried out a recall of potentially affected products, and modified the mould tool to prevent recurrence of this fault. We are constantly striving to improve the reliability of our manufacturing and assembly processes, by making use of available technologies, and we appreciate that even one fault of this type is not acceptable.

We agree with Dr Thomas that stringent checking of all equipment prior to use will help prevent anaesthetic accidents, and that all parts of the breathing system, including the catheter mount, should be included in pre-use checks.

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The Intavent® tracheal tube in nasotracheal fibreoptic intubation

We would like to add our support to the findings of Drs Lucas and Yentis which illustrate the advantages of the Intavent® tracheal tube for oral fibreoptic intubation (Lucas & Yentis. Anaesthesia 2000; 55: 358–61). They point out that the ease of railroading of this tube is likely to be due to the tapering of the soft silicone tip, which reduces the gap between the fibrescope and tube tip. This gap is thought to be the source of railroading problems with bevelled tip tubes, the incidence of which has been reported as 46–71% [1, 2]. Drs Lucas and Yentis postulate that similar results would have been obtained by the nasal route but did not investigate this further. We can report that our research (currently submitted to another journal) has shown the Intavent® tube to have similar advantages over bevelled tip tubes during nasotracheal fibreoptic intubation. Our results, presented at the Difficult Airway Society Meeting in Edinburgh (November 1999), showed 100% success rate at first time railroading for the new Intavent® tube. We also showed that difficulty in railroading tracheal tubes during nasal fibreoptic intubation can occur in 50% of patients using a Portex® tube, and 40% using a Mallinkrodt® reinforced tube. Unfortunately, Lucas and Yentis could find no practical way to blind their study. Our group, and that of another from Manchester [3] investigating oral fibreoptic intubation, independently found similar solutions to this problem: a disposable paper towel was placed over the tracheal tube with a hole to insert the fibrescope (Fig. 6). After placement of the scope correctly in the trachea by the operator, an observer then assessed the movement of the operator’s hand when advancing the tube into the trachea. This removed both visual and tactile clues to the type of tube used. The observer then graded the ease of railroading according to the classification described by Jones et al.
These three separate studies have all reached the same conclusions; that the new tapered tip design of the Intavent tube confers significant advantages over conventional bevelled tip tubes, and can be recommended for use in oral and nasal fibreoptic intubation.

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References

A new incident with the laryngeal mask airway

The laryngeal mask airway has become a very popular, convenient and useful part of our equipment for airway control during routine general anaesthesia, difficult intubation and ventilation situations. We would like to report an incident that occurred with the use of the laryngeal mask airway.

Anaesthesia was induced in a 40-year-old ASA 1 patient admitted for elective knee arthroscopy. There was some difficulty with mask ventilation due to the presence of a large beard, but a size 5 laryngeal mask airway was placed successfully. However, we could not connect it properly to the breathing circuit because the fit was loose. As there was no other easily available size 5 laryngeal mask airway, we had to enlarge the laryngeal mask connector by wrapping around it with 3M micropore tape. The laryngeal mask could then connect snugly with the breathing circuit and the operation continued uneventfully.

After anaesthesia, we inspected the laryngeal mask airway and discovered that the connector had been placed inside out, i.e. the end that should fit into the shaft of the laryngeal mask now became the 15-mm male connector end. According to the laryngeal mask airway manual, it is not possible to pull the connector out, but as our photo shows (Fig. 7) this has clearly happened. This could have happened during repeated cleaning and sterilisation. An unsuspecting member of staff might have fitted the connector the wrong way round.

To minimise the above problem, emphasis on the following should be made:

1. Education of staff regarding proper cleaning and sterilising techniques as described in the manufacturer’s manual [1].
2. Recording the number of times the laryngeal mask has been re-used (up to 40 times as recommended by the manufacturer) to avoid degradation of the components.
3. Conducting a careful and thorough pre-use check including the shape and size of the connector and its fit to the breathing circuit.

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Reference

A reply

Thank you for the opportunity to respond to this letter. The Laryngeal Mask Company fully agrees with the conclusions drawn by Drs Tan and Koh, that the correct cleaning and sterilising techniques described in the Instruction Manual should be taught to all staff, that the number of times each mask is used should be recorded and that the pre-use Performance Tests in the Instruction Manual should always be performed.

It should be noted that the Performance Tests state that the user should – ‘examine the 15mm connector. It should fit tightly into the outer end of the airway tube. Ensure it cannot be easily pulled off by hand using reasonable force.’ The tests further state that – ‘Failure of any one test indicates that the device has passed its useful life and should be replaced.’

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Subcutaneous emphysema caused by a fenestrated tracheostomy tube

We wish to report a complication that we encountered with the use of a fenestrated tracheostomy tube in one of our patients on ICU.

We performed a percutaneous dilational tracheostomy (Ciaglia technique) on a female patient with easily identifiable surface landmarks, and inserted a size 8.0 Portex Blueline Ultra fenestrated tracheostomy tube with an unfenestrated inner cannula (see Fig. 8). The procedure was performed under endoscopic guidance with a fiberoptic bronchoscope and proceeded without any difficulty until immediately after insertion of the tracheostomy tube. After we connected the tracheostomy tube to the ventilator, the operator noticed that the patient developed surgical emphysema around the tracheostomy site. The endoscopist confirmed with the fiberoptic bronchoscope that the tracheostomy tube was correctly placed in the trachea but the surgical emphysema was undoubtedly getting worse over the next 2±3 min. We decided to replace the tube with the same size and type, but without the fenestrations. We changed the tube without any difficulty and this immediately stopped the progression of the surgical emphysema. The remaining surgical emphysema cleared up over the next 12 h. The rest of the patient’s clinical course was uncomplicated. We successfully weaned the patient from mechanical ventilation and she was decannulated after 14 days.

The rationale for the use of a fenestrated tube from the outset is to eliminate the need to change the tube when the patient is weaned from mechanical ventilation as the fenestrations allow the patient to speak with the tracheostomy tube still in place. The new range of Portex Blueline Ultra tracheostomy tubes are designed to be used for up to 30 days.

Although surgical emphysema is a well-recognised complication following percutaneous tracheostomy [1, 2], we could not find any reference in the literature to the use of a fenestrated tracheostomy tube as the direct cause of surgical emphysema. We notified SIMS Portex of this incident and the company is now reviewing the design and position of the fenestrations on the Portex Blueline Ultra fenestrated tube (personal correspondence with SIMS Portex). The company updated the instructions for use of the product (IFU) to warn against the use of a fenestrated tube in a newly formed tracheostomy and to include a recommendation that clinicians should check the correct position of the fenestrations at the time of insertion.

We want to make clinicians aware of this potential complication of the use of a fenestrated tracheostomy tube and emphasise the importance of a translaryngeal endoscopic examination to confirm the correct position of the fenestrations in the trachea whenever a fenestrated tube is inserted. If the fenestrations are not correctly positioned in the tracheal lumen after insertion, it should be replaced with an unfenestrated tube.

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References

A reply
We would like to thank Drs Mostert and Stuart for raising awareness of the issue of subcutaneous emphysema associated with percutaneous tracheostomy, and fully support their recommendation to...
confirm the correct position of the fenestrations in the trachea.

As the authors state, SIMS Portex Ltd has updated the instructions for use of the Blue Line Ultra tracheostomy tube and is reviewing the design and positioning of tube fenestrations in order to minimise the risk of problems of this type.

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Optimum length of needles for internal jugular vein cannulation

We congratulate Dr Radhakrishna on his description of the use of a spinal needle to facilitate internal jugular vein cannulation (Radhakrishna. Anaesthesia 2000; 55: 1034–5). He used a spinal needle to facilitate venous location with the main needle after preliminary identification of the vein with a seeker needle.

He is right to point out the potential problem of vein location with the main needle after successful vein location with the seeker needle. The spinal needle method described would, however, only be needed if the incidence of difficulty in vein location with the main needle was significant. A previous study has shown that, after vein location with the seeker needle, two attempts with the main needle were needed in 12% of cases and three attempts were needed in 4% of cases; there was a 2% incidence of failure to locate the vein at all but only three attempts were permitted [1]. In this study, the cannulations were performed by inexperienced trainees and ultrasound was not used to demonstrate the anatomy.

It is likely that the incidence of difficulty in locating the vein with both the seeker and the main needle would be significantly lower if ultrasound was used to demonstrate the anatomy and experienced clinicians were performing the cannulations. Ultrasound is frequently used initially to demonstrate the anatomy and the procedure is then performed blindly. Probably the most common reason for this is that the sterile, disposable covers for the probe are unavailable.

Under these circumstances, we have observed that there can still be some difficulty in locating the vein with the main needle after successful location with the seeker needle. If the cannulation is performed using ultrasound under direct vision then Scott, an acknowledged expert, does not use a seeker needle [2]. He does, however, use a seeker needle if the cannulation is not performed under direct vision.

A technique using a spinal needle through the main needle was described in 1972 by Civetta and colleagues [3]. It is clear, however, that such a spinal needle technique in now rarely used. Experienced clinicians are able to cannulate the internal jugular vein without difficulty in most cases. If difficulty is experienced, they have the option of using ultrasound or trying an alternative vein. The use of a long needle should not be required for internal jugular vein cannulation because the skin vein distance is never longer than the length of a ‘green’ seeker needle (4 cm) provided that the needle is not advanced at a shallow angle to the skin.

The use of a long needle is inherently dangerous because inexperienced clinicians may be tempted to insert the needle into ‘tiger territory’ should they fail to locate the vein. This risk factor is present both when a spinal needle is used in this way and when a long cannula-over-needle device is used. The advantages of using a long spinal needle as described [3] must be balanced against the risks of using a long needle. Indeed, it could be argued that the needles provided in the kits for internal jugular vein cannulation should be the same length as the seeker needle (4 cm). If longer needles are used, increased safety would result from the introduction of cm markings on the needle, as is done with epidural needles. In order to enter the vein, the main needle should not be advanced more than 4 cm, provided the needle is advanced at an acute angle to the skin.

In his letter, Dr Radhakrishna suggests that a specially designed spinal/main needle combination should be produced by a manufacturer for safer internal jugular vein cannulation. We share his views. The needles in the kits currently provided are designed to be used for both internal jugular and subclavian vein cannulation; the extra needle length is designed for subclavian vein cannulation. It could certainly be argued that anaesthetists who frequently cannulate the internal jugular vein but rarely cannulate the subclavian vein would derive benefit from the modified needle. We would suggest that the main needle should be 4 cm in length and that the seeker needle should protrude only 3 cm past the end of the main needle. We have approached a manufacturer with a view to producing a modified seeker/main needle combination.

We note that Dr Radhakrishna successfully used the spinal needle technique on two difficult occasions. The use of ultrasound, if available, is especially indicated if difficulty in venous cannulation is predicted; however, such predictions are difficult to make. We appear to be in agreement that a seeker needle should always be used as part of a technique for safer internal jugular vein cannulation in the absence of ultrasound equipment.

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References
Catheter fixation problems – attention to detail required

Multiple lumen catheters are widely used; indeed, it is uncommon to find a critical care patient without at least one such line in situ. I wish to draw attention to a relatively simple detail of catheter fixation, which could have had potentially serious consequences and therefore merits wider coverage.

Most devices have one integral fixation site at the most proximal end of the catheter before it splits into component lumens (Fig. 9A), and then a further optional fixation clip, assembled on insertion of the line, and positioned distal to the permanent site to allow further stabilisation sutures and restrict displacement of the catheter on subsequent movement (see Fig. 9B).

I report the case of an inotrope-dependent patient on day 3 of their stay on intensive care, who became progressively hypotensive despite appropriate fluid and apparently increasing levels of inotropic support. The patient had a right internal jugular triple-lumen catheter in situ via which infusions of norepinephrine and dobutamine were being administered. On closer examination, it became evident that the catheter had been secured by sutures through site B only, leaving an appreciable section of the catheter unsupported and furthermore weighed down by the unattached site A. This had caused shearing forces just proximal to site B, which had eventually caused the catheter to split (Fig. 10), allowing infusions to take the path of least resistance and leak out onto the pillow away from the patient.

A fresh triple lumen was rewired and secured with sutures through sites A and B and the blood pressure successfully restored. The case highlights an important individual point and also serves to illustrate that attention to detail is required at all stages of a practical procedure, as the smallest deficiency in technique can result in failure of the whole procedure.

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Guillain–Barré syndrome may be associated with HIV infection

The recent case reports (Brooks et al. Anaesthesia 2000; 55: 894–8) of two pregnant women with Guillain–Barré syndrome (GBS) jogged my memory about work in central Africa. Ten years ago, a study in Zimbabwe found that 16 out of 29 GBS patients (55%) were human immunodeficiency virus (HIV) seropositive, compared with 4.3% HIV seroprevalence of blood donors in the same population [1]. An international review [2] and reports from Zambia [3] and Tanzania [4] had already drawn attention to GBS as a manifestation of HIV infection. The clinical features were similar to seronegative GBS, but the CSF was more likely to be pleocytotic [1, 2]. It was said in many institutions that HIV
infection was a frequent, even the most common, illness associated with GBS [1, 2, 5]. Retrospective data were collected on GBS patients admitted to the Intensive Care Units of the Harare Teaching Hospitals over a 28-month period up to March 1991: out of 14 patients, 10 were tested for HIV and seven found to be seropositive [6].

In the last few years, in parts of central, east and southern Africa up to 40% of pregnant women attending antenatal clinics are HIV seropositive [7]. I suggest that GBS occurring in HIV seropositive pregnant women may be underreported. The implications for the unborn baby are obvious.

The association of GBS and HIV-1, usually occurring at the time of seroconversion or during the phase of early infection (CD4 Category A), remains well recognised [8]. The same treatments – intensive care, plasmapheresis and infusions of human immune globulin – are effective [8]. In GBS, a history of a preceding flu-like illness and, especially, pleocytosis in the CSF, should prompt a search for HIV-1 infection; if initial serology is negative, follow-up testing may be appropriate [2, 8].

I wonder whether Brooks, Christian and May could reveal whether virology testing was undertaken in their second case. It would be interesting to receive their comments on this.

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References

A reply
We thank Dr McKenzie for pointing out the association between Guillain–Barré syndrome and HIV infection. In neither of our cases was serology testing for HIV performed. In the first case, virology confirmed recent infection with Epstein Barr virus and in the second case HIV testing was not felt to be indicated.

We agree that, in pregnancy, there should be a low index of suspicion for HIV testing for fetal reasons. It is now well recognised that appropriate anti-retroviral therapy in pregnancy, delivery by Caesarean section and avoidance of breast-feeding can significantly reduce vertical transmission to the fetus.

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Mendelson’s syndrome
The letter from Dr A. Ravalia quoting Mendelson’s paper in 1945 (Ravalia. Anaesthesia 2000; 55: 1040) demonstrates the continuing interest over many years of the effects of aspiration of stomach contents. The paper by Charles C. Hall [1], as noted by Dr Ravalia, details some experimental work carried out on rabbits in 1940 indicating that aspiration of acidic liquid material sets up a chemical pneumonitis. However, chemical pneumonitis may not be the complete explanation for the fatalities resulting from aspiration of liquid material. Work relating to sudden death in infants (cot deaths) carried out by Parish et al. on sensitised guinea-pigs suggests that an anaphylactic reaction might equally well be involved in these reactions [2].

Parish and his co-workers were testing the results of earlier investigations that had given rise to the belief that anaphylaxis did not occur when the subject was anaesthetised. Sensitised, anaesthetised guinea-pigs were challenged either by intravenous injection of 0.5 ml of a 1 in 2 dilution of antigen in saline, or by instilling 0.25 ml of the undiluted antigen over the larynx to be inhaled by the animal. The anaesthetic agents used were pentobarbital, di-ethyl ether, halogen in oxygen and nitrous oxide with trichlorethylene with oxygen, and carbon dioxide and oxygen. Death usually occurred between 3 and 5 min after challenge.

Of interest to anaesthetists was the finding that ether gave some protection from death due to anaphylaxis and that animals under pentobarbital narcosis showed none of the usual signs of obstructed breathing or respiratory arrest prior to death. The experiments demonstrated that the signs of anaphylaxis might become greatly modified under anaesthesia so that the reaction may not be recognised. Thus, it is possible that in humans, blood transfusions and the administration of drugs to which the patient is already sensitised may cause unrecognised anaphylaxis during surgical operations; so may the inhalation of vomit during operations if the stomach contains substances to which the patient is sensitised.

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**References**


**Patient-controlled oramorph – the future?**

I read with interest the letter ‘patient-controlled oramorph – the future?’ (Holt. *Anaesthesia* 2000; 55: 933–4). I am pleased that Dr Holt was impressed with our system of self-administered oral analgesia following Caesarean delivery. It is based on the ‘three-pot’ system of analgesia described in Chesterfield [1], combining paracetamol, an NSAID and an opiate. Oral morphine solution 10 mg in 5 ml is now dispensed in plastic single-dose vials (Oramorph®, Boehringer Ingelheim), which at this concentration is not a controlled drug. Although many hospitals still treat it as such, this is unnecessary and unhelpful. Dr Holt is right to question the preoccupation with measuring pain; the focus should be on function. We ask mothers whether they are able to feed and bathe their baby; and if not, why not? Of 30 mothers questioned in our baseline audit in 1996, 13 could not care for their baby, of whom 10 gave pain as the main reason. Following the introduction of self-administered oral analgesics, a re-audit in 1998 found that seven of 31 mothers could not care for their baby, with just one giving pain as the reason. This audit cycle was reported in ImpAct [2]. Although it generated considerable interest amongst pharmacists, pain nurses and audit coordinators, it seems to have remained largely unnoticed by anaesthetists.

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**Drug patient information leaflets**

I read with interest the letter concerning patient information leaflets (Paolini & Arrowsmith. *Anaesthesia* 2000; 55: 911). I agree that it is inappropriate to provide information to patients that can do little apart from increasing their anxiety. I wonder how many patients would agree to being given morphine when I patient information leaflet states ‘poor breathing, coma and death may occur’ (Medeva) in the paragraph ‘are there any side-effects?’ If anxiety levels are now leaving the patient with a feeling of impending doom the final line in bold type is ‘You may want to read this leaflet again.’ Would you? How many other drugs mention death in their patient information leaflets and do we really want to spend part of our valuable pre-operative assessment time trying to assuage the fears of our already anxious patients that anaesthesia is not as dangerous as all these patient information leaflets would have them believe?

A. J. Brookes
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**Confusing drug concentrations**

We read with concern the recommendations for treatment of hypocalcaemia in the review article ‘Calcium and the anaesthetist’ (Aguilera & Vaughan. *Anaesthesia* 2000; 55: 779–90). The authors advocate administration of 100–200 mg of elemental calcium over 10 min, given as calcium chloride or calcium gluconate, and then state ‘calcium chloride 10%: 10 ml (not 10 ml) = 27 mg = 0.68 mmol of elemental calcium’. Calcium chloride is also available as a 1 mmol.ml⁻¹ elemental calcium solution, which makes the calculations a little easier.

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**Amount of elemental calcium in calcium solutions**

I read with interest the review article ‘Calcium and the anaesthetist’ (Aguilera & Vaughan. *Anaesthesia* 2000; 55: 779–90). Most anaesthetists infrequently use intravenous calcium and appropriate doses can be difficult to remember. When intravenous calcium in most preparations is an odd amount of milligrams or millimoles. Unfortunately, the authors of this paper have fallen victim to this confusion, as they have misquoted the amount of elemental calcium in calcium chloride and calcium gluconate solutions. The correct amounts are:

- Calcium gluconate 10%: 1 ml (not 10 ml) = 9 mg of elemental calcium = 0.22 mmol elemental calcium.
- Calcium chloride 10%: 1 ml (not 10 ml) = 27 mg = 0.68 mmol of elemental calcium.

A reply

We would like to thank Dr Jeffreys for his interest in our review and for correcting the mistake regarding the amount of elemental calcium in both the chloride and gluconate solutions. We confirm that his observations are correct.

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**References**


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We read with concern the recommendations for treatment of hypocalcaemia in the review article ‘Calcium and the anaesthetist’ (Aguilera & Vaughan. *Anaesthesia* 2000; 55: 779–90). The authors advocate administration of 100–200 mg of elemental calcium over 10 min, given as calcium chloride or calcium gluconate, and then state ‘calcium chloride 10%: 10 ml = 27.2 mg of elemental calcium’. This would imply the administration of between 40 and 80 ml of 10% calcium chloride over 10 min. We hope, even inexperienced clinicians should identify this as a fatal dose of calcium, although it is unfortunate that the error has made it into print in a major medical journal. In fact, 1 ml (not 10 ml) of calcium chloride 10% contains 27.2 mg of...
elemental calcium. The same 10-fold error is made for calcium gluconate. This error highlights the continuing problem of drug concentration presentation. Expressions of drug amount per unit volume currently include those where units of both amount and volume are specified (e.g. 1 mg.ml\(^{-1}\); 1 mmol.l\(^{-1}\)) and those which require prior knowledge of the expression of dilution (e.g. 1% or 1 : 1000). In the case of the latter, Rolfe and Harper [1] have clearly shown that among hospital doctors, such knowledge is wholly inadequate. Recommendations that, where appropriate, drugs should be labelled with amount and unit volume specified [2] have not been heeded; yet everyday experience suggests that there has been little change from the level of knowledge identified by Rolfe and Harper. The traditional, yet poorly understood, expressions continue to be used and, most alarmingly, are particularly prevalent for drugs used in the resuscitation and emergency settings (e.g. epinephrine, lidocaine, calcium), where difficulty with the expression of concentration may lead to delay or administration of an inappropriate dose. This is unacceptable when a simple alternative exists. We believe that it is time for the resuscitation authorities to take a lead on this issue, so that all drugs prescribed in terms of amount per unit body weight are labelled, at the very least, an expression of concentration that specifies both amount and volume. A traditional expression of concentration could be used alongside if felt necessary. It is not acceptable to state that all doctors should be familiar with per cent or ratio expressions of concentration. Research and everyday experience show that they are not. The error made by Aguilera and Vaughan would not have occurred if ampoules of calcium chloride were labelled 100 mg.ml\(^{-1}\) (or better still labelled as 'elemental calcium 27.2 mg.ml\(^{-1}\)) rather than as 10%, since there would be no need to attempt a concentration conversion. When experienced clinicians preparing a review can make such a serious error, what hope is there for the house officer at 3 a.m. on the ward, having to manage his first emergency alone?

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References

Peripheral nerve injuries associated with anaesthesia

In their valuable review of nerve injuries associated with anaesthesia, Dr Sawyer and colleagues do not emphasise the danger of peripheral nerve compression associated with regional anaesthesia or analgesia (Sawyer *et al.* *Anaesthesia* 2000; 55: 980–91). Peripheral nerve compression may cause nerve injury in such circumstances for two principal reasons.

1 Nerve compression may cause neurapraxia that is masked by local anaesthetic-induced nerve conduction blockade.
2 Signs of nerve compression (numbness, paraesthesiae and muscle weakness) are wrongly attributed by carers to pharmacological blockade. Thus the condition fails to provoke the necessary change of position, and the compression is allowed to continue long enough to cause a persistent neuropathy. Such a mechanism is described for peroneal [1] and sciatic [2] nerve damage.

With increasing use of regional blockade to provide postoperative analgesia, the risk of nerve compression deserves attention. The use of low-dose combinations of local anaesthetic and opioid, or better still opioid alone, should avoid such confusion, as signs of nerve compression should not be masked by pharmacological blockade. To profit from this advantage, however, it becomes crucially important to educate nurses and midwives that regional blockade by opioids or low-dose combinations should not produce profound numbness or weakness.

It is disturbing to note that the value, and the relative safety, of neuraxial opioids are still not universally appreciated, and that in some centres local anaesthetics are still used alone, necessitating relatively large doses, to provide postoperative analgesia. Such management has the disadvantages of persistent numbness and poor mobilisation, potential hypotension and urinary retention and, as recently reported, pressure sores [3]. Moreover, local anaesthetics are far more dangerous than opioids if misplaced either intrathecally or intravenously. Unlike local anaesthetics, neuraxial opioids do not feature in the reports on confidential enquiries into maternal deaths.

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References

Detecting the epidural space

We wish to thank Dr da Silva for his constructive comments highlighting the shortcomings of our idea as an aid to detect the epidural space (da Silva, *Anaesthesia* 2000; 55: 1041). We fully agree with the six points he highlighted and, independently, we have already modified our method. The current, fourth prototype is far less cumbersome, does not have multiple connections and is lightweight, and indeed may prove to be useful to beginners.

The problem to date is that we have failed to muster technical support from industry. Our experience has highlighted once again how difficult it is to transform a new innovative idea into a real life model, which can then be tested.
for flaws. We believe that the success of regional anaesthesia will depend on the ease with which the masses accept a technique. Sadly, technological developments have not always kept pace in making the practice of regional anaesthesia any easier, and ours was an attempt to challenge this.

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SuprACLAVICULAR BLOCK FOR
DAY-CASE ANAESTHESIA AT ALTITUDE

We read with interest the article by Pande and colleagues (Pande et al. Anaesthesia 2000; 55: 798–810). Paediatric regional anaesthesia has gone through significant development with increasing popularity in recent years [1]. We have, however, some concerns regarding the use of the classical supraclavicular brachial plexus block as the anaesthetic technique for day-case surgery in children. There is a reported incidence of pneumothorax (0.5–6%) with the supraclavicular brachial plexus in the literature [2]. The pneumothorax is usually small and slow developing [3]. The supraclavicular block has traditionally been seen for this reason to be contraindicated in day-case surgery.

In this particular study the block was carried out at altitude (‘eastern hills of Nepal’). Air in the interpleural space is a concern for the transport of patients by air due to the possible low-pressure environment [4]. At altitude and more specifically at lower atmospheric pressure any air-contained spaces will expand. In this study all patients were reviewed the morning after surgery and clinically deemed not to have a pneumothorax. If the pleura is punctured at altitude and patients discharged on a day-case basis, could a pneumothorax in this setting develop more rapidly or with a different expected course, with serious consequences?

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References

A reply

We appreciate the concern of Harmon and colleagues regarding the potential problem of pneumothorax in children undergoing supraclavicular brachial plexus block. However, the literature data on its incidence are old [1, 2] and do not strictly refer to the paediatric population.

We fully agree with Harmon et al. and feel that supraclavicular block would be inappropriate in the day care setting for inexperienced personnel and should be taken as a potentially high-risk procedure. Regarding the potential problem of expansion of pneumothorax due to low barometric pressure at high altitude, we would like to state that our institution is located in the foothills at an altitude of 610 m above sea level and any such risk is minimal.

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References