**Tracheal intubation in trauma**

Sir,—The article by Nolan and Parr1 depicted in-line cervical stabilization and cricoid pressure during tracheal intubation in the trauma patient (fig. 2). In figure 2, in-line cervical stabilization is demonstrated as if it were a single-handed technique. It is accepted practice, however, to perform this manoeuvre as a double-handed technique (although we are sure this is the effect of the line drawing rather than the authors’ practice). The same figure also depicted single-handed cricoid pressure. There has been some controversy in the literature regarding the practice of cricoid pressure in the trauma patient with potential cervical spine injury. Some authors have claimed that there is no difference in the view obtained of the larynx whereas others assert that bimanual cricoid pressure enhances the view of the larynx.3 There is debate on the safety and efficacy of either technique on the stability of the cervical spine during tracheal intubation.

In a recent investigation,4 Gabbott examined movement of the cervical spine with single-handed cricoid pressure in subjects in whom the trachea was intubated, although unfortunately he did not compare it with bimanual cricoid pressure. In the discussion that follows, he makes recommendations that we would agree with in the management of the potentially unstable cervical spine during intubation. We would suggest that if a hard cervical collar (particularly the two-piece collar) has been applied before tracheal intubation, then the posterior piece provides adequate support for the back of the neck and single-handed cricoid pressure may then be applied. Indeed, to remove the collar may risk additional movement of the cervical spine.5 If a hard collar is not in place then bimanual cricoid pressure is probably the safer practice.

S. N. FLETCHER
J. M. McNEILL
Department of Anaesthesia
Royal London Hospital
Whitechapel, London


**Calculating catecholamine extraction from plasma**

Sir,—We would like to comment on the article by James and colleagues.1 They measured flow rate and catecholamine concentrations in the inflow and outflow blood across several organs, and calculated the extraction ratio (ER) of catecholamines in each organ. They reported that the ER of norepinephrine by the porcine liver was 30 ± 11 %. However, it has been established that every organ, with the exception of the brain, not only extracts catecholamines from plasma but also releases catecholamines into plasma.2 This implies that plasma catecholamine concentration is determined by the balance between its spillover into the systemic circulation from sympathetic nerve terminals and its disposal from the circulation. Therefore, the ER values obtained by James and colleagues cannot be considered real, but only as “net” values.

To obtain a true ER by an organ, an isotope dilution method should also be performed. For example, Åneman and colleagues have combined a conventional and isotope dilution method, obtaining an ER for norepinephrine in the human liver of 86 ± 6 %.3 Although species difference should be taken into account, there is a possibility that James and colleagues underestimated the ER values in each organ. Whatever the existing methodological problems, we agree with the hypothesis that the liver plays an important role in clearing plasma norepinephrine, especially in restricting the amount reaching the systemic circulation from the mesenteric organs.

K. BRITA
S. TAKAHASHI
Department of Anaesthesiology and Critical Care Medicine
Faculty of Medicine
Kyushu University
Fukuoka, Japan


SIR,—We are in full agreement with the comment of Irita and Takahashi that our measurements reflect net extraction ratio of catecholamines and not “true” extraction ratio. This distinction was, in fact, referred to briefly in our article. Our interest in this study was to establish the effects of various organs on circulating plasma catecholamine concentrations in response to heptectomy and we were therefore interested primarily in the net value. Although we are aware of the isotope studies and their importance in terms of establishing the real rate of removal of catecholamines by any organ, we are happy that the net effect which we examined was appropriate for our study. We concur with the observation by these authors of the importance of the liver in catecholamine metabolism.

M. F. M. JAMES
Department of Anaesthesia
University of Cape Town
South Africa

Extradural buprenorphine and breast feeding after Caesarean section

SIR,—I would like to comment on the article on extradural buprenorphine and breast feeding after Caesarean section by Hirose, Hosokawa and Tanaka.1 In this study, the authors administered 0.25% bupivacaine 0.7 ml h−1 or bupivacaine and buprenorphine 12 μg ml−1 in the control study and groups, respectively, as a continuous infusion for 3 days. They also administered dicyclomine suppositories 25 mg for additional analgesia. I am curious why the authors did not choose patient-controlled analgesia (PCA).2 Another intriguing point is why the patients were kept in hospital for 11 days. It seems that the authors had no concern as to the financial and psychological costs of this prolonged hospitalization.

The authors published a study with similar results in 1996 in Anesthesia and Analgesia.3 Despite the fact that the authors used spinal anaesthesia in addition to continuous extradural analgesia in their previous study, the results were similar.

The amount of breast milk is dependent on factors such as quantity of glandular tissue in the breast, nutritional status of the mother and the psychological status of the mother.4 I assume that the psychological burden on the patients hospitalized for 11 days was significant. The authors seemed to have ignored this issue.

On reviewing their references, it noteworthy that there is a lack of studies on this subject.

B. ÇELBIOGLU
Department of Anaesthesiology
Hacettepe University Medical School
Ankara, Turkey


SIR,—Dr Celebioglu questioned why we did not use patient-controlled analgesia (PCA) in our studies.2 This was because we did not have enough PCA pumps for all of our subjects.

Dr Celebioglu also noted the prolonged hospitalization after Caesarean section in our studies,3 which was the key point in obtaining our data on the total weight of breast milk. The length of hospitalization after Caesarean section was 12 or 13 days in our studies, which is longer than that reported in other countries. Similarly, mean duration of hospitalization after laparoscopic and open cholecystectomy was reported as 11.4 and 35.5 days in Japan,4 but only 1.8 and 2.8 days in Sweden.5 In Japan, doctors and related professionals are well aware of the fact that postoperative hospital stay is relatively prolonged compared with other nations. This prolonged hospitalization is partly a result of the Japanese health care system. It may have affected breast feeding psychologically after Caesarean section, but it had no effect on between-group evaluation of our data, all of which were obtained during the same hospital stay.

We appreciate that Dr Celebioglu compared our two studies. Postoperative analgesia was performed with or without extradural bupivacaine in the first study, and in the second study it was performed with extradural bupivacaine or extradural bupivacaine plus buprenorphine.6 After we reported that extradural bupivacaine after Caesarean section improved breast feeding,1 many clinical researchers asked us about the effect of extradural opioids on breast feeding. The second study was designed to evaluate the effect of extradural buprenorphine on breast feeding after Caesarean section. Taken together, breast feeding with postoperative extradural bupivacaine and buprenorphine was suppressed to the same level as that without extradural analgesia after Caesarean section.

Why the Y?

SIR,—Perhaps it is a sign of secular times but, in searching for the essential Y. J. Simpson, and putting a modern “spin” on his life, Rue and Wildsmith7 have missed the importance of religious faith to the era, the environs and his life.8 It was a life lived during one of turmoil in Scotland’s theological history. The defence of anaethesia, on biblical textural grounds, is all the more poignant for the fundamental break-away, Free Church.2 It may sound pious to the era, the environs and his life.2 It was a life lived during one of turmoil in Scotland’s theological history. The defence of anaethesia, on biblical textural grounds, is all the more poignant for the fact that Simpson was active in the disruption of the Church of Scotland in 1843 and is identified with the more radical, and fundamental break-away, Free Church.2 It may sound pious to the era, the environs and his life.2 It was a life lived during one of turmoil in Scotland’s theological history. The defence of anaethesia, on biblical textural grounds, is all the more poignant for the fact that Simpson was active in the disruption of the Church of Scotland in 1843 and is identified with the more radical, and fundamental break-away, Free Church.2 It may sound pious to the era, the environs and his life.2


readers would have known all about. Armstrong Davison’s “story” has gone to his grave. Or has it?

I. D. CONACHER
Department of Cardiothoracic Anaesthesia
Freeman Hospitals’ NHS Trust
Newcastle upon Tyne


Sir,—We thank Dr Conacher for his interest in, and comments on, our editorial. It was produced to indicate why the sesquicentenary celebration in Edinburgh was used to mark Simpson’s “whole contribution to medicine” rather than just the anniversary of the first anaesthetic use of chloroform. We did refer to Simpson’s “extensive knowledge of the bible” where it was relevant, but deliberately eschewed considering his involvement in the religious controversies of the day; these were not immediately relevant to our theme. In addition, we do appreciate that an incorrect reference to such controversy might have produced even more vigorous comment than Dr Conacher’s.

The source of the sobriquet “Young” seems self-evident to us. He was the youngest of nine children, born 4 yr after his immediate senior—surely he was neither the first nor the last such child to be nicknamed “Young”. The really intriguing aspect is why Armstrong Davison and Sykes were so obsessed with the issue. Both used it to denigrate Simpson, but neither provided any primary source information—very uncharacteristic behaviour for such “assiduous historians”. Thus we too would like an answer to Dr Conacher’s final question, but we wonder if it is simply the historical equivalent of the “tall poppy” syndrome, precipitated by their clear opinion that Simpson had not been as assiduous as he might in deflecting the credit he was often given for “discovering” anaesthesia.

S. M. RAE
J. A. W. WILDSMITH
University Department of Anaesthesia
 Ninewells Hospital and Medical School
Dundee

Propofol in paediatric intensive care: further evaluation is justified

Sir,—We were interested to read the study by Martin, Murthy and Petros1 suggesting that infusion of propofol may be used safely to sedate critically ill children after cardiac surgery, and support the recommendation by Hatch1 that larger controlled studies need to be undertaken to develop criteria for its safe use.2 The ICU at Frenchay hospital admits 60–80 children per annum, mainly after head injury or with other neurosurgical/neurological diagnoses. We believe that continuous infusion of propofol offers advantages in sedating these children, as it allows rapid changes in the level of sedation, facilitating rapid and repeated assessment of the child’s level of consciousness and neurological function.

We audited our experience with the use of propofol infusions to sedate children over a 3-yr period.3 Propofol was infused alone or in combination with an opioid. The maximum infusion rate of propofol allowed was 6 mg kg⁻¹ h⁻¹. If this failed to achieve adequate sedation, propofol was discontinued and midazolam was used. No child received propofol if there was evidence of respiratory tract infection, and the infusion was discontinued if the child developed lipoaemic serum. There were 158 children admitted, 71 of whom received propofol for sedation, usually in combination with an infusion of morphine (74.6% of children). Median age was 9.5 yr (range 10 months to 15 yr). All children were admitted after head injury or with another neurosurgical/neurological diagnosis, except for three children who were admitted with severe burns.

Median rate of infusion of propofol was 3.1 (range 0.6–6.0) mg kg⁻¹ h⁻¹ for a median duration of 16 (range 2–149) h. All children were adequately sedated except for two in whom midazolam was substituted for propofol when 6 mg kg⁻¹ h⁻¹ failed to sedate the child adequately. All children were easy to assess neurologically by simply reducing the rate of infusion of propofol. Median time to extubation after stopping the propofol infusion was 40 (range 10–510) min. No child developed lipoaemic serum, unexplained acidosis, hepatic or cardiac failure.

The dose of propofol used was slightly more than the median rate of infusion of 2.1 mg kg⁻¹ h⁻¹ reported by Martin, Murthy and Petros.1 This probably reflects the deeper level of sedation required in the management of patients with head injuries or other diseases likely to cause increased intracranial pressure. In every other aspect our experiences were similar. We support the conclusion of Martin, Murthy and Petros that propofol may be used safely to sedate children receiving intensive care provided that the maximum infusion rate is not exceeded. The criteria they recommend for the safe use of propofol in children should be used as the basis of further larger studies evaluating the use of the drug to sedate other groups of critically ill children in addition to the postoperative cardiac surgical population.

A. E. R. YOUNG
Royal Children’s Hospital
Melbourne, Australia

A. R. MANARA
Intensive Care Unit
Frenchay Hospital
Bristol


Sir,—We welcome the comments of Young and Manara. Propofol continues to be used in the PICU throughout the UK and we are pleased to read that its sensible use has produced no problems. We are in the process of setting up a prospective study on the use of propofol in the PICU and the incidence of propofol associated metabolic acidosis, and would welcome collaboration with other centres.

A. PETROS
Great Ormond Street Hospital for Children
NHS Trust and the Institute of Child Health
London

Sevoflurane in acute airway obstruction

Sir,—I was interested to read Mostafa and Atherton’s report on the use of sevoflurane in patients with difficult airways.1 They remarked how rapidly deep anaesthesia was achieved, although none of their patients had a significant respiratory obstruction. I recently anaesthetized a 75-yr-old woman with stridor caused by laryngeal carcinoma. She was obese and a heavy smoker. She had refused any procedure under local anaesthesia. After administration of nebulized epinephrine, anaesthesia was induced with sevoflurane in 100% oxygen. Starting at a concentration of 2%, this was increased in increments to 8%. Loss of consciousness was rapid (less than 1 min). After 2 min, I attempted laryngoscopy, but the patient gagged. A similar incident occurred at 5 min. In the meantime, her airway was becoming increasingly difficult to hold owing to loss of muscle tone in a bulky neck. Finally, after 10 min, laryngoscopy was possible; the vocal cords could be seen and succinylcholine was given. Intubation, however, was unsuccessful and necessitated intervention of the ENT surgeon with a rigid laryngoscope. The improved view allowed intubation with a size 6 tracheal tube.

Deep anaesthesia with sevoflurane was hard to achieve in this situation. It was better than with halothane. Sevoflurane has a
lower blood-gas solubility, but is much less potent than halothane. In the case of respiratory obstruction, is potency as important as solubility?

P. Board
Anaesthetics Department
South Cleveland Hospital
Middlesbrough


Sir,—I was interested to read about the three patients with expected difficult intubation in whom inhalation induction with sevoflurane proved useful in airway management. Since the introduction of sevoflurane in Singapore, I have found this agent to be similarly useful in patients with head and neck tumours presenting for examination under anaesthesia, biopsies or surgery. Two cases may be worth mentioning.

An obese 56-year-old woman presented with a massive multinodular goitre for thyroidectomy. Awake fibreoptic intubation proved impossible because the patient became distressed, uncooperative and hypoxic during the attempt. After inhalation induction with sevoflurane up to 6% in 100% oxygen, direct laryngoscopy was undertaken to reveal a grade 4 larynx.2 Blind passage of a gum elastic bougie into the trachea was suggested by the patient’s cough and confirmed by capnography when a tracheal tube was successfully “railroaded”. A 48-year old man presented with a nasopharyngeal tumour for examination under anaesthesia and biopsy. After inhalation induction of anaesthesia with sevoflurane in oxygen, direct laryngoscopy was performed. A haemorrhagic tumour was found occluding the laryngeal inlet. Intubation was not attempted and spontaneous ventilation was maintained using sevoflurane in oxygen while the surgeon proceeded to perform a tracheostomy.

I agree with Mostafa and Atherton that sevoflurane has replaced halothane for inhalation induction of anaesthesia in the management of the difficult airway. I have found no advantages in the use of nitrous oxide in this particular situation.

P. C. Ip-Yam
Department of Anaesthesia
Singapore General Hospital
Singapore


Sir,—Thank you for the opportunity to reply to the letters of Dr Board and Dr Ip-Yam. Our interest in using sevoflurane in the management of patients with difficult airways was not how rapidly it achieved anaesthesia, albeit important, as much as how easily anaesthesia was accomplished and how the airway was maintained without difficulty. It is pertinent to state that our experience is not limited to the three cases we described. Our impression is supported by Dr Ip-Yam’s letter describing the use of sevoflurane in the management of children and adults with difficult airways. Our experience is also corroborated by the recent report of Frerk and ‘Tordoff’ in which they described the management of three patients with difficult airways. Furthermore, and perhaps most important, the low blood-gas solubility of sevoflurane implies that, if difficulty is encountered with induction, it should be easier and quicker to lighten anaesthesia and wake the patient. This is the great advantage of inhalation induction.

Dr Board unfortunately raises the wrong question. In certain cases of stridor, such as in the patient he described, the choice for securing the airway should lie between awake intubation and tracheostomy under local anaesthesia, not the potency or solubility of a general anaesthetic agent. In our view, such a patient should not be anaesthetized before securing the airway. General anaesthesia may reduce muscle tone which can lead to complete airway obstruction. But sometimes our approach is not feasible because of lack of cooperation of the patient, or because of lack of an anaesthetist skilled in awake intubation.1 In addition, the use of halothane may be contraindicated, as in the cases we described. In these circumstances the use of sevoflurane for inhalation induction may offer the safest solution. As Dr Board had not used halothane when he encountered difficulties it would be impossible for anyone to judge if it or sevoflurane was the better agent.

In conclusion, we would maintain that inhalation induction with sevoflurane is a useful technique in the armamentarium of anaesthetists for the management of patients with difficult airways.

S. M. Mostafa
A. M. J. Atherton
Royal Liverpool University Hospital
Liverpool


Anaesthesia for surgery of emphysema

Sir,—Dr Conacher’s comprehensive review article on anaesthesia for surgery for emphysema1 is marred by the advocacy of extradural analgesia as the only vital analgesic technique.

Paravertebral analgesia blocks not only the intercostal nerves but also the sympathetic chain and block posterior primary rami, providing equally effective postoperative analgesia in addition to inhibiting the stress response to surgery.2 It has fewer side effects than extradural analgesia,3 4 and is safer and technically easier to perform.1 By producing only unilateral block, paravertebral analgesia avoids hypotension and thus the need to administer excessive i.v. fluids or vasopressors to maintain tissue perfusion.1 Balanced analgesia, by blocking both peripheral and central pain mechanisms, has also been recommended in the form of preincisional paravertebral block, opioid premedication, preoperative non-steroidal anti-inflammatory drugs (NSAID), is one of several options for analgesia that could be justified, on the grounds suggested, for patients whose pulmonary condition renders them suitable for routine thoracotomy or thoracoscopy. However, for patients with bilateral emphysematous conditions, verging on respiratory failure, undergoing lung transplantation, volume reduction surgery or thoracic surgery, the technique does not have a track record and would not be viewed as best practice.

For the last group of patients (those most likely to be encountered by the non-specialist in the middle of the night) the risk of pneumothorax5 in a vulnerable population would have to be added to the dangers caused by the balancing of analgesia with NSAID. The use of non-essential drugs that can precipitate renal failure in the presence of low volume urine production are viewed askance by those caring for patients in whom one organ is already failing. These patients may require toxic immunosuppressants, and a vital element in their recovery is fluid restriction. The international consensus, from the review I undertook, is that extradural analgesia is the gold standard and I would not, at this time in the evolution of the third age of thoracic anaesthesia,2 suggest anything less except in failure to achieve that standard. In which case, there are safer options than paravertebral block and NSAID.

L. Lafrenière
Department of Anaesthetics
Leeds General Infirmary
Leeds


Sir,—I would agree with Dr Lafrenière that paravertebral block, balanced with opioids and non-steroidal anti-inflammatory drugs (NSAID), is one of several options for analgesia that could be justified, on the grounds suggested, for patients whose pulmonary condition renders them suitable for routine thoracotomy or thoracoscopy. However, for patients with bilateral emphysematous conditions, verging on respiratory failure, undergoing lung transplantation, volume reduction surgery or thoracic surgery, the technique does not have a track record and would not be viewed as best practice.

For the last group of patients (those most likely to be encountered by the non-specialist in the middle of the night) the risk of pneumothorax in a vulnerable population would have to be added to the dangers caused by the balancing of analgesia with NSAID. The use of non-essential drugs that can precipitate renal failure in the presence of low volume urine production are viewed askance by those caring for patients in whom one organ is already failing. These patients may require toxic immunosuppressants, and a vital element in their recovery is fluid restriction. The international consensus, from the review I undertook, is that extradural analgesia is the gold standard and I would not, at this time in the evolution of the third age of thoracic anaesthesia, suggest anything less except in failure to achieve that standard. In which case, there are safer options than paravertebral block and NSAID.

I. D. Conacher
Department of Cardiothoracic Anaesthesia
Freeman Hospitals NHS Trust
Newcastle upon Tyne


Suboptimal use of inhaled aerosol therapy during mechanical ventilation in intensive therapy units in the UK and Ireland

Sir,—Aerosol delivery of nebulized drugs appears less efficient than in patients breathing spontaneously and is highly dependent on the method used.1 2 To examine the pattern of use of aerosol drug therapy during mechanical ventilation within intensive care units in the UK and Republic of Ireland, and to establish how frequently proven methods of optimizing drug delivery were used, a postal questionnaire was sent to consultants in those intensive care or high dependency units listed in the 1995 Directory of Emergency and Special Care Units in the UK and Ireland, between March and October 1995. In the event of no response, after 3 months a reminder letter was sent. The questionnaire enquired about the size and type of the intensive care unit and number of patients treated. Details were sought about the use of aerosol therapy during mechanical ventilation, including drugs, ventilators and methods of nebulization used.

Responses were received from 175 of 213 (82%) units. All had used aerosol drug therapy during mechanical ventilation within the last year and 168 units had used it within the previous month. Drugs used in the previous month included beta, adrenoceptor agonists (168 units), anticholinergic drugs (140 units), corticosteroids (57 units), antibiotic or antiviral agents (18 units), prosta
cyclin (13 units) and surfactants (three units). Most (162) units used jet nebulizers, while the simpler, less expensive and possibly more efficient3 metered dose inhalers were used less often (34 units). Although very efficient,4 ucspasonic nebulizers were used rarely (six units).

Methods of enhancing aerosol delivery were used infrequently (table 1). Most units did not position the nebulizer on the inspira
tory limb of the breathing system and few used spacers, even though combining these methods may double aerosol delivery to the patient.14 Less than 50% of the responding units used a high drug solution volume in jet or ucspasonic nebulizers, even though delivery may be doubled by increasing the volume of the nebulizer solution from 3 to 6 ml.3 Most units did not adjust ventilator settings during nebulization and may thus have inadvertently altered the conditions of ventilation, including respiratory minute volume.1 Discontinuing humidification during aerosol administration improves delivery5 but was performed infrequently. Most units removed nebulizer apparatus from the breathing system when not in use, which is good practice as it reduces the risk of bacterial contamination.1

In spite of the drawbacks in using a postal survey to study the management of patients, we believe the results are a valid reflection of recent practice in the UK and Ireland as the response rate was high. It showed that aerosol drug therapy is used often during mechanical ventilation but the methods used are suboptimal and could easily be improved by some simple modifi
cations. The British Thoracic Society has recently issued guidelines on nebulizer use during mechanical ventilation.6 Adoption of these guidelines will improve aerosol drug delivery to patients undergoing ventilation in intensive care units, and this is expected to improve the effectiveness of drugs administered by this route.

Table 1 Numbers (%) of units using methods which enhance aerosol delivery during mechanical ventilation. *In units who administered these drugs within the past year

<table>
<thead>
<tr>
<th>Method of improving delivery</th>
<th>Users (% responders)</th>
<th>Non-users</th>
<th>No response</th>
</tr>
</thead>
<tbody>
<tr>
<td>Jet nebulizers (162 units)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Placement on inspiratory limb of ventilator system</td>
<td>43 (27%)</td>
<td>115</td>
<td>4</td>
</tr>
<tr>
<td>Placement 10–30 cm from Y-piece on inspiratory limb</td>
<td>8 (5%)</td>
<td>150</td>
<td>4</td>
</tr>
<tr>
<td>Use of spacer</td>
<td>5 (3%)</td>
<td>157</td>
<td>0</td>
</tr>
<tr>
<td>Use of ≥ 5 ml nebulizer solution</td>
<td>66 (43%)</td>
<td>89</td>
<td>7</td>
</tr>
<tr>
<td>Use of inspiratory phased jet nebulization</td>
<td>67 (41%)</td>
<td>95</td>
<td>0</td>
</tr>
<tr>
<td>Interruption of humidification</td>
<td>31 (22%)</td>
<td>109</td>
<td>22</td>
</tr>
<tr>
<td>Adjustment of ventilator settings</td>
<td>46 (29%)</td>
<td>114</td>
<td>2</td>
</tr>
<tr>
<td>Nebulizer apparatus removed between use</td>
<td>153 (94%)</td>
<td>9</td>
<td>0</td>
</tr>
<tr>
<td>Metered dose inhalers (34 units)</td>
<td>Use for administering beta agonists*</td>
<td>27 (79%)</td>
<td>7</td>
</tr>
<tr>
<td>Use for administering anticholinergic drugs*</td>
<td>20 (59%)</td>
<td>14</td>
<td>0</td>
</tr>
<tr>
<td>Use for administering corticosteroids*</td>
<td>20 (38%)</td>
<td>14</td>
<td>0</td>
</tr>
<tr>
<td>Placement on inspiratory limb of ventilator system</td>
<td>13 (38%)</td>
<td>21</td>
<td>0</td>
</tr>
<tr>
<td>Placement 10–30 cm from Y-piece on inspiratory limb</td>
<td>2 (6%)</td>
<td>32</td>
<td>0</td>
</tr>
<tr>
<td>Use of spacer</td>
<td>7 (21%)</td>
<td>27</td>
<td>0</td>
</tr>
<tr>
<td>Interruption of humidification</td>
<td>6 (21%)</td>
<td>22</td>
<td>6</td>
</tr>
</tbody>
</table>

S. H. L. THOMAS
Wolfson Unit of Clinical Pharmacology
University of Newcastle upon Tyne

C. J. PAGE
S. F. BARRINGTON
M. J. O’DOHERTY
Department of Nuclear Medicine
St Thomas’ Hospital
London