Complications of Rigid Laryngoscopy and Tracheal Intubation

To the Editor:

We read with great interest the article of Lee JJ *et al.* that describes two complications of tracheal intubation in a neonate.[1]

We believe, however, that these two complications could have been avoided if the following precautions and techniques were used. (1) Tracheal intubation was achieved after three esophageal intubations. It was not mentioned if the esophageal intubation was done because of difficult airway or involved an endoscopist not experienced in neonatal intubation. If the case was difficult airway and the glottis was not seen, a supraglottic airway such as an Air-Q™ #1.0 (Clearwater, FL) could have been immediately inserted to provide ventilation. If an endotracheal intubation is mandatory, a fiberoptic-guided tracheal intubation through the Air-Q™ can be performed.[2] (2) Proper placement of the endotracheal tube (ETT) to 8 cm distance at the lips could have avoided the right endobronchial intubation if the ETT was pulled to 8 cm at the lips and not left at 11 cm while starting positive pressure ventilation. (3) Proper placement of the ETT can be confirmed by: bilateral breath sounds, capnogrpahy, and insertion of a lubricated ultra-thin fiberoptic scope through the ETT (Olympus LF-P [Center Valley, PA] with a 2.2 mm external diameter) and confirming placement of the ETT (1.0–1.5 cm) above the carina in this case. If these precautions were followed, the endobronchial intubation and unnecessary surgery for gastric perforation from esophageal intubation could have been avoided. The authors have currently concluded that specialized training and experience are needed for neonatal airway management. However, use of a supraglottic airway as described above improves the likelihood of successful airway management by less experienced clinicians.

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In Reply:

We read with great interest the letter from El-Ganzouri and Ads. They made helpful comments on our report.[1] It is unfortunate that we did not mention why the complications were developed.

The infant at 33-weeks gestation who weighed 2,050 g was delivered at a private hospital. At birth, the baby was dyspneic, and tracheal intubation was attempted by a physician who lacked much experience in neonatal intubation. An endotracheal tube was inserted into the esophagus three times, resulting in marked abdominal distension. Tracheal intubation was successful on the fourth attempt. After 80 min of ventilator care, the baby was tachycardic with an oxygen saturation of 80–90%. He was transferred to our hospital.

I agree that these complications were made more likely by the prior esophageal intubations by a less experienced clinician.

However, all hospitals, especially private hospitals, don't have experts in airway management available at all times.

The main message of our report remains that immediately after intubation, adequate placement and depth of the endotracheal tube should be confirmed using end-tidal carbon dioxide, auscultation, endotracheal tube depth, and chest x-ray.

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Cognitive Dysfunction after Inhalation *versus* Intravenous Anesthesia in Elderly Patients

To the Editor:

We would like to acknowledge the contribution of Cai *et al.* in their January 2012 publication "Association between apolipoprotein E4 and postoperative cognitive dysfunction in

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elderly patients undergoing intravenous anesthesia and inhalation anesthesia."[1] Prospective studies in this realm are sorely lacking, and a study of this magnitude is indeed an achievement. However, we do have some questions about the methodology and external validity of their findings.

In a prospective cohort of elderly patients undergoing general anesthesia and major noncardiac surgery, Cai *et al.* demonstrated enormously disparate postanesthesia Mini-Mental State Examination (MMSE) scores between patients who received total intravenous anesthesia and those who received isoflurane. The MMSE includes questions about orientation to time and place (which may include specific information like the floor of the building the patient is in), immediate and delayed recall of words, attention and calculation, language skills, and ability to follow complex commands, such as copying a figure. Although the MMSE is not a very descriptive tool for perioperative cognition, extreme scores have value. It is unlikely that a patient with a perfect score (30) is disoriented, but patients in the range of 25–30 may comprise a broad range of cognitive ability. In their analysis of apolipoprotein E genotype effects, there was a relatively higher incidence of apolipoprotein E ε4 allele in patients who received isoflurane (but not total intravenous anesthesia) and had MMSE scores less than 25 at any time-point postoperatively. The authors conclude this suggests that apolipoprotein E ε4 allele is a risk factor for cognitive dysfunction in patients who receive inhalational anesthesia. The possibility remains that significant variance may be explained by baseline MMSE status in these groups, as patients who move from a score of 25 preanesthesia to 24 postanesthesia would be classified the same as patients who moved from a score of 30 to 10. The causal link between exposure to an anesthetic and cognitive dysfunction would be more clearly represented as change from preoperative baseline score.

Irrespective of the effects of apolipoprotein E genotype, the effects of isoflurane anesthesia on postoperative MMSE reported by Cai *et al.* are not subtle. In figure 2, it appears that profound cognitive impairment persisted to at least 3 days after surgery in the group that received isoflurane.† Regarding the clinical correlate of the MMSE scores seen, patients with early Alzheimer's disease may score 24 or less, scores between 10 and 19 indicate moderate dementia, and scores less than 10 generally denote severe impairment. In this study patients did not recover to a score above 20, which still represents significant impairment, until somewhere between 3 and 10 days postoperatively. In other words, patients who were generally healthy with MMSE of 25 or more (and mean more than 27) and no major heart, lung, or renal dysfunction became floridly demented for at least 3 days after surgery following isoflurane anesthesia. We find this extremely unusual. In many cases we would expect patients to be conversational and ambulatory by the end of 48 h after procedures that included nephrectomy, gastrectomy, and esophagectomy. If this type of major impairment persisted at our institution, where more than 80% of elderly patients receive isoflurane for major surgery, it would prevent mobilization of a massive number of postsurgical patients and create a huge public health problem.

Very little is discussed regarding the anesthetic technique in the study by Cai *et al.* Depth of anesthesia is described as only actively managed before intubation and otherwise is noted to have been "normal." It is unclear if the two groups have equivalent anesthetic depths throughout the procedure, which could have accounted for at least the early difference but not the prolonged recovery. Alarmingly, the authors state that patients in the inhalation group, after a loading dose of propofol, received "continuous inhalation [of] 2–3% end-tidal concentration of isoflurane, which was used for maintenance of anesthesia." Using equation 3 of Nickalls and Mapleson,[2] this expired concentration of isoflurane represents a range of 2.06 to 3.09 minimum alveolar concentration of isoflurane for a 70-yr-old (the mean age of patients in their study). The carrier gas is not specified, but if it contained nitrous oxide, the effective minimum alveolar concentration would be much higher. The minimum alveolar concentration value of isoflurane in 100% O_2 in this age group is 0.97; with 50% N_2O, the minimum alveolar concentration of isoflurane is 0.41. Thus, their results may reflect a toxic effect of administration of a very high dose of isoflurane. In contrast, the group that received intravenous anesthesia had age-appropriate serum concentration of propofol,[3] around 1 μg/ml. Postoperative visual analog scale score, incidence of delirium, and postoperative complications by group were also not mentioned. Intraoperative physiologic parameters, such as blood pressure and bispectral index, are also not mentioned.

Cai *et al.* admit that there are issues of external validity in comparing the results of their studies of elderly Han Chinese patients with other populations. We are concerned that given the magnitude of the cognitive decline seen in patients who received inhalational anesthesia, and the apparently exceptionally high levels of inhalation anesthesia that were used, that there are significant differences other than ethnicity between patients in the study and the standard of anesthetic care for elderly patients. In practice, we administer general anesthesia with isoflurane to millions of elderly patients per year, and we are not on average seeing 3 days of profound cognitive impairment in previously healthy patients. We suggest that duplication of this study in other patients would not yield similar results and do not believe that there is sufficient cause to

† In Table 1 of the Cai *et al.* article, the first (baseline) MMSE in the intravenous anesthesia group is a mean of 27.25 ± 1.13, and the patients who received inhalational anesthesia had a MMSE of 7.45 ± 1.08. This would indicate that the patients who received isoflurane were extremely impaired preoperatively. We can only assume this is a typographical error, because in other parts of the article, the two groups are noted to be comparable at baseline.

suspend use of isoflurane maintenance anesthetic in elderly patients.

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In Reply:

Thank you for your attention and good suggestion regarding our article.[1] In clinical practice, we have observed that even if known factors had been excluded, there were still some patients who suffered from postoperative cognitive dysfunction—even to the extent that there is a handful of patients who suffered from long-term postoperative cognitive dysfunction. We hypothesized that there must be some other factors that we did not know about, this is why we designed this clinical trial. Because the incidence of postoperative cognitive dysfunction is low, to analyze the reason for this, a large sample is needed.

Sometimes postoperative cognitive dysfunction that occurred because of anesthesia is reversible, but the cognitive dysfunction resulting from some diseases (such as Alzheimer disease) is not reversible. The essence of the cognitive dysfunction is different, even if the Mini-Mental State Examination score is same.

There is no nitrous oxide in the inhaled anesthesia group.

To focus on the association between postoperative cognitive dysfunction and apolipoprotein E4, some sections of results were deleted during the process of modification.

Apolipoprotein E single nucleotide polymorphism varies among people with different ethnic backgrounds and living in different regions. The current study was conducted in patients who are of Han ethnicity residing in northwest China; thus, inevitable limitation exists in our research findings. The scientific results would be more universal if performed and verified in much more diverse territories and ethnic groups. We hope to see more similar or different results.

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Feedback Control Is Engineering, Let's Treat It as Such

To the Editor:

As a practicing anesthesiologist with a Ph.D. in control systems engineering, I feel compelled to comment on both the merits and hazards of closed-loop control (CLC) in the operating room, as discussed in the February 2012 article by Liu *et al.*[1] and the accompanying editorial.[2]

CLC is a mature field of engineering with well established standards of analysis, design, and reporting – points missing from Liu *et al.*'s article and the accompanying editorial. This absence suggests unfamiliarity with the hazards of CLC, which include the risk of introducing instability where none existed before.

My main criticism of Liu *et al.*'s work is that there is no evidence that the appropriate groundwork was done to ensure that the control algorithm was safe (stable) before entering the operating room. Minimizing the hazards of CLC requires a thorough stability analysis before implementation. By way of comparison, it is as important to precede CLC operational testing with computer modeling and simulation studies as it is to precede human drug trials with animal modeling and testing. Testing under extremes of "physiologically challenging conditions such as hypertension, hypotension, morbid obesity, in pediatric patients, or during major surgery such as cardiac surgery or lung transplantation," as the authors propose for their next clinical trial (see Discussion[1]), should have been done by simulation before the first clinical trial.

Controller design is the key to ultimate success and acceptance of any closed-loop strategy. There are dozens of ways to design closed-loop algorithms. Of all of the available approaches, it is noteworthy that the authors decided to use the PID, or proportional, integral, derivative, approach. The details of how PID controllers work are not important for this point of discussion. The important issue is that the PID approach is the most basic and unsophisticated of algorithms–the first one learned by every CLC engineer in their first undergraduate control course–and is suitable for some simple mechanical, electrical, or hydraulic systems (*e.g.*, automo-

This letter was sent to the authors of the referenced Editorial View, who felt that a reply was not necessary.—James C. Eisenach, M.D., Editor-in-Chief.