extension, rather than balance the upward force of laryngoscopy. Limited head extension would make laryngoscopy more difficult and could contribute to higher grade views. In our 1991 paper, the assistants did not oppose head extension.

We congratulate Santoni et al. on their highly relevant study. The degree to which MILS is beneficial in patients with potential cervical spine injury is an important and timely issue.5 The application of physics and engineering principles to medical problems will help answer clinical questions such as this. We would welcome further research about how laryngoscopy and MILS impact the forces exerted on the spine and airway, and how best to implement MILS.

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In Reply—We thank both Dr. Loane and colleagues, and Drs. Hastings and Delson for their thoughtful comments regarding our study of manual in-line stabilization (MILS).1 Loane et al. criticize our study on two levels. Because we did not allow the use of intubation aids, such as a stylet, bougie, or external laryngeal manipulation, Loane et al. suggest that our findings have little clinical relevance. They further suggest, because we did not allow the use of intubation aids, our study placed patients at increased and/or unnecessary risk of injury. When we designed our study, we decided to prohibit the use of airway aids to ensure that all of the forces of intubation would be reflected by the pressure values obtained by the instrumented laryngoscope blade. Had we done otherwise, any number of variable and nonquantifiable forces could be applied during intubation, which would preclude a valid test of our primary hypothesis. However, when making this decision, we were well aware that MILS would impair glottic view and that intubation would likely be more difficult in some patients. Nolan et al. had previously reported that 5 of 74 patients (7%) of patients could not be intubated with MILS without the use of airway aids.2 Accordingly, we included two elements in our study design to increase patient safety. First, we employed very stringent enrollment criteria to ensure that only patients who appeared easy to intubate and to have a low risk of intubation-related complications were eligible. Second, all intubations were performed by experienced faculty anesthesiologists. We anticipated that with these extra safety precautions all patients would be successfully intubated, even with MILS. Contrary to our expectation, in two of nine patients (22%), MILS precluded successful intubation, with one of these two patients experiencing a minor dental injury. This certainly begs the question of why, in our study, MILS so greatly increased intubation difficulty. This is an important question that we address in our response to Drs. Hastings and Delson. Nevertheless, because MILS so greatly increased intubation difficulty, we stopped our study after the preplanned interim analysis. We decided that the patient-related risks of continuing outweighed the statistical benefits of continuing. Therefore, we think we took appropriate steps, both in the design and conduct of our study, to minimize patient risk.

Our study made two key observations. First, even in patients who were otherwise easy to intubate, and even with experienced anesthesiologists, MILS often severely impaired glottic visualization and greatly increased intubation difficulty. Second, when confronted with a difficult intubation, anesthesiologists responded by applying much greater lifting pressures with the laryngoscope. As we review in the Discussion section of our article,1 cadaver studies show external stabilization methods that result in impaired glottic visualization—such as MILS3—increased pathologic motion at the unstable segment. Increased pathologic motion at the unstable segment can only be explained by an increase in net force across the unstable segment, with the laryngoscope serving as the instrument by which that force is applied. Accordingly, our study calls into question whether MILS actually decreases the risk of cervical cord injury with intubation. In contrast, the risks of MILS are abundantly clear. Based on our experience, we now consider MILS to almost automatically put patients into the difficult airway pathway. Accordingly, we agree with Loane et al. that having several airway aids immediately available is necessary and, in fact, may often be required to successfully intubate the patient when MILS is employed. From our perspective, these observations and conclusions are highly clinically relevant.

Drs. Hastings and Delson correctly point out that we did not measure the net force applied to the cervical spine during intubation. We agree that if the forces of laryngoscopy are perfectly counterbalanced by the assistant who applies MILS, cervical spine movement should be zero. However, two cadaver studies indicate that external stabilization methods do not appear to entirely offset the increased forces applied internally when glottic view is impaired. In cadavers with unstable spines, external stabilization methods that impair glottic view—either MILS3 or a cervical collar4—increase pathologic motion at the unstable segment; this can only be explained by increased force across the unstable segment.

Drs. Hastings and Delson suggest that our findings may have been influenced by the method by which MILS was performed. We think that they are certainly right. It is ironic that although MILS is currently considered to be a standard of care, there is no standard for how MILS is to be performed. There is no formal description of how MILS is to be performed in the current Advanced Trauma Life Support manual other than “during intubation, the neck must be maintained in neutral position.”5 When described at all, MILS techniques vary widely among studies. Our MILS technique was based on the descriptions of Nolan et al. (“The aim of [MILS] is to prevent cervical spine movement by the application of equal and opposite forces to those generated by the intubator”)2 and Heath et al. (“The patient’s neck [is] immobilized by . . . holding the sides of the neck and mastoid processes, thus preventing any movement of the neck during . . . laryngoscopy”).6 Accordingly, we applied MILS to prevent any appreciable movement of the head or neck, and quite specifically, to prevent craniocervical extension during intubation. Although anesthesiologists applied increased pressure to airway tissues, increases in tissue displacement were often not sufficient to obtain a line of sight (Grade 3 or 4 glottic view in five of nine patients). Because of recent concerns

References


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regarding the safety and efficacy of MILS\textsuperscript{7} and wide variability in MILS technique, we would welcome the development of a consensus statement regarding how MILS is to be applied and the clinical endpoints to be achieved. We need to understand what MILS truly is before we can determine what it truly does; to determine if MILS is safe and effective or, possibly, otherwise.

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References


To the Editor.—We read with interest the study by Bilotta \textit{et al.}\textsuperscript{1} comparing intensive insulin therapy to conventional treatment in two cohorts of neurosurgical patients. When an arterial catheter was not available for glucose determination, capillary blood was measured with the HemoCue point-of-care device (HemoCue, Angelholm, Sweden). This was a wise choice, as this system has been shown to be accurate, even at low glucose concentrations.\textsuperscript{2} What is not commonly appreciated is that most other point-of-care glucose measurement devices are not sufficiently accurate for critical care use, and that such devices originally approved and regulated for home self-monitoring by patients with diabetes have migrated into the hospital setting without further regulatory scrutiny. In fact, it has been specifically recommended that point-of-care devices designed for patient use at home, yet sometimes used in hospitals, should not be used in critically ill patients.\textsuperscript{3}

Although their use of capillary blood for glucose determination is understandable, it should be pointed out that arterial blood glucose concentrations have been demonstrated to be a better representation of plasma glucose as compared with simultaneous capillary measurements.\textsuperscript{4,5} This may be secondary to a variable time constant in the fingertip blood pool. Furthermore, hypotension, hypoxia, and acidosis, which are common problems in the critical care population, can significantly affect these capillary readings.\textsuperscript{6}

Not all glucose measurements are equivalent, and care must be taken in their interpretation, especially at low concentrations.

References


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In Reply.—Dr. Rice \textit{et al.} have raised a very interesting question about our article published in the March 2009 issue of \textit{Anesthesiology},\textsuperscript{1} that concerns the accuracy of the glycemic measurements on capillary blood samples. We agree that the most reliable measurement for blood glucose concentration is on blood samples obtained by arterial line. Generally, the measurement of blood glucose concentration through the HemoCue B-Glucose analyzer (HemoCue, Angelholm, Sweden) with peripheral capillary blood is considered to be reliable in noncritical patients.\textsuperscript{2,5} Thus, previous studies even in critical care patients have used this approach, including the one by Van den Berghe \textit{et al.} that specified: “The dose of insulin was adjusted according to whole-blood glucose levels, measured at 1- to 4-h intervals in arterial blood or, when an arterial catheter was not available, in capillary blood, with the use of a point-of-care glucometer (HemoCue Bglucose analyzer, HemoCue).”\textsuperscript{4} Our protocol is similar to that of Van den Berghe \textit{et al.}, but we hypothesized that in our patients population, which is relatively younger than in previous studies, the risk of hypotension, hypoxia, and acidosis is lower.

Nevertheless, as we have mentioned in our article, to overcome the risks of this bias we have performed the measurements of blood glucose concentration exclusively on capillary blood samples only in few cases (i.e., patient in which was difficult to get arterial blood sample: <5% of the measurements). Thus, “in all patients, glucose concentrations were measured in whole blood or on undiluted arterial blood rather than capillary blood samples.” Furthermore, as we reported in the paper, we have made systematic cross controls...