Letter

Letter to the editor

Balavenkatesh Kanna[1], Haj Asaad Ayman[2] and Anita Soni[3]

[1]Assistant Program Director of Internal Medicine, Lincoln Hospital, and Assistant Professor of Internal Medicine, Weill Medical College of Cornell University, New York, New York, USA
[2]Resident, Department of Internal Medicine, Lincoln Hospital, Bronx, New York, USA
[3]Chief and Program Director, Lincoln Hospital, and Associate Professor of Internal Medicine, Weill Medical College of Cornell University, New York, New York, USA

Corresponding author: Balavenkatesh Kanna, bvkanna@aol.com

Published online: 7 February 2005
This article is online at http://ccforum.com/content/9/4/414

Critical Care 2005, **9:**414-416 (DOI 10.1186/cc3043)

Regarding the interesting article on early tracheostomy by Arabi and coworkers [1] recently published in *Critical Care*, we should like to offer the following comments.

There are significant differences between the two study groups in terms of baseline characteristics. Patients with maxillary trauma, as reported by the investigators, are usually preselected for early tracheal intubation. This practice is dependent on the notion that respiratory failure is due to airway compromise rather than lung injury. The study also reported delayed tracheostomy in patients with spinal injury because of the need to stabilize the spine before tracheostomy. Therefore, it will be useful to know what were the indications for endotracheal intubation among the study patients [2]. It would also be interesting to know what were the weaning criteria employed in each study group before and after tracheostomy [3].

Whether critically ill intensive care unit (ICU) patients with medical comorbidities derive benefit from early tracheostomy, as compared with the predominantly surgical patients included in the study by Arabi and coworkers, remains unknown. However, other studies have demonstrated a lack of benefit from early tracheostomy in patients with additional medical comorbidities [4].

Because the length of stay (LOS) in ICU was the same in both groups after tracheostomy, we believe that the decrease in total LOS in the early tracheostomy group cannot be attributed to the timing of tracheostomy alone. Criteria for transfer of patients out of the ICU to the ward are highly variable among different institutions and depend on local facilities and availability of staff to manage patients on mechanical ventilation in the medical wards. Therefore, outcome measures of overall LOS in an ICU will depend on local facilities and practices.

Authors' response

Yaseen Arabi, Samir Haddad, Nehad Shirawi and Abdullah Al Shimemeri

We should like to thank Dr Kanna and colleagues for their letter.

In our article [1] we addressed the issue of differences in baseline characteristics, namely whether maxillofacial injuries or spinal cord injuries were present. Using multivariate analysis, we found late tracheostomy (odds ratio 6.9, 95% confidence interval 2.6–18.1; $P < 0.001$) and, to a much lesser extent, spinal cord injury (odds ratio 4.7, 95% confidence interval 0.99–22.6; $P = 0.052$) to be independent predictors of prolonged ICU stay.

As we indicated in our report, the study was based on an ICU database, and therefore details regarding the reason for intubation and mode of weaning were not available. However, our trauma patients (like other trauma patients) are typically intubated for airway protection as part of their initial resuscitation.

The purpose of our study was to examine the impact of tracheostomy timing in trauma patients – a population that is typically young and free from medical comorbidities. According to Acute Physiology and Chronic Health Evaluation II definitions for chronic illnesses [5], there was only one patient with chronic renal failure in the early tracheostomy group and one patient with chronic respiratory insufficiency in the late tracheostomy group, and no patients

ICU = intensive care unit; LOS = length of stay.

had chronic cardiac or liver disease or immunosuppression. Therefore, no conclusions could be drawn from our study regarding the impact of medical comorbidities. However, we disagree with the assertion by Kanna and colleagues that no benefit has been demonstrated for early tracheostomy in medical (as opposed to surgical) patients. A recent prospective, randomized controlled trial in medical patients found significant reductions in mortality rate, incidence of pneumonia and LOS [6].

We were surprised by the statement by Dr Kanna and colleagues that the ICU LOS was 'the same in both groups'. This was not the case because the main finding in our study was a significant reduction in ICU LOS (10.9 ± 1.2 days for the early tracheostomy group versus 21.0 ± 1.3 days for late tracheostomy patients; $P < 0.0001$). Because our patients were discharged at comparable periods after tracheostomy, as shown in Table 2 of our report [1], the difference in ICU LOS could only be explained by the reduction in pretracheostomy duration (i.e. the timing of tracheostomy, or days from ICU admission to tracheostomy: 4.6 ± 0.5 days versus 14.1 ± 0.5 days; $P < 0.0001$).

We agree that ICU discharge practices vary among institutions, but this is unlikely to affect the results and implications of our study because the comparison is made between two groups cared for in the same institution, by the same physicians and using the same discharge practices.

Our clarification to the Author's comments

Balavenkatesh Kanna, Haj Asaad Ayman and Anita Soni

In reply to the following comment on the length of ICU stay by Arabi and coworkers, we would like to clarify our interpretation of the study results.

Author's Comment

"We were surprised by the statement by Dr Kanna and colleagues that the ICU LOS was 'the same in both groups'. This was not the case because the main finding in our study was a significant reduction in ICU LOS (10.9 ± 1.2 days for the early tracheotomy group versus 21.0 ± 1.3 days for late tracheotomy patients; $P < 0.0001$). Because our patients were discharged at comparable periods after tracheotomy, as shown in Table 2 of our report [1], the difference in ICU LOS could only be explained by the reduction in pre-tracheotomy duration (i.e. the timing of tracheotomy, or days from ICU admission to tracheotomy: 4.6 ± 0.5 days versus 14.1 ± 0.5 days; $P < 0.0001$)".

Our Clarification

The length of stay in ICU after tracheostomy was the same in both groups.

With reference to Table 2 in the study by Arabi and coworkers, we would like to point out that the length of stay in ICU (ICU LOS, 10.9 ± 1.2 versus 21.0 ± 1.3) was different in both groups due to the difference in days from ICU admission to tracheotomy (4.6 ± 0.5 versus 14.1 ± 0.5) rather than a difference in the days from tracheotomy to ICU discharge (6.3 ± 1.3 versus 6.9 ± 1.1) Also, the duration of mechanical ventilation (9.6 ± 1.2 versus 18.7 ± 1.3) was different between both study groups due to difference between Ventilation days before tracheotomy (4.6 ± 0.5 versus 13.9 ± 0.5) as opposed to days from tracheotomy to weaning (4.9 ± 1.2 versus 4.9 ± 1.1).

If the days from tracheotomy to ICU discharge or days from tracheotomy to weaning were less in the early tracheotomy group, then one can attribute the difference in the total ICU LOS or duration of mechanical ventilation between the two study groups to tracheotomy alone. In this study, a significant number of patients with maxillary trauma, low Glasgow Coma scale score underwent early tracheotomy and those with spinal cord injury had delayed tracheotomy. This introduces selection bias which could partly explain the differences among days from admission to tracheotomy and ventilation days before tracheotomy between the study groups.

Despite limitations among studies, we appreciate the advantages of early tracheotomy including provision of patient comfort, secure airway, facilitation of weaning from ventilator and improving the ability to manage ventilator-dependent patients in non- intensive care step-down units. [7] The observations made by Arabi and coworkers are important in the airway management of critically ill patients.

Authors' response

Yaseen Arabi, Samir Haddad, Nehad Shirawi and Abdullah Al Shimemeri

Dr Kanna and colleagues cite again our findings that ICU LOS after tracheostomy was not significantly different in both groups. We have addressed this point both in our article and in our previous response. Our primary endpoint was "total" ICU length of stay (ICU LOS) used as a surrogate for ICU resource utilization. Subdividing ICU LOS is of less importance (and is probably irrelevant) as an indicator of resource utilization. Our study showed that early vs. late tracheostomy was associated with a significant reduction in the primary endpoint (ICU LOS). The points mentioned in Dr Kanna's second letter fully support our point that ICU LOS was shorter in the early tracheostomy group mainly because of differences in the timing of the procedure and not because of its effect on weaning.

Additionally, the focus of our study was whether early (versus late) tracheostomy and not tracheostomy itself (versus no tracheostomy) shortens ICU LOS. Therefore, we are not attributing the significant differences in ICU LOS in our study cohort to tracheostomy but rather to timing of tracheostomy. The issue of whether tracheostomy (versus no tracheostomy) facilitates weaning and shortens ICU LOS was not the objective of our study and is better resolved with different study design. However, timing of tracheostomy may in fact affect weaning. The lack of a significant effect in the trauma population does not exclude such an effect in other patients' populations. A recent retrospective cohort study on medical ICU patients showed that tracheostomy performed after 21 days of intubation was associated with a higher rate of failure to wean from mechanical ventilation, longer ICU stay and higher ICU mortality [8].

Once again, we have addressed the issue of differences in maxillofacial and spinal cord injuries using multivariate analysis. We have shown that after adjustment to the presence or absence of these injuries; late timing of tracheostomy remained the most significant predictor of prolonged ICU stay. We refer Dr Kanna to the article and to our previous response for the values of Odds ratios and Confidence Intervals.

Recently, we examined a cohort of 347 ICU tracheostomized medical-surgical patients [9]. Again we found that timing of tracheostomy was an independent predictor of ICU LOS.

The efficient use of ICU resources is focus of all ICU administrators facing the increasing demands and financial and staffing constraints. Our study suggests that we can potentially reduce resource utilization without negatively affecting patient outcomes by modifying our practices of tracheostomy timing.

More than ever before we recognize that time is the essence in different aspects of critical care [10]. Tracheostomy timing is probably no exception.

Competing interests

The author(s) declare that they have no competing interests.

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