Ribavirin Should Be Tested in Clinical Trials in Combination With Other Antiviral Agents for Severe Acute Respiratory Syndrome

To the Editor:

We read with interest the article in CHEST by Chion et al (July 2005) and offer the following comments. The ribavirin-treated patients had higher lactate dehydrogenase levels, a well-known adverse prognostic factor in severe acute respiratory syndrome (SARS). The nonsignificantly higher mortality could be due to the more severe disease in this group. Viral load, another important predictor of mortality, was not available. Moreover, Figure 1 seemed inaccurate: the survival in ribavirin-treated patients by proportion (y-axis) is difficult to understand. In Figure 4, the shaded triangles were supposed to represent the more severe drop in hemoglobin in the hypoxemic subgroup was that they had more severe disease. The survival curves in Figure 4 also appeared inaccurate: the survival in patients with drop in hemoglobin > 2 g/dL should be 0.69 (5 of 16 patients died) instead of 0.45. Hence, the result of the log-rank test (p = 0.007) needs to be justified.

Only factors that were potentially associated with hypoxemia were analyzed in Table 2. No univariate or multivariate analyses on factors related to death were reported. The conclusion that hemoglobin level was the only factor associated with death was not supported by the data presented.

In Figure 6, the shaded triangles were supposed to represent the hemoglobin of patients who were hypoxic and had received ribavirin. There were 22 triangles, but there should only be 17 patients. In addition, expressing the survival of individual patients by proportion (y-axis) is difficult to understand.

Therefore, there is no convincing evidence that ribavirin has contributed to a life-threatening drop in hemoglobin or mortality in this report. As of today, three independent studies have shown ribavirin to have in vitro activities against SARS-coronavirus, alone or in combination with other agents. Ribavirin should be tested in future randomized controlled studies in combination with other potential antiviral agents for SARS.

REFERENCES


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A Modified Percutaneous Tracheostomy Technique Without Bronchoscopic Guidance

A Note of Concern

To the Editor:

We read with interest the article in CHEST by Paran and colleagues (September 2004) on a modified percutaneous tra-
cheostomy (PcT) technique performed without bronchoscopic guidance. Although their initial results seem to match those reported in the literature following conventional PcTs, we think that some points need to be addressed.

An important technical detail in avoiding PcT-related complications, such as tracheo-innominate artery fistula and late postintubation cricoid stenosis, is the site for the surgical incision. The authors performed a midline, vertical 2-cm incision just above the sternal notch with subcutaneous dissection in order to enter the airway between the second and third tracheal rings. This maneuver is more effectively achieved if the incision is made 1 to 1.5 cm below the cricoid cartilage as this gives direct access to the required point of entry to the airway with minimal subcutaneous dissection. It is advisable to locate the incision with reference to the cricoid and not to the sternal notch because the larynx and the trachea move independently of the sternum when the neck is flexed or extended.2

The second point regards the issue of stomal bleeding and/or local soft-tissue infection following PcT. The low rate of stomal bleeding and infection associated with conventional PcT3,4 is mainly ascribable to certain features of the PcT stoma. Following conventional PcT, the stoma fits snugly around the cannula, and the absence of dead space serves to both tamponade bleeding vessels and to prevent infection.3 Because of the relatively wide skin incision proposed by the authors and the subsequent blunt dissection of the subcutaneous tissue needed to manually assess the level of the entry into the airway, the stoma is likely to fit loosely around the cannula, without any compression effect on the surrounding tissue and, therefore, with an increased potential risk of stomal bleeding and/or infection. Under such conditions, the risk of bleeding is further increased by potential injury to the highly vascularized thyroid isthmus, which lies over the second and third tracheal rings.

In conclusion, although the type of modified PcT proposed by Paran and colleagues1 has the advantage of being performed without bronchoscopic guidance, it may expose patients to the unnecessary risk of serious complications that would inevitably entail increased hospital costs and fails to match the positive results achieved by conventional PcT performed under bronchoscopic guidance in larger series of patients.

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To the Editor:

Melloni and colleagues expressed their concern regarding possible complications following the modified technique of percutaneous tracheostomy. They hypothesize that the skin incision using the sternum as a reference point could result in wrong placement of the cannula. Since the technique described in our report is primarily based on limited blunt dissection of the subcutaneous tissues, it allows accurate positioning in insertion of the cannula. We believe that the surface landmark should serve only as a guideline, as opposed to the strict percutaneous tracheostomy, which relies entirely on surface landmarks and thus necessitates bronchoscopic guidance.

The second point of the letter addresses the potential complication of bleeding from subcutaneous tissues. Based on our experience in > 100 cases, the careful blunt dissection down to the pretracheal fascia allows careful navigation and displacement of blood vessels and the thyroid isthmus. This technique, therefore, when performed by a surgeon, actually prevents inadvertent damage to local blood vessels, which sometimes may occur when blindly inserting a cannula from the skin surface directly into the trachea. In our series, we had only one case of small but persistent bleeding from subcutaneous tissues that required revision in the operating theater. We did encounter in two cases a larger blood vessel that was detected while performing blunt dissection. This finding led us to abort the procedure and continue by conventional surgical tracheostomy. In these cases, performance of the conventional percutaneous tracheostomy would most probably have led to significant bleeding.

As for the theoretical danger of infection, we believe that infection in this setting usually originates from the contaminated airway and not from skin. A larger incision allows for adequate drainage, while snug closure of the skin around the cannula may actually even predispose to infection. Indeed in our series there were no cases of soft tissue infection that required additional drainage.

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References
2 Grillo HC. Surgery of the trachea and bronchi. Hamilton, ON, Canada: BC Decker, 2004; 499–506

“Frequent Fliers” Do Not Receive a Free Trip in the Emergency Department

To the Editor:

I read with interest the article by Griswold et al (May 2005).1 The article characterized the patients who are more likely to have a high number of visits to the emergency department (ED). The study was well-conducted, confirmed previously reported data, and moved the field forward by adding solid new information.

In their article, the authors referred to the patients who had a high number of visits to the ED as “frequent fliers.” I think that the use of this term, besides being misleading (patients do not get a free trip or enjoy visiting the ED frequently), dangerously stereotypes this group of patients. Stereotyping our patients...