had a poor peak flow response after one nebulizer treatment with albuterol. This excluded the subset of patients who have a markedly improved peak flow response after one treatment and are less likely to need steroid therapy. From a statistical standpoint, if steroid vs control response detection by pulmonary function studies is evaluated in both arms of therapy for all patients with a poor peak flow response before albuterol therapy, and some patients respond dramatically in both groups, the ability to detect a difference will be minimized due to the large range of outcome data in both groups. By implementing this initial inclusion/exclusion criteria in our study design, we increased our chances of finding a difference in peak flow response between treatment and control. This is another possibility for our study findings compared to others.

We congratulate Rodrigo and Rodrigo1 for their evidence-based effort. Despite our possible finding, the weight of the evidence reveals no effect of systemic steroids in improving pulmonary function in patients with acute asthma in the first 6 h. Even if our finding turns out to be real, the effect on improving pulmonary function compared to standard therapy would probably be marginal.

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Infusion Phlebitis in Patients in a General Internal Medicine Service

To the Editor:

In relation to the recent article by Monreal et al., we believe readers may be interested to learn of the results of a study carried out in our General Internal Medicine Service and communicated at the International Symposium on Thromboembolism held in Lisbon in June 1999. In this work, we investigated the local complications of IV therapy in the 363 patients to whom it was applied in the Service over a period of 3 months, with emphasis on the frequency of phlebitis following venous catheterization and the risk factors favoring this complication; only first catheterizations were considered. Twenty-gauge catheters were used in 92% of cases, 22-gauge catheters in 3%, and 18-gauge catheters in 1.9%; needles were used in 1.7%, and in the remaining 1.4% of cases, central catheters were installed.

Phlebitis arose in 35% of cases. Monreal et al. observed a slightly higher incidence, 39%, possibly because their study was limited to pneumonia patients and thus to patients receiving IV antibiotics, a known risk factor (see below); other authors have reported incidence rates from 20 to 70%.

Onset was invariably within the first few days of catheterization (95% confidence interval, 2.7 to 3.5 days). There were no statistically significant differences among the different types of catheter (unsurprisingly, in view of the great predominance of 20-gauge catheters). Like Monreal et al. and others, we found that IV administration of antibiotics was the greatest risk factor. Furthermore, risk increased with the number of antibiotics administered. Also like Monreal et al., we found that incidence was lower among patients who were receiving IV corticosteroids than among those who were not, with similar findings for diuretics and bronchodilators, although in none of these cases was the difference statistically significant. We regret not having investigated the influence of hemoglobin level, the pro-phlebitic effect of which was the most interesting finding of Monreal et al.

In conclusion, we found a high incidence of phlebitis among patients receiving IV medication. There appears to be no doubt that IV administration of antibiotics is a risk factor, and the risk increases with the number of antibiotics administered. Possible prophylactic measures include the use of heparin and limitation of the level of insult by control of drug dilution and infusion rate.

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Differences in Percutaneous Dilational Tracheostomy Kits

To the Editor:

I read with puzzlement the “Communication to the Editor” by Dean P. Sandifer, MD, FCCP,1 in regard to the article by Trottier et al,2 pertaining to percutaneous dilatational tracheostomy (PDT) using the PDT kit manufactured by Smith Industries (Keene, NH). Dr. Sandifer justifiably criticized the high rate of complications that occurred in the 24 PDT cases studied.

But then Dr. Sandifer stated, “We have completed scores of PDTs using this kit without a single tension pneumothorax, mainstem tracheostomy tube placement, or tracheostomy tube obstruction, and certainly without any patients requiring post-PDT thoracotomy.” He went on to say “the tapered Portex PDT tube (Smith Industries) offers a distinct advantage over the kit manufactured by Cook Inc. (Bloomington, IN) regarding the final step of tracheostomy tube insertion over a dilator.”1 This opinion may have some validity, but the Portex tube was in the kit used in the 24 PDTs performed by Trottier et al,2 as they pointed out on page 1384, under “Materials and Methods,” that they used “a custom Portex tracheostomy tube (Sims Portex Inc; Keene, NH), with a tapered, flush opening.”

Trottier et al2 then showed, clinically and in animal and laboratory work, a very clear explanation of their complications. What puzzles me (and we believe him) is that Dr. Sandifer, using the same kit, got such good results. Evidently, in his technique, he observed certain points such as the obliquity of the dilators and avoidance of excessive force, and he probably has a lot more experience. Such an operator may not need a stabilizing ridge, but because many operators are not as skilled as Dr. Sandifer, Smith Industries has now placed a ridge on the tube, as shown in Figure 3 of the article by Trottier et al,2 to prevent the dilator from sliding onto the wire guide.

However, with the new, flexible, long-cone, one-pass dilator, recently put on the market by Cook Inc, the problems associated with rigid dilators will be avoided.

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