to the minimal COX-1 inhibition in rofecoxib. In any case, we recommend caution when prescribing selective COX-2 inhibitors to patients with aspirin-sensitive asthma.

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To the Editor:

We appreciate the comments of Drs. Passero and Choudhry regarding our study of selective cyclooxygenase-2 (COX-2) inhibitors in patients with aspirin-induced asthma (AIA), which was published in CHEST (June 2002). We agree that recent reports about the safety of such inhibitors in patients with AIA were carried out with a limited number of patients; as we suggested in our article, further challenge procedure studies performed with higher doses of rofecoxib and other highly selective COX-2 inhibitors on larger series of patients with AIA are necessary in order to achieve the safety of such new drugs in patients with AIA.

The authors mentioned that the acute exacerbation of asthma developed after three doses of rofecoxib, each 25 mg. We supposed each rofecoxib dose was taken once per day on 3 consecutive days, and not all three doses in 1 single day. There are no published data at present about the safety of COX-2 inhibitors in patients with AIA receiving high doses, and we regard that studies concerning this point are necessary.

It is known that the degree of sufficient enzymatic inhibition to induce bronchial narrowing in patients with AIA is an individual hallmark; therefore, we believe that oral challenges with progressive doses of the drug (rofecoxib or celecoxib) would have been done until a therapeutic and tolerable dose was reached. Nevertheless, despite the safety demonstrated by the new highly selective COX-2 inhibitors, caution when prescribing any type of nonsteroidal anti-inflammatory drugs in patients with AIA should always be recommended.

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Noninvasive Ventilation Is More Than Mask Ventilation

To the Editor:

I read with discouragement the article by Markstrom et al (November 2002) comparing the quality of life for patients with neuromuscular disease treated by noninvasive ventilation (NIV) vs that for patients who have tracheostomies. It is discouraging that an article could be published that equates NIV only with mask ventilation; that does not indicate pulmonary function or the extent of the need for ventilatory support and, thus, makes no effort to match cohort groups; that does not indicate the type of ventilator used, or settings, indications, or approaches; that ignores the vital need for mouthpiece ventilation, or even pneumobelt use via portable volume-cycled ventilators, for patients with advanced disease who require NIV continuously; that makes no mention of manually or mechanically assisted coughing methods or their vital need during intercurrent chest infections; and then concludes that tracheostomies are considered desirable by many postpolio patients and postkyphoscoliosis patients.

It is obvious that patients who are not trained in air stacking, effective and convenient daytime aid methods, or mechanically assisted coughing would feel more secure having tracheostomy tubes for disease management during intercurrent infections. Indeed, patients who are limited to mask ventilation, quite possibly at low pressure spans or inadequate daytime volumes, might feel better with a tracheostomy tube, even as a nocturnal aid. We have already reported on >100 patients who used both NIV and tracheostomy ventilation for continuous ventilatory support for ≥1 month, and only a few of those changing from NIV to tracheostomy ventilation who were never taught mouthpiece ventilation, air stacking, or mechanically assisted coughing considered the tracheostomy tube to be more desirable. Furthermore, there are cohort-matched studies of the quality of life comparing patients using the noninvasive and tracheostomy methods that the authors never mentioned. I suggest that the authors obtain a recent book on noninvasive ventilation and learn that there is more to NIV than mask-only ventilation.

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2156 Communications to the Editor

Downloaded From: http://journal.publications.chestnet.org/pdfaccess.ashx?url=/data/journals/chest/21995/ on 06/26/2017
The comments on our article published in CHEST, about home mechanical ventilation (HMV), are very refreshing as the results of the study elucidated those same questions by us. However, the main purpose of the study was to assess quality of life (QoL) of patients receiving HMV. The study was retrospective, and the purpose was never to relate QoL with lung function and compliance during NIV. We consider it a strength to be able to compare the results show that, despite severe physical limitations, patients receiving HMV perceived good QoL. It is mandatory to optimize both treatment with Tracheostomy with good, individually fitted cannulas and compliance during NIV. We conclude that the patients treated with both NIV and invasive HMV reported a good QoL. As a result of this study, we have started to look into the questions raised in Dr. Bach’s comments. We hope to publish the results of this year, but we still must remember that this is not a prospective study, I can only apologize for having forgotten your references. To sum up, our article is not a comparison study of which method is best for ventilation of patients with chronic respiratory insufficiency. The results show that, despite severe physical limitations, patients receiving HMV perceived good QoL. It is mandatory to optimize both treatment with Tracheostomy with good, individually fitted cannulas and compliance during NIV. We consider it a strength to be able to offer both treatment options for patients with chronic respiratory insufficiency. This is a very important message. Because treatment with HMV has increased in Sweden, it is important to show that long-term survival is improved by HMV, and the QoL is also good.

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To the Editor:

I have read with great interest the excellent review by Shafazand and Weinacker on the use of blood cultures taken from patients in the critical care unit (November 2002). Indeed, problems with false-positive and even false-negative results have long been a bane for all of us who work in the ICU environment. While agreeing with almost everything that the authors said, I do want to suggest two modifications to their recommendations, listed on Table 2 (page 1729).

First, published data strongly suggest that the diagnostic yield of routine anaerobic blood cultures is virtually nil (<1%), in fact, plating a second aerobic bottle may be a better diagnostic option. In two large series (almost 30,000 blood cultures analyzed), all patients with positive anaerobic culture results had very strong pretest probability (ie, clinical suspicion) that such an etiology of bacteremia was likely. Thus, it would appear that anaerobic bottles do not need to be routinely included in ICU blood culture sets; rather, they should be used only in cases where an anaerobic culture is deemed to be clinically indicated.

Second, arguably the major problem with blood cultures is the relatively high proportion of false-positive results. The authors discussed this issue quite nicely in their article. In our ICU, to better deal with this pervasive problem, we have opted to define a “blood culture set” not as two blood culture bottles from one venipuncture site (as proposed by Shafazand and Weinacker), but as two anatomically separate venipuncture sites obtained at one point in time, with one or two blood culture bottles being filled from each site. In other words, in our ICU, a blood culture set refers to a point in time when blood cultures were obtained, and each set includes a minimum of two separate venipuncture sites. For example, if a patient is to have three blood culture sets drawn, this means that he or she will be sampled at three different points in time over a 24-h period, each time having a strong pretest probability (ie, clinical suspicion) that such an etiology of bacteremia was likely. Thus, it would appear that anaerobic bottles do not need to be routinely included in ICU blood culture sets; rather, they should be used only in cases where an anaerobic culture is deemed to be clinically indicated.

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