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To the Editor:

We are grateful to Dr. Monteserin and colleagues for their correction of the typographical errors in our papers and also for providing clarification of his data. Of major interest, the increased prevalence of the S variant for α1-antitrypsin in asthmatics is again indicated, this time in a population from Spain.

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More on Asthma Prevention

To the Editor:

Dr. Bailey’s response to Dr. Unger’s critique on the former's not necessarily benign neglect on preventive measures in controlling asthma is like preparing a pot of chili with only bell pepper.

Even if the pulmonologists choose to ignore allergen immunotherapy as an effective form of antiallergy treatment, the general lack of understanding on effective measures to reduce allergen exposure amounts to poor medical practice.

None of us would argue Dr. Bailey’s fear that restricting a patient’s life-style excessively is undesirable. We allergists have come to the same conclusion by providing our patients the information as well as the tools to handle their indoor allergen exposure. There is simply no excuse for anyone treating patients with allergic asthma to be ignorant of the recent advances in house dust mite and cat allergen avoidance measures. Our ultimate goal, of course, is to serve our patients better.

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Patient number 3 is particularly instructive in that this patient experienced an apical pneumothorax after the talc instillation, and it was never clear that good pleural apposition occurred. When the effusion did recur at 10 weeks, it was loculated and limited to the apical portion of the lung with good pleural synthesis being achieved about the base of the lung. With the exception of the patient with technical difficulties, we have had virtually no recurrence of ipsilateral effusion in our patients thus far. Other observations from our experience include the possibility of recurrence of effusion on the contralateral side and the relatively short survival of patients with malignant effusion. Our survival profile is very similar to other larger controlled studies.5,7

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This report is not a controlled clinical trial but rather simply a sharing of information regarding a simple and effective clinical procedure.

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Pain Control After Thoracotomy

To the Editor:

We read with interest "Pain Control After Thoracotomy: An Extra Pleural Tunnel to Provide a Continuous Bupivacaine Infusion for Intercostal Nerve Blockade" and the accompanying editorial.1,2 We apologize for the delay in making this reply; nevertheless, we would like to do so as our experience in this technique spans thousands of patients over almost a decade, and there was an inaccurancy in a reference to our work that we think we should correct.

As stated in this paper, we do indeed have a very high degree of pain relief with this technique3 that is capable, if used in conjunction with pre-emptive, balanced analgesia including a percutaneous paravertebral block, of totally maintaining preoperative lung function postoperatively.4 In addition, we are able to show complete inhibition of stress responses to the posterolateral thoracotomy, which hitherto has been an unattainable goal.4

The results from the study by Majid and Hamzah with a similar technique are less impressive. A third of their patients had severe pain in the first 24 h, and moreover, pulmonary function, te, a more objective assessment of pain on chest movement, was not assessed. We would like to offer some explanations for these somewhat suboptimal results. In their description of the creation of the extra pleural tunnel, it was not stated that the pleura was stripped right up to the vertebral bodies. We feel this is important because the method of action of extra pleural analgesia is via the paravertebral route,5 and to achieve global analgesia, it is essential to block the sympathetic chain and the posterior branch of the intercostal nerve in addition to a standard intercostal nerve block. The creation of the tunnel by Majid and Hamzah, according to their description could have been somewhat too lateral to occasion blockade of all these three structures.

They recommended the use of 0.25 percent bupivacaine and no bolus was used. We use a bolus of 0.5 percent bupivacaine (10 ml pre-emptive percutaneously and 10 ml on chest closure) followed by an infusion of 0.5 percent bupivacaine at a rate of 0.1 ml/kg/h and continuing for 5 days into the postoperative period. This amounts to about 200 µg in the first 5 h, not 300 µg, as stated by Majid and Hamzah. The degree of analgesia with this regimen is excellent, but in addition, we have only occasionally observed probable bupivacaine toxicity presenting as mild confusion, which has always responded to a cessation of infusion for a few hours. Bupivacaine levels with this regimen are 1.2 to 1.6 mg/ml with a Tmax of 35 to 45 min. At 58 h we have found Cmax of 4.3 to 5.1 mg/ml.6

The presence or absence of an intact pleural membrane and a well-sealed tunnel as taken up by the editorial accompanying this paper, we have found it to be irrelevant; indeed we have also successfully evaluated the use of this technique in patients undergoing pleurectomy.7

We commend this technique to all workers in the field of thoracic surgery, but we believe, in addition, that the benefits offered from a balanced pre-emptive approach to pain afterward should be extended to all patients undergoing major surgical procedures.

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