Communications for this section will be published as space and priorities permit. The comments should not exceed 350 words in length, with a maximum of five references; one figure or table can be printed. Exceptions may occur under particular circumstances. Contributions may include comments on articles published in this periodical, or they may be reports of unique educational character. Please include a cover letter with a complete list of authors (including full first and last names and highest degree), corresponding author’s address, phone number, fax number, and e-mail address (if applicable). An electronic version of the communication should be included on a 3.5-inch diskette. Specific permission to publish should be cited in the cover letter or appended as a postscript. CHEST reserves the right to edit letters for length and clarity.

Communications to the Editor

Confounding Issues in COPD Risk Study?

To the Editor:

We read with extreme interest about the Baltimore Longitudinal Study of Aging (February 2002)1 and have some elementary questions regarding the definition of COPD and the implication of a high body mass index (BMI).

First, the case definition of COPD involved both objective spirometric parameters as well as clinical diagnosis of chronic bronchitis, emphysema, and chronic obstruction. Therefore, an alternate explanation for the study results (low BMI correlating with higher risk for COPD) could be that patients with high BMI are less likely to receive a clinical diagnosis of COPD. It is intuitive that there is a higher chance that the clinical symptoms in volunteers with a high BMI would more likely be attributed by the physicians to the obesity and likewise, in volunteers with a low BMI, be attributed to a pulmonary pathology, ie, bronchitis or emphysema.

Secondly, the study failed to assess the effect of obesity on the FEV1/FVC ratio (the criteria for diagnosing COPD). Obesity, as other extra parenchymal restrictive lung diseases, could have a variable effect on the FEV1/FVC ratio, thereby acting as a confounder. This could conceivably influence the data in patients with a high BMI.

Another parameter in question is the BMI. For instance, could weight-training exercise, which involves an increase in the absolute muscle mass and thereby the BMI, be an explanation of the spirometric outcomes? One would be inclined to think that the pulmonary outcomes would be more favorable in the exercisers with a higher muscle mass than the sedentary person with the lower muscle mass. Perhaps controlling for exercise tolerance between the groups would clarify the question. The discussion explaining the relation of low BMI with COPD implied that low BMI correlates with poor nutritional status; however, those in the lower tertile had optimum BMIs of 20 to 25.

Other confounders of the results that deserved discussion are the issues of second-hand smoking and occupational exposures.

Like all good studies, this one raises many important questions and was a thought-provoking study of interest in the specialist as well as primary care setting.

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REFERENCE

1 Harik-Khan RI, Fleg JL, Wise RA. Body mass index and the risk of COPD. Chest 2002; 121:370–376
Dry Talc Pleurodesis Via Chest Tube

To the Editor:

Parulekar et al [July 2001] report an interesting retrospective review of malignant pleural effusions drained with small-bore catheters or large-bore chest tubes. Most of their patients received tetracycline as the sclerosant and not surprisingly showed only a 51 to 53% long-term success rate. Although the success rate is disappointing, it is noteworthy that smaller-bore catheters were as effective as large tubes.

Of perhaps greater importance is their allusion to the use of dry talc insufflation via a large-bore chest tube in a small subset of their series. Talc by thoracoscopic insufflation or by slurry has become the “gold standard” in pleurodesis during the past decade. Any effort to simplify the application of dry talc and thus avoid the need for thoracoscopy is noteworthy. Since there are few data anywhere on this approach, it would be of interest if the authors could provide more information on their talc insufflation subset: (1) of the 27 patients receiving talc, how many received it by insufflation via large tube, (2) what is the 30-day and long-term success rate of these patients, (3) how many milliliters (or grams) of talc were insufflated, and (4) how many days of drainage before and after talc insufflation?

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To the Editor:

In reply to your questions:

1. Of the 27 patients receiving talc, how many received it by insufflation via large tube? During the period of interest, all patients who underwent talc installation by large tube also underwent insufflation. Eleven patients were in this group.

2. What were the 30-day and long-term successes of these patients? Of the 11 patients, 3 patients had recurrence of pleural effusions within a median follow-up period of 5 weeks (range, 3 to 15 weeks). Eight patients had no recurrence at a median of 8 weeks of follow-up (range, 1 to 94 weeks). We saw no difference in the long-term success rates of the large- vs small-bore groups treated with talc (W. Parulekar, MD; unpublished data; June 2002). Seven patients were followed up for at least 30 days; three patients had a recurrence. One of four patients followed up for at least 100 days had a recurrence.

3. Were patients insufflated with milliliters or grams of talc? This information was not specifically recorded at the time of data retrieval. However, the patients were treated at a tertiary hospital with an active thoracic surgery department; it is a fair assumption that the dose delivered was in keeping with current practice.

4. Is there a record of the days of drainage before and after talc installation? We measured the duration that the tube remained in place. For this small group of patients, the median duration of tube placement was 7 days (range, 3 to 21 days). Information regarding days of drainage relative to talc installation was not retrieved.

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Does Diabetes Predispose to the Development of Multidrug-Resistant Tuberculosis?

To the Editor:

We read with interest the article by Bashar et al [November 2001]. The authors have used “odds ratio” interchangeably with “relative risk” while discussing multidrug-resistant tuberculosis (MDR-TB) among diabetes patients, which is not correct. Among diabetes patients, 13 of the 18 MDR-TB patients were enrolled in directly observed therapy (DOT), compared to only 4 of 10 patients in the control group. Therefore, the enrollment in DOT itself becomes an important confounding factor for studying the incidence of MDR-TB among diabetic patients receiving DOT. Without controlling for this confounding factor, it is not

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