1 negative biopsy finding among 5 adequate biopsies on 5 patients with an endoluminal mass, 3 negative biopsy findings among 8 adequate biopsies on 8 patients with frank mucosal infiltration, 1 negative (it was adequate) biopsy finding in a patient with malignant tracheoesophageal fistula, and 8 negative biopsy findings among 13 adequate biopsies on 13 patients with a rigid protrusion. Even more striking are the results of biopsies after CRT.1 (Table 4). From these data, it is evident that there are a lot of negative biopsy findings also if taken from pathologic tissue (frank mucosal infiltration, tracheoesophageal fistulas, endoluminal mass), and that, also in the experience of Riedel et al,1 are not very reliable. Similar results are reported with brush cytology and washing cytology.1 (Tables 2, 4)

Certainly, if biopsy findings are positive, the airway invasion is sure and a radical resection is impossible. However, Riedel et al1 (Table 5) reported a patient with a “microscopic proof of cancer at bronchoscopy” (that is, we suppose, positive biopsy or brushing findings) and “no airway invasion at surgery.” This is really surprising and shows that biopsies are not reliable.

In conclusion, we think that it is wrong to make any preoperative judgment about radical resectability based on the results of biopsies. These can be useful if considered in an integrated fashion with bronchoscopic findings (distinguishing mobile from rigid protrusion), CT scan, etc.

Alessandro Baisi, MD
Luigi Bonavina, MD
Ospedale Maggiore Policlinico
Milan, Italy

Correspondence to: Alessandro Baisi, MD, Dipartimento di Scienze Chirurgiche, Pad. Monteggia, Osp. Policlinico, Via F. Sforza, 35, 20122 Milan, Italy; e-mail: alessandro.baisi@unimi.it

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To the Editor:

Drs. Baisi and Bonavina, in their comments about our article on bronchoscopic staging of esophageal cancer,1 suggest that it is wrong to make any judgment about radical resectability based on results of biopsies. They cite our findings of some negative biopsies taken from macroscopically abnormal tissue; however, they fail to realize that our decision about resectability was based on a combination of macroscopic findings, biopsies, and brush and washing cytology. With this combined mode of evaluation, the specificity and accuracy was statistically significantly better than it would be if we were to rely on the subjective interpretation of macroscopic findings only. In our series, 18 patients with macroscopic abnormalities, but without microscopic proof of cancer, eventually underwent an R0 resection; they would have been rejected for curative surgery if the diagnosis of airway invasion had been based on macroscopic findings only.

The interpretation of bronchoscopy in the assessment of airway invasion of esophageal cancer after radiochemotherapy is, without doubt, more difficult than at baseline staging; the positive predictive value of macroscopic abnormalities without microscopic proof of cancer is low.1,2 This underlines the importance of biopsies rather than questions their value.

The one patient reported1 (Table 5) with “microscopic proof of cancer at bronchoscopy” and no airway invasion at surgery had normal results of macroscopic examination, negative biopsies, and negative washing cytology: only the results of brush cytology were evaluated as “strongly suspicious of cancer,” as clearly stated in the text of our article. The final decision to operate on this patient was made by the surgeons and was clearly the correct decision. This case certainly does not support the conclusion of Drs. Baisi and Bonavina that biopsies are not reliable.

Martin Riedel, MD
Medizinische Klinik der Technischen Universität
Munich, Germany

Correspondence to: Martin Riedel, MD, I Medizinische Klinik Poliklinik, Klinikum rechts der Isar, Ismaninger Strasse 22, München D-81675, Germany; e-mail: m.riedel@tln.tum.de

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Outpatient Pulmonary Rehabilitation

To the Editor:

We read with great interest the article by Finnerty et al (June 2001).1 This large, randomized, controlled trial showed that, in patients with COPD, outpatient rehabilitation can improve walking distance and health-related quality of life for 12 weeks. These results were obtained in a nonteaching hospital, so the authors suggested that the results of previous studies could now be extrapolated beyond centers dedicated to these regimens. Still, several earlier studies have shown comparable positive results even after rehabilitation in the home setting.2-5 In the present paper, the studies by Wijkstra et al6 and Cambach et al7 were both cited as outpatient rehabilitation programs. However, the patients in these studies received their training from a local physical therapist in the community and were not supervised by hospital staff. Studies with a comparable design, like Strijbos et al8 and Hernandez et al9 also showed that rehabilitation programs carried out in a home setting were beneficial. Thus, rehabilitation programs can be very effective in specific groups of patients with COPD in different settings. If these patients receive adequate training, it can be beneficial not only in an outpatient setting beyond a teaching hospital but even in a setting in the community, with no direct supervision from the hospital.

P.J. Wijkstra, MD, PhD
G.H. Koeter, MD, PhD
University Hospital
Gröningen, The Netherlands

Correspondence to: P.J. Wijkstra, MD, PhD, University of Hospital, Department of Pulmonary Diseases, P.O. Box 30.001, 9700 RB Gröningen, The Netherlands

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To the Editor:

I agree that two of the studies cited in our paper as outpatient-based were, in fact, not supervised by hospital staff but were entirely community-based.1,3 I am entirely in agreement that rehabilitation programs can be very effective in the community, and indeed, while the professional input remains similar to before, our current pulmonary rehabilitation program is conducted entirely in the community, in co-operation with general practitioners.

James P Finnerty, MD
Countess of Chester Hospital,
Chester, United Kingdom

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Spiral CT Is Not the Final Answer

To the Editor:

We enjoyed reading the article by Paterson and Schwartzman in CHEST (June 2001),1 concluding that “spiral CT can replace pulmonary angiography in patients with nondiagnostic V/Q [ventilation/perfusion] scans.” We wish to raise several issues regarding these recommendations.

This conclusion is based solely on a hypothetical model that does not represent actual clinical practice and decision making. We think that adopting their diagnostic approach may not be sufficient to exclude clinically significant pulmonary embolism. Furthermore, this could potentially lead to unnecessary treatment or lack of appropriate anticoagulation.

First, there are known issues surrounding subsegmental pulmonary embolism. It is known that the sensitivity of spiral CT in this area is not high. Relying on spiral CT in these situations may result in missing small peripheral clots and their potential impact on patients with limited cardiopulmonary reserve.

Second, the differences among radiologists in interpreting helical CT, especially in centers with less experience, are considerable. This fact was not discussed in this article or taken into account in their model.

Several investigators have studied the role of the d-dimer test in the workup of pulmonary embolism. We are glad that the authors referred to d-dimer in their discussion. Dabbagh et al2 studied the correlation between spiral CT of the chest and d-dimer latex agglutination test (Accuclot; Sigma Diagnostics; St. Louis, MO) among 79 patients (77% women). They found that a negative d-dimer result (<0.25 mg/dL) highly predicted a negative spiral CT of the chest result (negative predictive value, 100%). We believe that spiral CT scan of the chest might not be necessary in the presence of a negative d-dimer test result by latex agglutination.

Although we believe that spiral CT of the chest can be very helpful in the evaluation of pulmonary embolism, we do not think it is the complete and final answer.

Oussama Dabbagh, MD
Carl Kaplan, MD, FCCP
University of Missouri-Columbia
Columbia, MO

Correspondence to: Oussama Dabbagh, MD, Chief Fellow, Division of Pulmonary, Critical Care Medicine, University of Missouri-Columbia, One Hospital Dr, MA417, Columbia, MO 65212; e-mail: DabbaghO@health.missouri.edu

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Salmeterol Powder Provides Significantly Better Benefit Than Montelukast in Asthmatic Patients Receiving Concomitant Inhaled Corticosteroid Therapy

To the Editor:

I have read with interest the study by Fish et al (August 2001).1 In this study, salmeterol added to inhaled corticosteroids was statistically superior to adding montelukast to inhaled corticosteroids in improving a number of traditional outcome variables such as morning and evening peak expiratory flow (PEF), percent of symptom-free days, percent of rescue-free days, supplemental albuterol use, nighttime awakenings, and some subjective symptoms. Reported daytime wheezing was not different. I am afraid that the design of this study favored this outcome as one of the inclusion criteria was an improvement in FEV1 of at least 12% to 273

2-agonist was predetermined by these entry criteria. On the contrary, it is noteworthy that montelukast also improved the primary efficacy measure, which was PEF. While the authors claim that the sample size per treatment arm provided a power to detect a significant difference of 15 L/min from baseline in the morning PEF, the mean difference between the two treatments observed was only 13.3 L/min. I question the scientific interpretation as well as the clinical significance of their observation. Furthermore, in my view, statistically significant differences such as a reduction of 0.1 nighttime awakenings per week are hardly clinically relevant. Again, I question whether, indeed, salmeterol powder provides better benefit than monte-