Communications to the Editor

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.

Localization of Pulmonary Nodules

To the Editor:

In response to the article of Sugi and colleagues (July 2003),1 we would like to express our opinion about some aspects of their article. We congratulate the authors on the results obtained in their study, but we think that radio-guided surgery is not better than other localization techniques,2 such as intrathoracoscopic ultrasound.

The localization of pulmonary nodules by radio-guided technique has shown to be reliable, but revealing some drawbacks.3 Most important is the notable and fast diffusion of contrast medium in the pulmonary parenchyma surrounding the nodule, due to the rich vascularization of the lung. A second problem is locating deep and posterior nodules due to the dimension and structure of the probe, which cannot move freely in the thorax.

However, the intrathoracoscopic ultrasound technique has been shown, in experienced hands, to be very sensitive for the localization of pulmonary nodules.4 In addition, intrathoracoscopic ultrasound is less expensive than the radio-guided technique, and it allows study of the structures surrounding the nodule (eg, vessels, bronchi, and lymph nodes). Furthermore, intrathoracoscopic ultrasound may play a role predicting the pathology of the nodule. In fact, in all patients the lesion appeared as a homogeneous hypoechoic pattern with the sonographic disappearance of the hyperechoic pulmonary surface. Heterogeneous echogenicity of the lesion was observed in two patients, due to air bronchogram, presence of different tissue, or hamartoma. This ultrasound pattern, however, was not understandable between malignant or benign lesions.4,5 Unfortunately, the radio-guided technique does not give any additional information about the pathology of the nodule.

Other drawbacks of radio-guided techniques are pneumothorax or mild intrapulmonary bleeding.1,6 We are sure that in most cases these complications are nonsymptomatic, but they can negatively influence the health of the patient and the surgical approach; bleeding, for example, can influence the margins at resection.

In conclusion, intrathoracoscopic ultrasound seems to be superior to the radio-guided and finger palpation techniques to locate small pulmonary lesions, but we know that ultrasound is strongly operator dependent. Fortunately, at our institution there are very experienced colleagues in ultrasound imaging. Thus, we are now using ultrasound routinely to localize solitary pulmonary nodules.

Paolo Carcoforo, MD
Carlo Feo, MD
Davide Sortini, MD
Enzo Pozza, PhD
Giovanca Carrella, MD
Andrea Sortini, PhD
University of Ferrara
Ferrara, Italy

REFERENCES

To the Editor:

We received the letter from Carcoforo et al concerning our article published in CHEST (July 2003).1 We would like to express our gratitude for their interest in our article and the kind comments.

In our article,1 we demonstrated that a radioisotope imaging (RI) method was effective in determining the position of small tumors in the periphery of the lung and that of tumors that present as a faint shadow on CT scanning. In a trial using an RI method, we were able to identify tumors in all 25 patients, enabling thoracoscopic resection and a tissue diagnosis for the tumors.

Carcoforo et al responded to our article by saying that ultrasonographic tumor identification is safer than using our RI method and is superior, in that the intratumor ultrasonic pattern...
can be used to distinguish between benign and malignant tumors. The injected RI disperses readily, moreover, making identification difficult, and deeply or dorsally located tumors are particularly difficult to delineate. These criticisms are not, however, correct. We experienced no cases in which a radioisotope that had been injected the day before the procedure dispersed, making tumor identification impossible. Furthermore, the RI probe was able to reach the entire lung surface, and we did not encounter any site-related difficulties with tumor identification. The only complications we encountered that were associated with the RI method were some minor cases of pneumothorax and intrapulmonary hemorrhage in a few patients.

We have not attempted ultrasonographic tumor identification, so we may not properly understand the technique, but we think that there may be a subset of tumors in which identification using ultrasonography is difficult. An example would be bronchioalveolar carcinoma, a small tumor presenting as a faint shadow on CT scans. To identify such a tumor ultrasonographically, it would be necessary to collapse the lung completely. It might be difficult to identify a bronchioalveolar carcinoma, which does not form an obvious mass, even in a completely collapsed lung.

As Carcoforo et al note, tumor identification using ultrasonography requires considerable training and expertise. The RI method is technically simpler and does not require any particular training or expertise. Both the ultrasonographic and RI methods of tumor identification can be considered as options for determining the position of masses that are difficult to distinguish macroscopically during surgery because they are small or present only as a faint shadow radiographically. The choice of method should be made according to the circumstances at each institution, and the expertise and preference of the proceduralist.

Kazuo Sugi, MD, DMedSc
Sanyo National Hospital
Yamaguchi, Japan

Reproduction of this article is prohibited without written permission from the American College of Chest Physicians (e-mail: permissions@chestnet.org).
Correspondence to: Kazuo Sugi, MD, DMedSc, Sanyo National Hospital, Yamaguchi University School of Medicine, Higashikiwa 665, Ube, Yamaguchi, Japan 755-0241; e-mail: ksugi@sanyou.hosp.go.jp

REFERENCES

Term Ambiguity

To the Editor:

The recent article “Ratio Between Forced Expiratory Flow Between 25% and 75% of Vital Capacity [FEF25–75%] and FVC Is a Determinant of Airway Reactivity and Sensitivity to Methacholine” by Parker et al (July 2003) is confusing and perhaps misleading. Whereas the term vital capacity is in the title and suggests slow vital capacity, FVC is used in the ratio with FEF25–75%. Thus, ambiguity is suggested.

Of greater importance is the fact that the ratio between FEF25–75% and FVC involves two numbers, a numerator and a denominator. Thus, the reduction of the ratio could be a reduction in FEF25–75% or an increase in FVC (although unlikely). The authors do not mention whether they measured the FEF25–75% under isovolume conditions (ie, from the same part of the FVC as the prebronchodilator FEF25–75%). Of greater concern is the omission of the FEV1/FVC ratio. This ratio is not given for comparison of the FEF25–75%/FVC ratio. The absolute response of FVC to methacholine is also not given in the article. Thus, the article presents many unanswered questions that require elaboration.

For some time, I have been trying to get rid of the FEF25–75% as being unnecessary and misleading. The most common complaint I receive even from board-certified internists at medical meetings is that spirometric values are difficult to interpret. Previous studies have shown that the FEF25–75% and other “sensitive tests” for small airways dysfunction were not predictors of decline in FEV1 or the emergence of clinical illness.

The National Lung Health Education Program recommends using only two parameters (FEV1 and FVC) or a surrogate (forced expiratory volume in 6 s) for the spirometric assessment of lung function. Testing for early stages of disease, of course, is somewhat different than methacholine challenge, but comparing the FEV1 number with a ratio that has two determinants seems inappropriate.

Thomas L. Petty, MD
University of Colorado Health Sciences Center
Denver, CO

Reproduction of this article is prohibited without written permission from the American College of Chest Physicians (e-mail: permissions@chestnet.org).
Correspondence to: Thomas L. Petty, MD, Health ONE Center, 1850 High St, Denver, CO 80218; e-mail: nlhep@aol.com

REFERENCES
1 Parker AL, Abu-Jilieh M, McCool FD. Ratio between forced expiratory flow between 25% and 75% of vital capacity and FVC is a determinant of airway reactivity and sensitivity to methacholine. Chest 2003; 124:63–69

To the Editor:

We thank Dr. Petty for his interest in our article (July 2003). In regard to the ambiguity of the title, the original title of the article was “FEF25–75% [forced expiratory flow between 25% and 75% of vital capacity]/FVC Ratio Is a Determinant of Airway Reactivity and Sensitivity to Methacholine.” The change to its present title was an editorial decision.

The FEF25–75%/FVC ratio as an indicator of disproportionately small airways for a given lung size, was derived from the description by Mead and colleagues of airway-lung size dysanapsis. This was manifested as an inverse relationship between vital capacity (VC) and the product of the ratio of maximal flow at 50% of VC divided by VC and the static recoil pressure of the lung at 50% of VC (Vmax50/VC × Pst[L] 50). The FEF25–75%/FVC ratio may be reduced either by reducing the FEF25–75% or by increasing the FVC. Since this ratio is used to gain insight into the presence of dysanapsis, it does not matter whether the ratio is low because the person has small airways with a normal-sized lung (a