

LETTERS TO THE EDITOR

In our opinion, however, to prevent any risk of confusion that might lead to undertreatment in candidate patients it might be best not to call any of these promising techniques currently under investigation “segmentectomy.”

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Response

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We thank Dr Seror and colleagues for an in-depth review of our article and welcome the opportunity to respond to their queries and comments (1).

With regard to their concerns about nomenclature, we agree that any confusion on the part of patients or physicians regarding the use of radiation segmentectomy as an equivalent substitute for surgical segmentectomy would not be acceptable. We believe that the risk of confusion related to the term “segmentectomy” as it pertains to both transarterial and surgical therapies is nearly nonexistent when practicing in a multidisciplinary setting with hepatologists, liver transplant surgeons, interventional radiologists, and radiation oncologists. In our experience, the misnomer of radio- “embolization” when using 20–40- μ m glass microspheres as well as the general perception—among referring and sometimes even treating physicians—that all transarterial therapies are roughly equivalent are far more common. Although the study of radiation segmentectomy is still arguably in its early stages, clinical inquiries showing high rates of complete pathologic necrosis when doses are administered in excess of a “tumor threshold” imply that this may not be the case (2). In this light, we assert that the potential benefits of raising awareness about the possibility of radiation segmentectomy as a treatment option in patients who are not candidates for either surgery or ablation may very well outweigh the supposed risk of confusion regarding this term leading physicians or patients to believe that this endovascular treatment option is another surgical therapy.

Last, we agree with Dr Seror and colleagues’ assertion that the technique

of radiation segmentectomy is promising, and we eagerly look forward to further studies examining its efficacy.

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Inverted Mucoïd Impaction Signal in Allergic Bronchopulmonary Aspergillosis: Are We Calling a Spade a Big Spoon?

From

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Editor:

We read with interest the study by Dr Dournes and colleagues, which was recently published online in *Radiology*, wherein the authors report a sensitivity and specificity of 94% and 100%, respectively, for the so-called inverted mucoïd impaction signal (IMIS) sign on unenhanced magnetic resonance (MR) images for the diagnosis of allergic bronchopulmonary aspergillosis (ABPA) complicating cystic fibrosis

(CF) (1). We applaud the authors for using a noninvasive test that is free of both contrast material and radiation for the diagnosis of ABPA. However, a few issues require further clarification.

In their study, Dr Dournes and colleagues report that 13 of the 18 patients with ABPA had high-attenuation mucus at computed tomography (CT) of the chest. The prevalence of high-attenuation mucus is unusually high, suggesting that the study recruitment was not consecutive. Most importantly, the mean attenuation of mucus was 86 HU (95% confidence interval: 73 HU, 121 HU) on CT scans of the chest, whereas the attenuation of the skeletal muscle was 49 HU (95% confidence interval: 43 HU, 54 HU), indicating that all patients with ABPA in the current study had high-attenuation mucus. In fact, we have previously proposed that the objective criterion for the diagnosis of high-attenuation mucus is attenuation higher than 70 HU (2). Thus, to our mind, the IMIS sign is not anything novel but only represents the MR finding of high-attenuation mucus seen at chest CT. Finally, the study included only 18 subjects with ABPA, thus making the study underpowered to make any meaningful conclusions, especially with regard to sensitivity.

Ideally, Dr Dournes and colleagues could have recruited consecutive subjects and investigated the diagnostic performance of the IMIS sign in hypo- and hyperattenuated mucus (3). This would have provided the true utility of the IMIS sign in the diagnosis of ABPA.

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Response

From

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We thank Dr Agarwal and colleagues for their interest in our study on ABPA in CF. Their concern that we may have examined nonconsecutive patients just does not correspond to the design of our study, which was prospective and did include consecutive patients. In the very special condition of CF, the unexpected sensitivity of imaging to diagnose ABPA is largely developed in the Discussion of our article (1). In addition, Dr Agarwal and colleagues may have misunderstood figures showing that, whereas ABPA and no ABPA groups overlapped by using CT, they did not by using MR imaging. Therefore, the threshold of normalized at-

tenuation between mucus and muscle was found herein greater than 1.7 HU and not greater than 1 HU. More important, there is large variability and uncertainty in the CT literature in defining a quantitative threshold. In this connection, Dr Agarwal and colleagues have proposed 70 HU in patients without CF (2), whereas Occelli et al (3) indicated 36 HU in patients with CF. Indeed, CT attenuation values are largely dependent on acquisition parameters. Using MR imaging, we found that contrasts were visually obvious, from white to dark; thus, no additional quantitative measurement was deemed necessary. Because 18 patients with ABPA were included, the study was powered enough to assess a 95% confidence interval of sensitivity of 73% to 99%. Finally, of the 108 patients with CF, all 18 patients with ABPA improved under appropriate treatment, supporting the utility of MR imaging as a radiation-free tool during lifelong management. As Garg and colleagues recently underlined (4), we confirm that MR imaging can be considered a new paradigm in the imaging evaluation of ABPA in CF (5).

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Limitations of Focused Assessment with Sonography in Trauma (FAST) Protocols in Transthoracic US

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Editor:

We read with great interest the review by Drs Richards and McGahan in the April 2017 issue of *Radiology* (1). We really appreciated the lung ultrasonography (US) section, where the authors pointed out the real value of the absence of “sliding sign.” Indeed, they listed most of the false-positive findings; they also outlined that the subcutaneous emphysema, frequently associated with pneumothorax, does not allow the ultrasound beam to reach the pleural line (1).

On the other hand, Drs Richards and McGahan declare that the presence of “comet tails” and B-lines (namely ring-down artifacts) “occur only when the lung surface can be reached by sound waves, thus represent[ing] a reassuring finding.” In our experience, and as previously reported (2), the presence of ring-down artifacts cannot reassure the physician, and the exact “length” of ring-down cannot be related to the presence of pneumonia or pulmonary edema, mainly because these kind of artifacts depend also on US machine setting (3,4).

Moreover, Drs Richards and McGahan recommend the use of a sector probe as a “multipurpose tool” (1). We do not agree with this statement because the use of a sector probe increases the creation of artifacts, such as ring-down artifacts (3).

Drs Richards and McGahan also state that the use of “lung point” can be used to estimate the size of pneumothorax (1). As previously demonstrated (4,5), and as stated by British Thoracic Society guidelines, it is not advisable to use only US to assess the presence and size of both pneumothorax and pneumomediastinum because only when these are visible with chest radiographs and computed tomographic (CT) scans is any intervention required (6,7). Besides, the “sliding sign” cannot be detected in the apex, so apical pneumothorax is not only “more challenging” (1) but is impossible to detect (3).

Another important issue is the specific training required for transthoracic US. It has already been reported that “before applying this technology in clinical practice, it is necessary to ensure that all practitioners are well trained and supervised while they are early in their experience” (8). We totally agree with this statement; as the whole medical practice, US cannot be performed by an improperly trained operator.

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We thank Dr Frongillo and colleagues for their correspondence regarding our review, “Focused Assessment with Sonography in Trauma (FAST) in 2017: What Radiologists Can Learn” (1). Our comment regarding the reassuring finding of comet tails and B-line artifacts should have been further clarified, as it was directed specifically to patients with acute trauma for whom extended