practice, and have some of the initiatives they have undertaken redounded to the detriment of physicians and patients?

Fundamental questions deserve careful reflection and wide debate. Is there a direct or inverse correlation between the time and effort required to meet accreditation requirements and the quality of educational programs? Assuming that some regulation is good but a lot of regulation is bad—a graph of this function would demonstrate an inflection point—how would we know when we were approaching or had passed that point? What is the quality of the evidence on which accrediting organizations base their dicta, and what efforts are they making to collect, analyze, and disseminate the results? No one contests that oversight is needed. But mere oversight is not good oversight, nor is more oversight necessarily better oversight.

Perhaps the overseers should devote less effort to telling bright, creative, and energetic educators, who are busier and operating under tighter resource constraints than ever before, what they must do and how they must do it and instead devote a bit more effort to listening, learning, and disseminating best practices. Does anyone seriously think that the vast majority of radiology residency and fellowship programs are defaulting on their fiduciary responsibility to educate qualified physicians who will provide good patient care? We need to be reaching for the stars, not proving that we are standing on the ground. Who oversees the overseers?

Accuracy of Diffusion-weighted MR Imaging for Differentiation of Pulmonary Lesions

From
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Editor:

I read with interest the article by Dr Uto and colleagues (1) in the July 2009 issue of *Radiology*, which examined the utility of the lesion-to–spinal cord ratio (LSR) for characterization of pulmonary lesions on high–*b* value diffusion-weighted (DW) magnetic resonance (MR) images. The authors deserve to be congratulated for introducing such a practical method. However, some issues need to be addressed to help readers better understand the study.

The title of the article is misleading because the specificity (90%) is the same for apparent diffusion coefficient (ADC) and LSR measurements. "Higher accuracy" instead of "higher sensitivity and specificity" would be more correct.

Furthermore, there are miscalculations of the predictive values of LSR for a cutoff value of 1.135 in the Results and in table 2. The positive and negative predictive values should be 93.8% (15 of 16) and 75% (nine of 12), respectively.

I think the lower accuracy of ADC (50% vs 85.7% for LSR) in their study may have resulted from the small patient population with a high proportion (11 of 18) of adenocarcinoma. In a larger series encompassing 140 pulmonary lesions, Mori et al (2) reported that DW MR imaging has accuracy (76%) comparable to that (74%) of positron emission tomography with a *b* value of 1000 sec/mm^2. However, with an ADC cutoff value of 1.1×10^{-3} mm^2, 39.6% (21 of 53) of well-differentiated adenocarcinomas had negative findings on DW MR images in their series, compared with 4.7% (one of 21) of squamous cell carcinomas. Tumor cellularity is lower in well-differentiated adenocarcinoma, accounting for less restriction in diffusion and resulting in higher ADC values (3). Adenocarcinoma was also identified as a cause of three false-negative cases when using LSR in the series of Dr Uto and colleagues (1).

References

1. Uto T, Takehara Y, Nakamura Y, et al. Higher sensitivity and specificity for diffusion-weighted imaging of malignant lung lesions without apparent diffusion coefficient quantification. Radiology 2009;252(1):247–254.
2. Mori T, Nomori H, Ikeda K, et al. Diffusion-weighted magnetic resonance imaging for diagnosing malignant pulmonary nodules/masses: comparison with positron emission tomography. J Thorac Oncol 2008;3(4):358–364.
3. Matoba M, Tonami H, Kondou T, et al. Lung carcinoma: diffusion-weighted MR imaging—preliminary evaluation with apparent diffusion coefficient. Radiology 2007;243(2):570–577.

Response

From
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We thank Dr Karabulut for pointing out some miscalculations that appeared in our article (1). His statement is reasonable that the positive and negative predictive values should be 93.8% (15 of 16) and 75% (nine of 12), respectively. The title should have been "Higher Sensitivity and Comparable Specificity for Diffusion-weighted Imaging of Malignant Lung Lesions without Apparent Diffusion Coefficient Quantification." Although he was concerned about the title, we feel our core message will be understood by readers of *Radiology*. To date, some studies (2–4) have shown that DW imaging is important in helping to diagnose various cancers, and most of the studies have used the ADC. However, current techniques for calculating ADC are not sufficiently mature to provide consistent values, particularly in the torso area. This is mainly because of the inherent problems of echo-planar imaging (we will not repeat the individual problems in this reply). The core problem we intended to address in our study is the inconsistency in ADC calculation with

current techniques. If the performance is comparable, our simpler and more practical method will be preferred by radiologists.

Dr Karabulut also provided important information to us by referring to the study of Mori et al (5). Differentiation of adenocarcinoma with DW imaging is challenging because of the low cellularity in the tumor. Dr Karabulut suggested that the improved results with our method might be owing to the small patient population and the high proportion of adenocarcinomas. The improved differentiation of adenocarcinoma that we reported may reflect the robustness of our new index, LSR, as compared with ADC. We suggest that Mori et al should measure LSR and assess its performance in comparison to ADC.

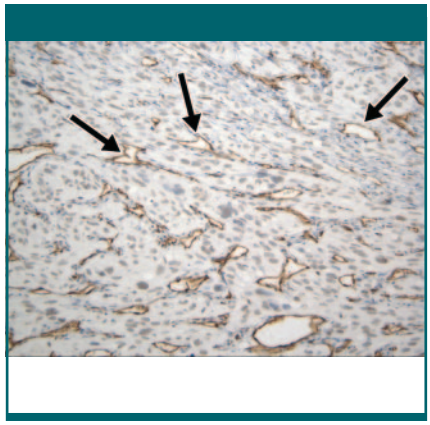
References

1. Uto T, Takehara Y, Nakamura Y, et al. Higher sensitivity and specificity for diffusion-weighted imaging of malignant lung lesions without apparent diffusion coefficient quantification. Radiology 2009;252(1):247–254.
2. Guo Y, Cai YQ, Cai ZL, et al. Differentiation of clinically benign and malignant breast lesions using diffusion-weighted imaging. J Magn Reson Imaging 2002;16(2):172–178.
3. Issa B. In vivo measurement of the apparent diffusion coefficient in normal and malignant prostatic tissues using echo-planar imaging. J Magn Reson Imaging 2002;16(2):196–200.
4. Taouli B, Vilgrain V, Dumont E, Daire JL, Fan B, Menu Y. Evaluation of liver diffusion isotropy and characterization of focal hepatic lesions with two single-shot echo-planar MR imaging sequences: prospective study in 66 patients. Radiology 2003;226(1):71–78.
5. Mori T, Nomori H, Ikeda K, et al. Diffusion-weighted magnetic resonance imaging for diagnosing malignant pulmonary nodules/masses: comparison with positron emission tomography. J Thorac Oncol 2008;3(4):358–364.

Errata

"Does Arterial Spin-labeling MR Imaging–measured Tumor Perfusion Correlate with Renal Cell Cancer Response to Antiangiogenic Therapy in a Mouse Model?" Radiology 2009;251(3):731–742

On page 735, Figure 3, the left image in the third row is incorrect. The correct image is shown below, and arrows indicate copious microvasculature.



"Pulmonary Embolism Detection with Dual-Energy CT: Experimental Study of Dual-Source CT in Rabbits." Radiology 2009;252(1):61–70

Page 62, the second sentence of the second paragraph should read as follows: In technologic terms, it is difficult to evaluate microcirculation of the whole lung with earlier-generation CT scanners, including single-source **64-section** CT units, because of postprocessing time and misregistration artifacts between two spiral scanners (8–14).

Page 63, the first sentence under "Image Reconstruction and Analysis" should read as follows: From the raw spiral projection data acquired with both x-ray tubes, images were automatically reconstructed into three data sets (an 80-kVp data set, a 140-kVp data set, and a data set of fused images with **30%** attenuation information from the 80-kVp scan and **70%** attenuation information from the 140-kVp scan) with a 0.75-mm section thickness and a 0.50-mm section interval (33% overlap).

Page 68, left column, the first full sentence should read as follows: Our pilot study with rabbits showed that **BF imaging alone had a diagnostic sensitivity of 89% in the detection of PE and had good agreement with pathologic analysis.**

"Carotid Artery Brain Aneurysm Model: In Vivo Molecular Enzyme-specific MR Imaging of Active Inflammation in a Pilot Study." Radiology 2009;252(3):696–703

Page 696, the fourth sentence of Materials and Methods should read as follows: After intraarterial injection of an MPO-specific (di-5-hydroxytryptamide of gadopentetate dimeglumine, 0.1 mmol per kilogram of bodyweight) or a non–MPO-specific (**dityramide** of gadopentetate dimeglumine, 0.1 mmol/kg) contrast agent, animals underwent 3-T MR imaging.

Page 696, the third sentence of Results should read as follows: In inflamed aneurysms, di-5-hydroxytryptamide of gadopentetate dimeglumine exhibited delayed washout kinetics compared with the kinetics of **dityramide** of gadopentetate dimeglumine.

Page 698, the first sentence under "MPO-specific and Non–MPO-specific Contrast Agents" should read as follows: An author (A.A.B., 18 years of experience) performed synthesis of MPO-specific (di-5-hydroxytryptamide of gadopentetate dimeglumine) and non–MPO-specific (**dityramide** of gadopentetate dimeglumine) MR contrast agents, as described previously (24,25), with slight modifications.

Page 698, right column, the third full sentence should read as follows: Animals were then injected with a sterile solution of 0.1 mmol/kg of di-5-hydroxytryptamide of gadopentetate dimeglumine or **dityramide** of gadopentetate dimeglumine in 15 mL of 5% meglumine with a pH of 7.

Page 698, right column, the fifth full sentence should read as follows: Time course experiments were performed up to 330 minutes after contrast agent injection with the same imaging parameters described previously after administration of either di-5-hydroxytryptamide of gadopentetate dimeglumine or **dityramide** of gadopentetate dimeglumine in animals with aneurysms into which LPS had been injected.

Page 699, the first sentence under "Sensitivity and Specificity for MPO in Inflamed Aneurysms" should read as follows: The kinetics of enhancement of the aneurysm and left CCA with both di-5-hydroxytryptamide of gadopentetate dimeglumine and **dityramide** of gado-

pentetate dimeglumine are shown in Figure 3.

Page 699, the third sentence under "Sensitivity and Specificity for MPO in Inflamed Aneurysms" should read as follows: Both the inflamed aneurysm and the left CCA in the animal that received **dityramide** of gadopentetate dimeglumine had similar enhancement ratios measured at each point of the time course.

Page 701, the caption for figure 3 should read as follows: **Figure 3:** Graphs show kinetics of enhancement of **(a)** a representative LPS-injected aneurysm and **(b)** the left CCA. The enhancement ratio (*ER*) of di-5-hydroxytryptamide of gadopentetate dimeglumine (*di-5-HT-GdDTPA*) is compared with that of **dityramide** of gadopentetate dimeglumine (*di-Tyr-GdDTPA*).

Page 702, left column, second paragraph, the third sentence should read as follows: We compared the observed enhancement ratio with that of **dityramide** of gadopentetate dimeglumine, which is a contrast agent that is structurally similar to di-5-hydroxytryptamide of gadopentetate dimeglumine and has been demonstrated in vitro to be activated by peroxidases but not by MPO (24).