Title
Dr Tang and colleagues respond

Journal
Radiology, 168(1)

ISSN
0033-8419

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Publication Date
1988-07-01

DOI
10.1148/radiology.168.1.284-c

Peer reviewed

usually visualized as a wedge-shaped configuration, and its attenuation is usually higher than that of true space-occupying lesions. Therefore, uneven opacification of liver parenchyma would be correctly diagnosed after much experience with AP-CT.

In any case, I think that the readers of *Radiology* may find it difficult to understand the low sensitivity of AP-CT because the contrast between the lesions and liver parenchyma is most definite on AP-CT scans compared with that seen on DS-CT and EOE-CT scans, as shown in Figures 3 and 4. I hope that Miller et al reevaluate their data after clarifying their diagnostic criteria and performing AP-CT in many more cases.

References

1. Miller DL, Simmons JT, Chang R, et al. Hepatic metastasis detection: comparison of three CT contrast enhancement methods. Radiology 1987; 165:785–790.
2. Matsui O, Takashima T, Kadoya M, et al. Liver metastases from colorectal cancers: detection with CT during arterial portography. Radiology 1987; 165:65–69.

■ *Dr Miller responds:*

I thank Dr Matsui for his letter. He has extensive experience in evaluation of the liver for metastatic disease and has clearly read our article (1) carefully. He raises some interesting points.

1. The criteria used for interpreting all three studies were the same. We looked for focal areas of decreased attenuation. Obvious nonmalignant lesions were not counted. Equivocal lesions were considered positive; if subsequently shown to be benign histologically, they were considered false positive. In Figure 3, the cyst was not identified as a false-positive lesion on DS-CT or EOE-CT scans because it was not detected among the background of low-attenuation vessels. The area of fibrosis was considered a false-positive lesion on EOE-CT scans. Because of the nature of the study, radiologists interpreting individual scans did not have access to results of any other imaging studies. As Dr Matsui notes, correlation with other imaging studies is usually extremely helpful and may allow exclusion of some false-positive lesions.

2. Just as Dr Matsui has extensive experience in the use of AP-CT, we have extensive experience in the use of EOE-CT and DS-CT. We have in fact demonstrated that the sensitivity of DS-CT and EOE-CT decreases for lesions under 1.5 cm in diameter (1–3), but in this study opacification of the hepatic vasculature during AP-CT did not increase sensitivity or decrease the false-positive rate compared with those achieved with DS-CT or EOE-CT.

3. I agree that uneven opacification is a major problem with AP-CT. It is noteworthy that this does not occur with DS-CT or EOE-CT. While flow-related defects are usually wedge-shaped, they need not always have this configuration, as shown in our Figure 5. Obviously, extensive experience with AP-CT is helpful, but some lesions will always be a problem. Incidentally, we were quite careful not to wedge the tip of the angiographic catheter or direct it into a side branch of the superior mesenteric artery.

Dr Matsui concludes that the reader might find it difficult to understand the low sensitivity of AP-CT because lesion-liver contrast is very high as shown in our Figures 3 and 4. I would stress that these figures show false-positive lesions and not tumor. This was proved histologically, and I think Dr Matsui's comment reiterates our point that AP-CT has an unacceptably high false-positive rate. It is for this reason, and not because of their sensitivity, that we prefer DS-CT and EOE-CT for the evaluation of hepatic metastases.

References

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2. Reinig JW, Dwyer AJ, Miller DL, et al. Liver metastasis detection: comparative sensitivities of MR imaging and CT scanning. Radiology 1987; 162:43–47.
3. Sugarbaker PH, Vermess M, Doppman JL, Miller DL, Simon R. Improved detection of focal lesions with computerized tomographic examination of the liver using ethiodized oil emulsion (EOE-13) liver contrast. Cancer 1984; 54:1489–1495.

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Musculoskeletal Infection of the Extremities: Evaluation with MR Imaging

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Editor:

We were puzzled by several of the conclusions drawn by Tang et al (1) in their article in the January 1988 issue of *Radiology*, which described magnetic resonance (MR) imaging findings in musculoskeletal infection of the extremities. As the authors noted in their Results section, the experiment lacked several design features necessary to evaluate sensitivity and specificity meaningfully. It is therefore surprising to see the conclusion drawn that "MR imaging had high sensitivity and specificity in the detection of active infection in bone, even in cases of chronic osteomyelitis with or without previous surgery or fracture." The article provided no information from which this conclusion can be drawn. No statistically meaningful conclusions can be drawn from the data presented because of the described limitations in experimental design (2).

The authors stated that both T1- and T2-weighted images are necessary to evaluate musculoskeletal infection because of the nonspecific appearance on T1-weighted images. Thus, the three patients studied with only one pulse sequence should presumably have been excluded from consideration, since it is unclear how a diagnosis for these patients was reached on the basis of MR imaging data. In cases in which surgical proof was lacking, correlation with clinical course and other available imaging studies was used as "proof" (method not described), despite the fact that the authors repeatedly referred to the limitations of these procedures. The abstract indicated that "MR images provided more accurate and detailed information regarding the extent of involvement than did radionuclide bone scans, computed tomographic scans, or standard radiographs." No information about pathologically proved extent of disease was provided. Clearly, the method being studied cannot be presented as its own proof.

While these results are interesting, they do not support modification of currently accepted protocols for the evaluation of osteomyelitis.

References

1. Tang JSH, Gold RH, Bassett LW, Seeger LL. Musculoskeletal infection of the extremities: evaluation with MR imaging. Radiology 1988; 166:205–209.
2. Scott JA, Phillips WC, Blasczcynski GM. Statistics for diagnostic procedures. AJR 1983; 141:409–411.

■ *Dr Tang and colleagues respond:*

We thank Drs Scott and Palmer for their response to our article. We agree with them that our results, although promising, are only preliminary and require confirmation by studies on a larger scale and with a more standardized protocol. We also agree that the limitations of our study prevent us from making a meaningful determination of the sensitivity and specificity of MR imaging in the diagnosis of osteomyelitis.

We are not promoting MR imaging, but merely exploring a potential clinical use. We realize that more data are needed about its efficacy in the diagnosis and

management of osteomyelitis. Because MR imaging is relatively nonspecific, we have always emphasized the need to interpret its findings in the light of those of other imaging modalities, especially plain radiography.

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Transthoracic Needle Aspiration Biopsy: Evaluation of the Blood Patch Technique

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Editor:

In their article in the January 1988 issue of *Radiology*, Bourgouin et al (1) reported their incidence of pneumothorax after transthoracic needle aspiration biopsy performed with a coaxial system. They reported that pneumothorax occurred in 28.8% of the patients (15 of 52) in whom the blood patch technique had been used (group A) and in 34.1% (30 of 88) of those who underwent biopsy without this technique (group B). Chest tube insertion was required in 7.7% of the first group and 9.1% of the second. The authors concluded that, in their series, the blood patch technique failed to affect the frequency of postbiopsy pneumothorax.

Our first 55 biopsies performed with the blood patch technique in 50 patients were reviewed. Pneumothorax occurred in only three of the procedures (5.5%), and chest tube insertion was not needed in any of the cases. No patient experienced significant hemoptysis or any other complication.

Our technique is similar to that used by Bourgouin et al. A preliminary chest radiograph was obtained. A coaxial needle system with a 19-gauge introducing needle and a 22-gauge aspiration cannula was used for all biopsies. The patients were not sedated before the biopsy. After installation of a local anesthetic, the 19-gauge needle was directed toward the target during quiet breathing. The pleura was punctured only once. Upon completion of the biopsy, 5–10 mL of autologous clotted blood was injected through the introducing needle as it was withdrawn.

Our technique varied as follows. The patients were usually supine or prone, but occasionally an oblique or decubitus position was employed, either for patient comfort or for better access to the lesion. All biopsies were performed under computed tomographic guidance (not fluoroscopy). The 19-gauge needle was advanced to or into the periphery of the lesion (not 1 cm from it). Aspiration was performed through the 22-gauge needle with a 20-mL dry glass syringe (not a 6-mL syringe filled with 1 mL of saline) and was repeated until sufficient material was obtained, usually three or four times. Our postbiopsy management also differed. The patient was kept in the horizontal position, usually supine, for at least 1 hour. Within 5 minutes after the biopsy was completed, a supine anteroposterior expiration chest radiograph was obtained, and this was repeated 1 hour later. If the patient was ambulatory, he or she was then permitted to ambulate. Four to 6 hours after biopsy, expiration chest radiography was performed with the patient erect. Additional radiographs were occasionally obtained as clinically indicated.

The reason for our low complication rate compared with that of Dr Bourgouin's group is not readily evident, since our technique does not differ greatly from theirs. Keeping the patients quiet in a horizontal position for 1 hour after the biopsy may contribute to better results. It is true that the supine radiographs obtained during that hour are not as sensitive in the detection of pneumothorax as an erect radiograph. However, at least one upright expiration radiograph was obtained in all our patients within the 6 hours after the biopsy, so we should have detected any significant pleural air. Other differences in our technique described above might also contribute to the low complication rate, or perhaps there is some other factor that we have not identified. We did not document the incidence of emphysema in our patients, but ours was an older population in which a significant incidence of emphysema would be anticipated.

In view of our present experience, we continue to advocate use of the blood patch technique to prevent pneumothorax.

Reference

1. Bourgouin PM, Shepard JO, McLoud TC, Spizarny DL, Dedrick CG. Transthoracic needle aspiration biopsy: evaluation of the blood patch technique. Radiology 1988; 166:93–95.

■ *Drs Shepard and McLoud respond:*

We commend Dr Surprenant for his low complication rates, including a pneumothorax rate of 5.5% (three of 55) and a chest tube insertion rate of 0% (zero of 55). However, we dispute his advocacy of the blood patch technique for the prevention of pneumothorax based on his series of 55 biopsies.

Dr Surprenant states that his was "an older population in which a significant incidence of emphysema would be anticipated." However, he did not specifically determine the prevalence of emphysema or indicate the depth of the lesions that were sampled in his patient population. We contend that our series comprised a large number of high-risk patients. Radiographic evidence of emphysema was present in 19.6% (nine of 46) in group A and in 21.7% (18 of 83) in group B.

Furthermore, Dr Surprenant's series did not include a control group. Without a control group he cannot correctly attribute the lower complication rate to the blood patch technique. Our series included a control group in which both the prevalence of emphysema and depth of the lesions were similar. Because Dr Surprenant's series did not include a control group and because the risk factors in his patients are not stated, we believe that his advocacy of the blood patch technique is unsound.

We do have reason to believe that Dr Surprenant's postbiopsy care may have contributed in part to his better results. Dr Surprenant kept his patients in the horizontal position for 1 hour after the biopsy. In our series the patients underwent erect chest radiography within 10–15 minutes of the biopsy. Asymptomatic patients were allowed to remain upright in bed or to walk.

Recently, we too have adopted a similar postbiopsy routine. Our patients now remain supine or prone for 30–45 minutes after the biopsy, pending a rapid cytologic interpretation. After an upright posteroanterior chest radiograph is obtained, they remain in the horizontal position for an additional hour. In a review of our last 242 cases managed in this manner, we have found that although our pneumothorax rate has remained the same, 31.8% (77 of 242), the chest tube insertion rate has declined to 2.5% (six of 242). (Approximately 75% of the biopsies entailed a single pass with a 22-gauge aspiration cannula or the use of a coaxial technique.)

In comparing Dr Surprenant's technique with our own reported series, the only significant difference that is readily apparent is the method of postbiopsy care. Keeping patients horizontal for at least an hour after the biopsy appears to contribute to a lower incidence of pneumothorax for which chest tube insertion is needed.

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