Prone Versus Supine Thallium-201 Myocardial SPECT

TO THE EDITOR: I read with much interest the paper by Segall and Davis (*1*). Although the authors obviously could not refer to our recent paper on the same subject (*2*) when their paper was accepted, even at that time it was possible to mention at least two abstracts comparing prone to supine myocardial single photon emission computed tomography (SPECT): one by Esquerre et al. (*3*) and one by Segall and Davis (*4*), both presented at the SNM San Francisco meeting in June 1988.

This being said, based on our experience now including over 1,500 prone myocardial studies, we are convinced as are Segall and Davis that myocardial SPECT should be systematically performed using the prone position. I think that both papers are very convincing about that. I would simply make a few comments and ask a question.

From our experience, we have noticed as Eisner et al. (*5*) that, in supine position, diaphragmatic attenuation, though possible in both sexes, is much less frequent in females. On the contrary, anterior wall attenuation is slightly greater because female breast attenuation is generally more important than male pectoral muscle so we also now use bull's-eye analysis and have generated mean bull's-eyes for normal males and females: very subtle differences remain but prone SPECT significantly minimizes gender differences by minimizing both diaphragmatic attenuation in men (and women if necessary) and breast attenuation in women because prone position, as said by authors, allows an optimal positioning of breast. We also think that upward creep is less frequent or less important in prone position may be because of a lesser heart freedom of motion due to lesser antero-posterior respiratory motion.

The authors say that subjective evaluation of image quality showed a slight advantage to supine imaging. This was also our opinion when we first used this technique using a conventional SPECT imaging table. This is due to the fact that, in prone position, most of the projections are acquired through the imaging table which introduces attenuation and scattered radiations especially for the low-energy X radiations of thallium-201. This problem may be particularly important in redistribution studies. We have solved it by designing a special imaging table (*2*) which suppresses almost completely attenuation over the 180° rotation and allows a contour mode rotation very close to the patient chest which lies on a taut cloth window. This (patented) table allows optimal imaging quality.

The authors find a small decrease in anterior and lateral wall activity in prone images obtained 4 hr after exercise. We do not; on the contrary, our normal redistribution polar map is slightly more homogeneous than the stress one. We assume that the authors observations might be due to the problems caused by the imaging table attenuation and scattered radiations which are more critical in redistribution studies because of less favorable counts statistics.

Finally, the authors construct their polar map from 16 contiguous short axis slices without correction for differences in cardiac size:

1. How do they perform the polar representation of apex which look very well done on their bull's-eyes?

2. What is their opinion about the method designed by Michael Goris which seems to be an elegant way to solve the problem of apex polar imaging (despite the risk of overestimating its activity) and which automatically corrects to a large extent the problem of different cardiac size among patients?

References

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5. Eisner RL, Tamas MJ, et al. Normal SPECT thallium 201 Bull's eye display: gender differences. *J Nucl Med* 1988; 29:1901–1909.

J. P. Esquerre
Hopiteaux de Toulouse
Toulouse, France

REPLY: We thank Dr. Esquerre et al. for mentioning the two reports comparing prone to supine myocardial single photon emission computed tomography imaging which were omitted from the reference list in our paper. We would also like to note a third omission, an early case report by Segall et al. (*1*)

The cloth window is an imaginative approach to minimizing attenuation and scatter of myocardial photons by the imaging table when patients are studied prone. However, image quality is only slightly worse in ~13% of patients when imaged prone as compared to supine on a conventional table. This difference, as well as the small decrease in anterior and lateral wall activity seen on delayed images, does not significantly affect the improved diagnostic accuracy of prone imaging without a special table.

Our polar maps were constructed from 16 consecutive short-axis slices beginning with the slice nearest the apex showing ventricular cavity. We used commercially available software despite problems with partial volume effect, image registration, and different cardiac size because this technique is widely used. Since these errors are systematic, prone, and supine images are equally affected. The method polar sampling devised by Goris et al. (*2*) is an elegant way to overcome these limitations and would be a better technique for creating a normative data base.

References

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single photon emission tomography. *Am J Physiol Imag* 1987; 2:176–180.

G. M. Segall
M. J. Davis
VA Medical Center
Palo Alto, California

Bone SPECT Evaluation of Patients with Persistent Back Pain Following Lumbar Spinal Fusion

TO THE EDITOR: A recent article by Lusins et al. (*1*) discussed the usefulness of single photon emission computed tomography (SPECT) when evaluating patients with the "failed back syndrome". Their patient population included those with laminectomy alone and in combination with spinal fusion. Although their observation that the role of lumbar spine scintigraphy has been limited, they neglected to comment on the results of the recent study by Slizofski et al. (*2*) which specifically addressed the issue of pseudoarthrosis, continued low back pain and scintigraphic appearances of both planar and SPECT examinations in postfusion patients.

That study demonstrated a reasonably high sensitivity and specificity (0.78 and 0.83) of bone scanning for determining if the pain is actually related to a pesudoarthrosis. The incidence of a pseudoarthrosis as the apparent cause for pain was higher than that found by Lusins (9 of 15 vs. 2 of 6); and increased activity at articular facet joints adjacent to the fusion mass, suggesting a cause for pain, was observed less frequently (2 of 15 vs. 4 of 6). These differences may be entirely related to the small sample size or variation in localization terminology by the authors. Whatever the reason, the paper by Lusins et al. does add a further dimension to the often frustrating problem encountered in these patients by depicting the involvement of the articular facets at levels adjacent to the fusion mass as further potential sources of pain.

I believe that both articles offer evidence for the usefulness of SPECT bone scanning in the screening of patients who continue to have pain after spinal surgery.

References

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2. Slizofski WJ, Collier BD, Flatley TJ, Carrera GF, Hellman RS, Isitman AT. Painful pseudarthrosis following lumbar spinal fusion: detection by combined SPECT and planar bone scintigraphy. *Skel Radiol* 1987; 16:136–141.

Walter J. Slizofski
Hahnemann University Hospital
Philadelphia, Pennsylvania

REPLY: Our paper deals with a spectrum of patients who have had spinal surgery for pain and included in the series were those patients who had only one level laminectomy and progressed to include patients who had multilevel laminectomy, as well as multilevel laminectomy and fusion. In our series the number of individuals who had fusion were only six, and obviously this is too small a number to derive any statistical significance as to the type of abnormality these patients had experienced. Slizofski et al. deals with a total of 15. I do believe that the value of both papers is not in comparing rather low level statistics, but rather in the fact that both papers point out the significant advantage of using single photon emission computed tomography scanning to gain insight into the failed back syndrome. Based on these early observations, larger series can be developed and are being developed by us, and hopefully others, regarding the specific types of failures that occur with lumbar surgery, including those of pseudoarthrosis and fusion.

John Lusins
Upper Main Street Neurodiagnostic Associates
Oneonta, New York

Correction: Beta Dose Point Kernels for Radionuclides of Potential Use in Radioimmunotherapy

In the article by William V. Prestwich, Josane Nunes, and Cheuk S. Kwok "Beta Dose Point Kernels for Radionuclides of Potential Use in Radioimmunotherapy," (*J Nucl Med* 1989;30:1036–1046) an error was made in calculating the data of Table 2. The corrected data is presented here. All other results presented remain as in the original manuscript. The authors would like to thank Douglas J. Simpkin of St. Luke's Medical Centre, Milwaukee for bringing this to their attention.

TABLE 2
Scaled Beta Dose Point Kernels

Scaled distance	^{32}P	^{67}Cu	^{90}Y	^{131}I	^{186}Re	^{188}Re
0.00	0.2632E+01	0.9349E+01	0.2575E+01	0.1006E+02	0.4873E+01	0.3064E+01
0.04	0.2651E+01	0.5343E+01	0.2447E+01	0.5550E+01	0.3603E+01	0.2822E+01
0.08	0.2627E+01	0.4131E+01	0.2395E+01	0.4246E+01	0.3209E+01	0.2732E+01
0.12	0.2537E+01	0.3202E+01	0.2308E+01	0.3278E+01	0.2854E+01	0.2593E+01
0.16	0.2401E+01	0.2453E+01	0.2198E+01	0.2509E+01	0.2510E+01	0.2421E+01
0.20	0.2229E+01	0.1842E+01	0.2071E+01	0.1878E+01	0.2178E+01	0.2222E+01
0.24	0.2031E+01	0.1352E+01	0.1928E+01	0.1359E+01	0.1859E+01	0.2004E+01
0.28	0.1815E+01	0.9702E+00	0.1772E+01	0.9416E+00	0.1559E+01	0.1772E+01

(continued)