the imaging of medullary thyroid carcinoma. *J Nucl Med* 1986; 27:1150–1153.

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REPLY: We appreciate the opportunity to reply to the letter of Hilditch, Murray, McLellan et al. in which they report continuing limited success with [^{99m} Tc](V)DMSA for imaging patients with medullary carcinoma of the thyroid (MCT).

We would disagree somewhat with the authors' interpretation of their own data, as three of four patients reported demonstrate uptake of [^{99m} Tc](V)DMSA, namely Patients 1, 2, and 3. Uptake in Patient 3 is much less than seen in Patients 1 and 2 but the authors do not comment on the volume of tumor resected from this patient. We would agree that Patient 4 gave a false-negative result.

In our article (*1*) we, in fact, report uptake in seven out of eight patients imaged and not all patients as Hilditch et al. suggest. We would entirely support the statement that "the outcome of imaging is dependent on the state of disease" as microscopic foci of tumor would be unlikely to take up enough tracer to be successfully imaged. However, our experience now indicates that positive results can be obtained in patients with small volumed disease, although more false negatives are obtained in this subgroup.

In light of our further experience with [^{99m} Tc](V)DMSA we continue to believe that this agent can play a significant role in the management of patients with MCT, particularly in patients with local recurrence when successful repeat surgery can significantly prolong the disease free interval.

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Reproducibility of Hepatic Perfusion Index

TO THE EDITOR: We have read with interest the article by O'Connor et al. (*1*) on dynamic hepatic scintigraphy. We take issue with the comment "Parker et al. administered a 25 mCi bolus of ^{99m}Tc but failed to obtain good reproducibility in a study of eight patients" on the following grounds.

1. The author's name is Parkin.
2. We used sulfur colloid labeled with technetium.
3. We administered 3 mCi per patient not 25 mCi.
4. We carried out repeat studies on 12 not eight normal subjects and found a mean difference between paired observations of 17%.
5. On reanalyzing the data from 20 studies drawn at random using a second observer we found the degree of correlation between the two results was 0.94 and in no case was the change sufficient to alter the diagnostic result.

In retrospect we should, perhaps, have included some patients with abnormal Hepatic Perfusion Index in the group who had repeat scans but this, we feel sure, would have further improved the reproducibility since the major source of error is the poor statistics in the arterial component of the liver time activity curve. In patients with hepatic metastases, the statistics of the arterial phase are improved.

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REPLY: We thank Parkin and Robinson for their comments and would like to apologize for the typographical error in the spelling of Parkin and for incorrectly stating the administered dose used in their study (*1*). However, these facts do not change the substance of our statement that their study failed to show good reproducibility.

Parkin et al. stated that reanalysis of 20 studies showed little interobserver variation. Although it is not stated in their study, the upper limit of normal for the hepatic perfusion index (HPI) would appear to be 0.4. A cursory glance at their data shows that at least one subject had a change from 0.45 to 0.27 on reanalysis. Furthermore, in the normal subjects who underwent repeat studies, several subjects showed a large difference in the HPI which was sufficient to alter the diagnostic result from normal to positive or borderline positive. Their value of 17% for the root mean square difference between paired observations should be compared with a value of 4.4% obtained with Method 3 in our study (*2*). We would also refer readers to the detailed analysis of the method of Parkin et al. published by Tindale and Barber (*3*). They found that the HPI was dependent, among other things, on the extent of bolus smearing and the level of tracer extraction and concluded that this technique should be used with caution when interpreting abnormal values.

Despite this poor reproducibility of the slope based methods, Parkin and his co-workers (*1,4*) have clearly demonstrated that measurement of the relative contribution of hepatic artery to total hepatic blood flow may be a valuable technique in the detection of liver metastases.

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Comparison of Bone Density Measurements from Different Skeletal Sites

TO THE EDITORS: The interesting study of Seldin et al. reported in the February 1988 issue (*U Nucl Med* 1988; 29: 168-173). mainly confirms the findings of our own investigations we were able to carry out in cooperation with the University of Wisconsin, Madison (*1*). However, we would like to indicate some special points. Obviously the bone density values of the scattergram cover the whole range from extremely low to high densities and it is not said which group of patients represent which values. Only if all groups would have the same or at least similar regressions, the pooling of all values might be considered statistically meaningful. Furthermore most of the suspected "osteoporotic" patients, obviously the majority of the displayed values, seem to turn out as normal age matched values. It is not clear what criteria had been used to establish the diagnosis of osteoporosis independently from the bone density measurements. It is doubtful also, if statistical analysis of male and female subjects together in the same regression analysis is allowed. The authors probably were just trying to obtain a large span of values.

We have done similar comparisons of SPA and DPA with a particular focus on prediction of spinal bone density, obtained with DPA from measurements of purely trabecular bone at the distal radius obtained with our specially built iodine-125 (^{125}I) QCT scanner (*2*). 146 individuals (patients and normals) were studied. We decided to indicate that pooling of our groups in order to extend the range of values might statistically not be meaningful, because different regressions between the groups were observed! The pooled correlations of course turned out significantly better.

In comparison Seldin's results our pooled results show correlations between peripheral sites and axial density not below 0.61, with the best correlation between radius trabecular bone density and spinal BMD (r = 0.72, s.e.e. was 10.7%). Although prediction ability of axial densities from peripheral measurements in our data was significantly better (s.e.e. 10–14%) than in Seldin's published data, our s.e.e.s still were too large to predict spinal BMD obtained with DPA.

It has to be considered that comparison of different sites with different ratios of cortical to trabecular bone in general and in different diseases in particular would result in an unapplicable regression analysis. In vitro measurements indicate a much better correlation between purely trabecular bone of lumbar vertebra and the distal radius (r = 0.7–0.9) (*3,4*). Our data suggest the same conclusion, assuming that lumbar vertebra represent the highest amount of trabecular bone when measured with DPA-equipment. Seldin et al indicate that the overall mineral content may be only one of several factors associated with fracture risk. Bone compound structure certainly is another. Therefore and for the reason of proportional bone turnover it is better to compare similar bone structures. It has to be emphasized not to generalize method-dependable findings like Seldin's and those of others (*5,6,7*) obtained with planar absorptiometric methods (SPA and DPA) which do not allow the selective analyzation of bone structures. As long as there is a lack of data on evaluation of more comparable equal bone structures (purely trabecular bone or cortical bone) at different sites with adequate methods—such as conventional QCT and special [^{125}I]QCT—conclusions on data should expressively be limited to the methods they are obtained with. There is no gold standard existing yet.

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REPLY: We thank Drs. Schneider and Börner for their interest in our work. Their soon-to-be-published investigation supports our conclusions that SPA and DPA measurements