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REPLY

We are very happy that our paper stimulated the interesting discussion by Dr. Turner. Our third study of liver scanning at Johns Hopkins differed from the previous two, referred to by Dr. Turner, in that the basic populations under study differed. In the studies by Poulose et al and by Fee et al, the basic populations were patients coming to abdominal surgery because of suspected abdominal malignancy; thus a relatively great number of patients had liver metastases at the time of surgery.

The present study population consisted of patients who had needle biopsy of the liver for various indications. The criterion for including a patient in this study was that the patient should have had a liver scan and a liver biopsy within 1 week. We can postulate some possible explanations for the low number of normal biopsies: either the clinicians have a very good index of suspicion in selecting patients for this procedure or the pathologists have a low threshold in diagnosing liver disease from needle biopsy preparations. We agree that the problem of liver imaging must take into consideration both the false positives and false negatives.

The high rate of false negatives in the study by Poulose et al using the rectilinear scanner was explained by the fact that numerous small metastases, as well as larger ones, on the liver surface could not be detected. It was hoped that the scintillation camera with its higher resolution and parallel-hole collimator and multiple views would decrease the number. Unfortunately, it did not. Despite the use of multiple views, we did not get rid of the problem of missing 20% of the lesions within the liver.

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CHEMICAL STATE OF TECHNETIUM IN VIVO (LETTER NO. 1)

In a recent article (*1*), Hambright et al state that Tc(IV) should be inert, that is, kinetically slow to substitution reactions. They invoke an inert Tc(IV) gluconate on theoretical grounds and on the basis of a competitive binding experiment between ^{99m}Tc-HEDP and 0.1 *M* gluconate. However, in a previous article (Ref. *2* of their paper) they point out that ^{99m}Tc-gluconate is dissociated by the competitive binding of ^{99m}Tc by Sephadex during a quick chromatographic analysis. The discussion of the mechanism of the inorganic reaction thus seems to contradict their previous statements about chromatographic artifacts.

They further state that the conclusion of earlier work on ^{99m}Tc chelates, including several papers by myself and others, is that "the chemistry at macro ^{99}Tc levels may be different than that shown by micro ^{99m}Tc." In fact, we had tried to show that ^{99}Tc and ^{99m}Tc reduced with stannous ion, for example, show similar reactivities with DTPA and, therefore, that the same reduced state present in ^{99}Tc-DTPA is probably present in the ^{99m}Tc radiopharmaceutical. This is similar to the authors' conclusion that ^{99}Tc and ^{99m}Tc reduced with stannous ion show similar biologic behaviors when chelated with HEDP and, therefore, that the Tc(IV) state can be assigned to the carrier-free ^{99m}Tc radiopharmaceutical. Both experiments are based on the assumption that the only variable is the concentration of technetium. However, the concentration of other species, such as stannous and stannic ion, which can affect the oxidation potential may not be held constant. Also, it has not been proved that only one oxidation state of technetium can bind to the chelating agent or give the