LETTERS TO THE EDITOR

Concerning the Usual Dose of I-131 to Ablate Thyroid

The letter of Drs. Caplan and Dvorak in the *Journal* (*1*) draws attention to the failure of "usual doses" of I-131 to ablate thyroid remnants, and relates this correctly to the level of the thyroid uptake in the general population.

It is similarly becoming obvious that larger doses are necessary for the treatment of thyrotoxicosis. We, and I believe others, have in the more recent past paid little attention to the 24-hr uptake of I-131 in the treatment of patients with thyrotoxicosis. Instead, we have tended to use standard doses related to clinical appraisal of the size of the thyroid. Clearly, this is no longer tenable in view of the low level of uptakes that now may be associated with thyrotoxicosis in either Graves' disease or toxic nodular goiter.

It is possible, indeed, that we shall see the emergence of patients with thyrotoxicosis whose iodine levels make it impossible for them to receive treatment with I-131. Currently we are attempting to assess the importance of this in our own material by estimating percentage uptake on treatment doses. I would be interested to hear whether this is indeed a problem in the experience of other users of I-131, and what expedient, if any, can be made available to circumvent this problem. The simple answer, "increase the administered dose," is not necessarily at all simple since we may soon reach or exceed daily disposal limits.

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REFERENCE

1. CAPLAN RH, DVORAK D: The usual dose of I-131 used to ablate thyroid. *J Nucl Med* 18: 946, 1977

Myocardial Imaging with Thallium-201

The paper by Narahara et al. (*1*), in which the background contribution in thallium myocardial imaging is estimated, has a few shortcomings. Although the authors concede that background is a function of anatomical relationship, they still assume that dog data can be used to predict the human situation.

The main objection to their paper, however, is procedural. They did not replace the blood and extracellular fluid after removing the heart. It is an interesting way of approaching the problem, since it assumes that all activity in the real volume occupied by the *heart* is in fact *myocardial* and related to blood flow.

A small additional comment. In the end they wanted to demonstrate that the result of a particular image-processing method is a distortion of the real data. Yet all their illustrations show heavy contrast enhancement, so much so that the 25% "error" in the interpolative method cannot be seen (Fig. 6). If the end result is the image, they need to show the difference in contrast, not in count profiles.

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REFERENCE

1. NARAHARA KA, HAMILTON GW, WILLIAMS DL, et al: Myocardial imaging with thallium-201: An experimental model for analysis of the true myocardial and background image components. *J Nucl Med* 18: 781–786, 1977

Reply

We appreciate Dr. Goris' comments. We agree that the dog model may not be completely applicable to humans, but for the reasons discussed in the paper, it is probably a reasonable substitute.

Dr. Goris is correct, that we in fact removed the whole heart containing myocardium, blood, and epicardial fat. This was not emphasized because the activity in the blood and fat was measured and found to be very small. On a weight basis, blood contained 2.5% and fat 20% of the activity found in myocardium (*1*). Given a cavitary volume roughly equal to myocardial mass, and fat equal to 5% of myocardial mass, the contribution of nonmyocardial activity is about 3%. We feel this is insignificant. Furthermore, the direction of this error tends to minimize the difference between "true" and interpolative background subtraction.

Except for the profile images, the images do not, in my opinion, show heavy contrast enhancement. Figure 6 does have very high contrast. With true background subtraction, there is no remaining background. With interpolative background subtraction, all background and some of the myocardial activity is removed. With no background activity for comparison, the profile analysis seems appropriate.

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REFERENCE

1. HAMILTON GW, NARAHARA KA, YEE H, et al: Myocardial imaging with thallium-201: Effect of cardiac drugs on myocardial images and absolute tissue distribution. *J Nucl Med* 19: in press

In Vivo Labeling of Red Blood Cells with Pertechnetate in Hyperthyroidism

There are two kinds of Tc-99m labeling procedures for red blood cells: in vitro labeling and the recently developed in vivo technique. In vitro labeling procedures are cumbersome, tedious, and time-consuming. By contrast, in vivo labeling of RBCs with pertechnetate, as reported by Pavel et al (*1*), is a simple procedure without any manipulation of the RBCs. These workers report high labeling efficiency for their method (over 95%) and that high quality blood pool images are obtained. We have confirmed this in our followup studies. They did not mention, however, whether or not Tc-99m appeared in the thyroid, choroid plexus, salivary glands, or stomach when pertechnetate was administered 30 min after injection of stable pyrophosphate.

While we were imaging the cardiac and cerebral blood pools in two hyperthyroid women, using a large-field-of-view camera and with RBCs labeled in vivo according to their method, we observed early, intense trapping of Tc-99m in the thyroids of both patients after several transits through the glands (Fig. 1). The choroid plexus and salivary glands were also faintly visible. The labeling efficiency determined