LETTERS TO THE EDITOR

Phantom Kidney in Tc-99m DTPA Studies of Renal Blood Flow

I read with great interest the paper by Dr. Holmes, et al., published in the July issue of the *Journal* (*1*). It seems to me that the authors' explanation of the observed phenomenon, although plausible, may not be the only possibility. The location, size, shape, and flow pattern of the "phantom kidney" were very similar to those of the spleen, and this possibility should have been excluded by a technetium-99m sulphur colloid study.

ZVI H. OSTER
The Chaim Seba Medical Center
Tel-Hashomer, Israel

REFERENCE

1. HOLMES ER, KLINGENSMITH WC, KIRCHNER PT, et al: Phantom kidney in Tv-99m DTPA studies of renal blood flow: Case Report. *J Nucl Med* 18: 702–705, 1977

Reply

The authors appreciate your interest in our article. You are correct that activity in the region of the kidneys on a technetium DTPA flow study could be caused by spleen perfusion. In our report this is mentioned and referenced.

EDWIN R. HOLMES III
Memorial Hospital
Colorado Springs, Colorado

In Vivo Labeling of Red Cells with [^{99m}Tc] Pertechnetate

A recent paper by Hamilton et al. (*1*) presents experimental work on rats aiming to determine, among other things, the optimal stannous-ion concentrations and best interval between the two injections. This very well-designed work has indeed answered these practical questions from the experimental point of view, and has confirmed the adequacy of the clinical protocol previously published and described (*2,3*). Unfortunately it is difficult to understand why the authors concluded that their results are at great variance with ours. They overlook the fact that our data were expressed in mg Sn-PPi, whereas theirs were in Sn(II). A simple transformation shows that our 200 μg Sn-PPi/Kg is equivalent to 30 μg Sn(II)/Kg, and thus only three times the dose specified in their article. Interestingly enough, our present clinical dose contains the equivalent of 15 μg Sn(II)/Kg (*3*), which is close to their experimental value.

As far as the time interval between injections is concerned, the authors agree that a 5-min interval may be adequate in a rat but not in a patient because of mixing considerations. The longer time interval needed brings them closer to our clinical protocol, which suggested a 30-min interval. In practice a 20–40 min interval is perfectly acceptable.

Such close agreement between experimental and clinical data is worth pointing out and in itself fully justifies the work of Hamilton et al., without any need to search for dissenting conclusions. The rest of their conclusions concerning labeling efficiency (% ID) and effect of delayed injection intervals also confirm our most recent clinical data (*3*).

We believe that the nuclear medicine community should be fully aware of the remarkably close correlation between clinical and experimental data, coming from two different institutions and concerning a labeling method that is not only fundamentally different but also so much in demand today, due to the rapid development of radiotracer cardiology.

DAN G. PAVEL
A. MICHAEL ZIMMER
University of Illinois Medical Center
Chicago, Illinois

REFERENCES

1. HAMILTON RG, ALDERSON PO: A comparative evaluation of techniques for rapid and efficient in vivo labeling of red cells with [^{99m}Tc] pertechnetate. *J Nucl Med* 18: 1010–1013, 1977

2. PAVEL DG, ZIMMER AM, PATTERSON VN: In vivo labeling of red blood cells with ^{99m}Tc: A new approach to blood pool visualization. *J Nucl Med* 18: 305–308, 1977

3. ZIMMER AM, PAVEL DG: Technical parameters involved in the in vivo red blood cell labeling technique. *J Nucl Med* 18: 637, 1977 (Abst)

Reply

We appreciate the comments of Pavel et al. concerning the close correlation between the clinical data published in their paper (*2*) and abstract (*3*) and our experimental results with Tc-99m-labeled red cells.

We are glad to learn that their current clinical stannous-ion dose is similar to the stannous-ion concentration recommended by our experimental studies. Their comments about the stannous-ion concentration raise a point that should be emphasized. We took great care in our work to express the stannous-ion dose as μg Sn(II)/Kg and not as μg Sn-PPi/Kg. This was done because we were concerned principally with the stannous-ion concentration administered and not the amount of pyrophosphate. We emphasize this because several investigators have expressed an interest in using noncommercial stannous pyroprosphate preparations that would contain a different ratio of stannous ion to pyrophosphate. In our experimental studies, we obtained successful red-cell labeling with commercially available stannous diphosphonate and stannous pyrophosphate preparations having ratios of Sn(II) to PPi or DiP different from the agent used by Pavel et al. Based on these experimental results, we suggest that future discussions concerning the amounts of stannous ion injected during in vivo red-cell labeling be expressed in terms of μg Sn(II)/Kg.

ROBERT G. HAMILTON
PHILIP O. ALDERSON
The Johns Hopkins Medical Institutions
Baltimore, Maryland