First New Appraisal of The Journal of Nuclear Medicine's Primacy Claim Policy

TO THE EDITOR: I read your comments in the April 1993 Randoms column and found them very sensible, however understated. Obviously the following publications and their outrageous presumptions should have been rejected out of hand:

> Joliot F, Curie I: Artificial production of a new kind of radio-element. *Nature* 10 Feb 1934;201.
>
> "These experiments give the first chemical proof of artificial transmutation, and also the proof of the capture of the alpha particle in these reactions."
>
> Frisch OR, Meitner L: Disintegration of uranium by neutrons: a new type of nuclear reaction. *Nature* 11 Feb 1939; 3615:239.
>
> "On the basis, however, of present ideas about the behaviour of heavy nuclei, an entirely different and essentially classical picture of these new disintegration processes suggests itself."

If only your "To the Best of Our Knowledge" had been published back then, the King could have avoided the embarrassment of awarding these egotistical advertisements Nobel prizes . . . and the spectre of nuclear war and of nuclear medicine itself might have been averted! Oops, do I mean that, do I mean that?

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REPLY: I would like to thank Dr. Hattner for identifying previous instances of hubris in the literature. The King did not honor these investigators for their claims of "me first." The quality of science reported in these articles would not be diminished by changing the titles to read:

> Joliot F, Curie I: Artificial production of a radioelement.
>
> Frisch OR, Meitner L: Disintegration of uranium by neutrons: a type of nuclear reaction.

We do not intend to reject articles on the basis of hubris. However, we will allow history to be the judge of that which is worthy—not author opinion.

In the case of our Nobel Laureates, recognition came in spite of the titles of their publications. A jury of their peers, who understood the value of their contributions, recommended these deserving investigators for the prize.

A brief search of Medline suggests that about 10% of entries use the word 'new' in a title or abstract. Science may be making progress but we are not moving that far that fast. Like cold fusion, more manuscripts claiming to be "first" or "best," have been relegated to dusty archives of scientific oblivion than have been recognized as pioneering and worthy.

Today the combination of global communications and the intense desire to be recognized make it difficult to differentiate between graffiti and art. In the era of the CNN "factoid," real contributions must be recognized as such by a peer group. Then, and only then, are they truly worthy of being called original.

H. William Strauss
Editor

Noninvasive Real-time Monitoring of Renal Function

TO THE EDITOR: Rabito et al. (*1*) claim to have described and validated "a new approach for the evaluation of GFR every few minutes under nearly real-time conditions." That this assertion is more than flirting with the truth is evident in that we have described and used in clinical practice a similar technique for many years. Initially we utilized sodium iodide (*2*) and later miniaturized cadmium telluride (*3,4*) detectors to monitor renal function in transplant patients by providing continuous measurement of clearance of ^{99m}Tc-diethylenetriamine-pentaacetic acid (DTPA) from the body.

The usual mitigatory excuse offered by our North American colleagues, that literature search of obscure European journals are too tedious, does not hold in this instance in that our first paper (*2*) actually was published in your own eminent journal.

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REFERENCES

1. Rabito CA, Moore RH, Bougas C, Dragotakes SC. Noninvasive, real-time monitoring of renal function: the ambulatory renal monitor. *J Nucl Med* 1993;34:199–207.
2. Sampson WFD, Macleod MA, Warren D. External monitoring of kidney transplant function using Tc-99m(Sn)DTPA. *J Nucl Med* 1981;22:411–416.
3. Macleod MA, Sampson WFD. An evaluation of a portable cadmium telluride detector and data storage system as a continuous monitor of renal transplant function. In: Joekes AM, Constable AR, Brown NJG, Tauxe WN [eds.] *Radionuclides in Nephrology*. London: Academic Press; 1982;341–346.
4. Smith RS, Sampson WFD, Warren DJ. Evaluation of miniaturised cadmium telluride detectors in renal transplant renography. *Nuc Med Comm* 1981;2: 121–125.

REPLY: The comments of Drs. Sampson and Macleod concerning our article on noninvasive, real-time monitoring of renal function are based on incorrect interpretation and on an essential misunderstanding of the concept of real-time monitoring.

There is a fundamental difference between our paper and previous publications, including those from Sampson and MacLeod regarding use of external counting to measure renal function, i.e., the development and implementation of the concept of real-time monitoring of renal function. The term "new approach" in our paper refers not to use of external counting devices to measure renal function, as the comment of Sampson and Macleod would imply, but to design of the instrument and analysis of data to produced those measurements under near real-time conditions.

As indicated in the introduction to our article, external counting devices have been used for many years to measure renal function. Proper credit to these contributions were given in references 10–15. We also indicated in the same paragraph that all these techniques were intended to measure function in patients with relatively stable renal function and the speed of response was never considered in design of the instruments. We also indicated in Discussion that "External counting has been used before to estimate renal function; however, the large variability of data demanded a prolonged counting period in order to obtain an accurate measurement of GFR (*10–15*)". The papers of Sampson and Macleod are not an exception to this problem. With activity measurements performed for 10 sec every 30–60 min, the minimal time required with their approach to obtain measurement of the rate of clearance is at least 3–4 hr (Although a minimum of 3 points are required to define a slope, accuracy of measurement increases with the increase in the number of points used). This time is not significantly different from the minimal time required to obtain a similar measurement with practically any of the standard clearance techniques that require urine and plasma collection. In other words, the concept of real-time monitoring of renal function is nowhere to be found in the papers of Drs. Sampson and Macleod.

The difference between both approaches becomes more evident in our second paper (*1*). The rapid changes in renal function that sometimes occur after infusion of contrast material, as shown in this paper, may have gone undetected with the Sampson and Macleod method.

We see some irony in their comments because our decision not to include their papers in our reference list was based not on the "tedious [nature] of literature searches of obscure European Journals" as they alleged, but on two facts: one, there is a limited number of references that can be included in a paper; two, we could not see much difference between their work and the studies described in references 10–15, which included contributions published 10 years prior to their first publication.

REFERENCE

1. Rabito CA, Fang LST, Waltman AC. Renal function in patients at risk of contrast material-induced acute renal failure: noninvasive, real-time monitoring. *Radiology* 1993;186:851–854.

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Nuclear Medicine: More Than Just Medical Imaging

TO THE EDITOR: I was thankful for Dr. Wagner's reply (*1*) to Doug Maynard in the January issue of the *Journal* (*2*). I agree with everything he wrote and, I believe, even what he did not write but manifestly uses as premises. I would like to add two considerations.

I find it interesting that Maynard entitles his essay *Medical Imaging in the Nineties: New Directions for Nuclear Medicine*. This is begging the question. The first question is whether nuclear medicine should be described as "medical imaging." Diagnostic radiologists remind me strongly of the past and present nationalist imperialists in Europe: they start by claiming some territory as theirs, take it, and failing to transform them forever hate those living in the territories as being too foreign. Nuclear medicine is reduced to nuclear imaging and then found wanting in comparison with other modalities.

Organ-centered affinity groups are appealing to some extent. Neurosurgeons and neuroradiologists did historically develop an effective collaboration, but again the question is biased. Diagnostic radiology is subdivided partially by organ but mostly by body region. Imaging techniques used in radiology yield structural information on everything in the region regardless of the question. In contradistinction, nuclear medicine is specific: the procedure is defined by the question asked (what Maynard would call organ-specific, but is in fact physiopathology-specific) and information obtained from the procedure is imbedded in the procedure. This has one advantage. Our procedures are naturally defined by a specific question and a specific answer. Efficacy is easily defined in contrast to procedures performed "to have a look." To the extent that we need system or disease-affinity groups, the focus for them should be in the clinical field, not in radiology.

Dr. Maynard, who within the Society of Nuclear Medicine is as established as one can be, did us a service by pointing out to those of us who love nuclear medicine that our society is very much biased toward those who only like nuclear imaging.

REFERENCES

1. Wagner HN Jr. The new molecular medicine. *J Nucl Med* 1993;33:165–166.
2. Maynard CD. Medical imaging in the nineties: new directions for nuclear medicine. *J Nucl Med* 1993;33:157–164.

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Labeling of Antibodies with Technetium-99m

TO THE EDITOR: A recent article in the *Journal* (*1*) compared the ^{99m}Tc-labeling of murine monoclonal antibodies (Mabs) labeled by the Rhodes procedure (*2*) to another method using the hydrazino-nicotinamide chelate (*3*). The authors observed a faster clearance of the directly labeled Mab-IgG, with 30% of radioactivity appearing in urine as early as 6 hr postinjection. In contrast, only 10% of radioactivity was present in urine at the same time after injection of the chelate-labeled antibody. The authors concluded that "important differences in vitro and in vivo in mice have been observed for ^{99m}Tc labeled to two IgG antibodies by one direct and one indirect labeling method" (*1*).

Our group has some experience in direct labeling of antibodies with isotopes of technetium and rhenium (*4–6*), and we believe that further comments and observations might be in order. In our view, the "pretinning" method for direct labeling is inferior to the thiol-reduction direct method used by us and other groups (*7–9*). Deficiencies in the pretinning approach are evident in the immunoreactivity losses seen during direct labeling (*1*).

The in vitro cysteine/glutathione challenges may also support this view. With such extensive isotope removal from directly labeled antibodies, it is difficult to see how the pretinning direct labeling method could be useful for Mab imaging agents. In comparison we prepared a ^{99m}Tc-direct-labeled conjugate by our thiol-