Loss of ^{123}I-MIBG Uptake by the Heart in Parkinson's Disease: Assessment of Cardiac Sympathetic Denervation and Diagnostic Value

TO THE EDITOR: I read with interest the article by Satoh et al. (*1*), which was published in the March 1999 issue of the *JNM*. Unfortunately, the content of this article overlaps substantially with another article (titled differently) published in 1997 in the journal *Nippon Rinsho* (*2*). Using the same data, the same results, and the same figures as those included in the article that appeared in *Nippon Rinsho*, in the 1999 *JNM* article, the authors reached nearly equivalent conclusions. Is this sort of secondary publication acceptable (*3*)?

Second, authorship credit should reflect substantial contributions to the study (*3*). Although the number of authors of the Japanese version (*2*) is 3, the English version (*1*) appears to have been coauthored by 6 persons. I would like to know on what basis these additional coauthors were given credit for the English version.

Third, in Figure 2 of both articles, the name of the vertical axis on the bar chart of the wash-out rate differed between the Japanese and English versions. In the Japanese version, it was "WR" (wash-out rate), whereas in English version, it was "H/M". Which is correct?

Fourth, early images were obtained in 15 min after injection of MIBG in the English version, though in the Japanese version, the time frame was 30 min. Which is correct?

Although the third and forth questions are minor problems, these also focus on the quality and reliability of this study.

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REPLY: The *Journal of Nuclear Medicine* (*JNM*) thanks Dr. Ohmura for his keen eye in calling to our attention the similarity of 2 articles published by Satoh et al. (*1,2*).

As stated in the Information for Authors printed quarterly in the journal (and available on line at http://www.snm.org/about/jnm_authors.html and through fax on demand at 888-398-7662 or 703-336-5573, document number 501), the *JNM* follows the *Uniform Requirements for Manuscripts Submitted to Biomedical Journals* (*3*). It is not the policy of the *JNM* to reprint previously published articles. In the *JNM*'s transfer of copyright agreement, the text of which is also included in the Information for Authors, the authors attest to the fact that "the material submitted to *The Journal of Nuclear Medicine* is **new**, original, and has not been submitted to another publication for concurrent consideration." To further clarify the *JNM*'s position on secondary publication, we have updated the Information for Authors to include the additional phrase: "... likewise, this manuscript has not been published elsewhere either in part or in its entirety." A printed copy of the revised statement can be found in the June 2000 issue of the *JNM* (*4*).

The first article was published in *Nippon Rinsho* in January 1997 (*1*). The final version of the *JNM* article (*2*) was submitted December 8, 1997, and published in March 1999. Clearly, the authors should have notified the *JNM* Editor in Chief of the publication of the first article. Not doing so meant that the copyright transfer agreement signed by all authors on November 26, 1997, was not accurate, in that the work had been previously published.

The substantial overlap in the scientific matter presented in the articles noted by Dr. Ohmura warrants careful attention. Because the first article was published in Japanese, it was only possible to compare the abstracts of both articles on MEDLINE. Although, the *JNM* article included 24 control subjects in addition to the original 35 patients in the *Nippon Rinsho* article, Dr. Ohmura cites "nearly equivalent conclusions." Also, no mention is made in the *JNM* article that the results were previously published: a common practice when initially small studies are continued. Although there is no acknowledgment of the original article, the *JNM* article does appear to be a secondary publication rather than new and original research.

Regarding the acceptability of secondary publication, the *Uniform Requirements* (*3*) do outline procedures for acceptable secondary publication. They are as follows:

"Secondary publication in the same or another language, especially in other countries, is justifiable, and can be beneficial, provided ALL (emphasis added) of the following conditions are met.

1. The authors have received approval from the editors of both journals; the editor concerned with the secondary publication must have a photocopy, reprint, or manuscript of the primary version.
2. The priority of the primary publication is respected by a publication interval of at least one week (unless specifically negotiated otherwise by both editors).
3. The paper for secondary publication is intended for a different group of readers; an abbreviated version could be sufficient.
4. The secondary version faithfully reflects the data and interpretations of the primary version.
5. The footnote on the title page of the secondary version informs readers, peers, and documenting agencies that the paper has been published in whole or in part and states the primary reference. A suitable footnote might read: 'This article is based on a study first reported in the [title of journal, with full reference].'

Permission for such secondary publication should be free of charge."

None of these procedures were followed for the article by Satoh et al. (*2*). The most important points to be noted about secondary publication are the approval of both editors, a note as to the secondary publication nature of the article, and that permission is required. It is this last point that stands out. Authors **must** obtain permission to reprint from the copyright holder. In most instances this is the publisher, whether that is a commercial publishing company or a nonprofit organization. That this permission be

granted free of charge speaks to the commercial nature of some publications and reinforces that secondary publication is for the benefit of the scientific community, not the publisher. American and international copyright law must be followed.

The *JNM* Editor in Chief has already addressed the issue of authorship in the May editorial "Authorship: Rite, Right, or Write of Passage?" (*5*). As for the difference in the number of authors, only the lead author can authoritatively comment on that. However, the difference in the number of subjects in the study population probably necessitated the involvement of additional investigators.

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Deconvolution Analysis or Renal Outflow Efficiency?

TO THE EDITOR: In their article on renographic analysis, Fleming and Kemp (*1*) compared mean transit time (MTT), obtained by deconvolution, and renal outflow efficiency (ROE) and concluded that both are useful in quantifying transit but with their own limitations: MTT to its requirement of time invariance and ROE to its dependence to overall renal function. Although there is no doubt that these conclusions are valid, it might be interesting to compare the impact of these limitations in clinical practice.

As mentioned by the authors (*1*), 1 limitation of MTT is the assumption of time invariance. During a renographic study, this requirement is not entirely fulfilled because back pressure from bladder filling may change the renal emptying during the procedure; moreover renal emptying is not a continuous phenomenon but occurs by propagation of contraction waves. We agree however that these 2 factors will probably only slightly affect the deconvolution analysis. Unfortunately, the baseline renogram offers, in clinical situations such as hydronephrosis and suspicion of obstruction, only a limited contribution: a continuous ascending curve tells us only that there is an impairment of transit, and the quantification of this impairment constitutes only an intellectual exercise. In such a case, the logical step is to use a diuretic, which may help differentiate a simple renal stasis with good response to furosemide from a more complicated situation, in which the response is poor. If the furosemide is administered at the end of the renogram (the so-called F+20 test), the urinary flow is going to change abruptly in the minutes after the injection of the diuretic. As a consequence, the assumption of stationarity is violated and the deconvolution technique is not applicable anymore. The same is true when the diuretic is given at the moment of the tracer injection (F0 test) or at any time during the renographic acquisition, because the urinary flow is not identical at the beginning and end of the renogram. Only in case of early injection of furosemide (F-15 test) can one assume that a stable urinary flow will be attained at the time of the renographic acquisition. Even then—and this was emphasized by the authors as well—the value of maximal transit time should be shorter than the duration of the renographic acquisition. This is not true in many of the cases of possible obstruction, in which MTT underestimates the duration of renal transit.

Regarding ROE, the authors produced simulated curves that tend to demonstrate that, for same values of MTT, ROE may be different, depending on the level of overall renal function (*1*). The authors highlighted the fact that MTT strictly reflects the transit whereas ROE does not. However, the model they used is oversimplified: they assume that the kidney is a simple tube, therefore neglecting the existence of a wide spectrum of transit times and exaggerating the effect of renal clearance. In a recent study (*2*), we tested the influence of the renal clearance on ROE using several spectrums of transit times. Although there was obviously an influence of renal clearance on ROE, regardless of tracer type, this influence was minimal. In conclusion, it is not fair to bring to the same level the disadvantages of both methods. In the particular case of the dilated kidney with high suspicion of renal obstruction, MTT is of limited value, whereas ROE seems to be a promising parameter in evaluating the kidneys' true capacity for emptying.

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REPLY: We thank Piepsz and Ham for their interest in our recent article and note their essential agreement with our findings (*1*). Most of their comments are very reasonable and helpful. In particular, we agree that renal outflow efficiency (ROE) seems to be a natural parameter for quantifying a response to an intervention during a renographic study. However, we feel that their conclusions that mean transit time (MTT) is of no value and that the dependence of ROE on renal function can be ignored are not supported by the facts.

Piepsz and Ham correctly point out that quantitative values of MTT are only strictly valid using an F-15 protocol. However, in this situation, which is arguably the optimal way of carrying out renography, the MTT may be as good a parameter as ROE or possibly even better given its independence of renal function. In