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

our own study using an F+8 protocol, which is not ideal for MTT derivation, a monotonic relationship between MTT and ROE was found (*1*). This suggests that a similar diagnostic decision would have been made on most of the renograms, using either parameter. In addition, Piepsz and Ham claim that MTT is limited by the duration of the acquisition. It is true that in situations in which the maximum transit time is not reached, the parameter calculated will not actually be the MTT. However, it represents the area-to-plateau-height ratio of the retention function, which is still a useful empiric measure of transit through the kidney. Thus, we feel that MTT may still have a useful role to play in clinical assessment of response to a diuretic.

Piepsz and Ham also claim that the presence of a spectrum of transit times will reduce the influence of clearance on ROE compared with the assumption of a single transit time. They correctly point out that our theoretical derivation of a relationship between MTT and ROE was based on the simplifying approximation of there being a single transit time through the kidney. However, given the systematic effect of clearance on ROE over a range of transit times, it seems likely that the relationship between MTT and ROE would be similar in the situation in which there is a spectrum of transit times present. Therefore, we question the claim that the influence of clearance will be reduced in this case. The clinical importance of the effect of clearance is, of course, another issue.

We feel that in the absence of firm evidence to the contrary, our original conclusion that both MTT and ROE have merits and limitations that should be considered in interpretation of renography is still valid. However, 1 caution on the use of MTT is worthy of mention. A recent interdepartment audit carried out in the United Kingdom (*2*) has shown large variation in the values of MTT obtained at different centers for some of the renograms studied. This reflects the lack of standardization of deconvolution software available on different nuclear medicine imaging computer systems. Thus, at present we consider that deconvolution analysis should be used only for routine clinical analysis if careful validation and testing of the software has been carried out. Unfortunately, relatively few centers in the United Kingdom use the ROE parameter, and it was not possible to obtain a measure of its interdepartment variability.

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Attenuation Correction in Dosimetry

TO THE EDITOR: In their article, "Pharmacokinetics and Dosimetry of an α-Particle Emitter Labeled Antibody: ^{213}BiHuM195 (anti-CD33) in Patients with Leukemia," Sgouros et al. (*1*) used the following formula to correct for attenuation of geometric mean of counts obtained over a source organ (Eq. 6 in their article):

$$\text{AttCorr} = e^{[\mu(T_{WB}-T_{ORG})]/2} \times \frac{\mu \times T_{ORG}}{2 \times \sinh(\mu \times T_{ORG}/2)}. \quad \text{Eq. 1}$$

However, as stated in the article, attenuation consists of 2 elements: self-attenuation within the source organ and attenuation by the surrounding tissue. Self-attenuation can be calculated as the mean attenuation over the source volume and, therefore, is equal to:

$$\text{SelfAtt} = \frac{1 - e^{-\mu T_{ORG}}}{\mu \times T_{ORG}}. \quad \text{Eq. 2}$$

Combining this with the attenuation in the overlying tissues, the correct formula to correct for total attenuation is:

$$\text{AttCorr} = e^{[\mu(T_{WB}-T_{ORG})]/2} \times \frac{\mu \times T_{ORG}}{1 - e^{-\mu T_{ORG}}}. \quad \text{Eq. 3}$$

An alternative form of the same formula uses the total attenuation in the body thickness, including the source thickness, instead of the attenuation in the overlying tissues only. This form has to be used if total attenuation is determined by transmission:

$$\text{AttCorr} = e^{(\mu T_{WB})/2} \times \frac{\mu \times T_{ORG}}{2 \times \sinh(\mu \times T_{ORG}/2)}. \quad \text{Eq. 4}$$

As can be seen, Equation 1 underestimates total attenuation by a factor $e^{\mu T_{ORG}/2}$. Organ activities obtained using Equation 1 will be underestimated by the above factor. Even for an organ thickness of only 10 cm, the factor already amounts to more than 1.6 and, therefore, will have considerable impact on the dosimetric results.

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1. Sgouros G, Ballangrud ÅM, Jurcic JG, et al. Pharmacokinetics and dosimetry of an α-particle emitter labeled antibody: ^{213}Bi-HuM195 (anti-CD33) in patients with leukemia. *J Nucl Med.* 1999;40:1935–1946.

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Reply: De Geeter correctly points out that the attenuation equation used in reference 1 has introduced a factor of $\exp(-\mu T_{ORG}/2)$. This term was introduced to better account for photon scatter and is equivalent to an organ-specific reduction in the μ value that reduces the attenuation correction to compensate for scatter. We found this necessary because even after the background correction, unrealistically high values (>10 %ID) were obtained for individual organs.

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