Letters to the Editor

Direct Determination of the Attenuation Coefficient for Radionuclide Volume Measurements

TO THE EDITOR: We have read with interest the recent article by Keller et al. (*1*). This is one of the many studies which has attempted to calculate a better μ for use in attenuation correction of count-based left ventricular volume measurements. The authors have directly measured the attenuation from the right and left cardiac chambers. Unlike others who have failed to correct for scatter which is inherent with the broad beam nature of clinical nuclear medicine (*2–6*), these authors have included a scatter correction but, unfortunately, their approach demonstrates again what we believe to be a lack of appreciation for the fundamental problem of attenuation as it pertains to absolute ventricular volume measurements.

Based on the publication of their approach, we believe it is time to again re-emphasize, emphatically, that a universal attenuation coefficient, μ, should not be used for left ventricular volume measurements. We have directly measured left ventricular attenuation in over 40 patients and obtained a μ with a range of 0.087–0.132 cm^{-1} with a mean of 0.113 cm^{-1} using our buildup factor approach (*2*). The extreme variability in this number is substantiated by the authors' work. The data given in Figure 2 makes a prima facia case against the use of a single value for μ. Indeed, if the authors would have reported the mean ± 2 s.d. for μ, we believe that they would have reached the same conclusion. Since μ is a function of multiple parameters (*2*) it must be directly measured on an individual basis. It is not sufficient to determine a "better μ" which is obtained from lumped data using another regression equation relationship.

We believe the authors' results help to substantiate the validity of our proposed use of another method such as the buildup factor (*2–4*). We have argued that the conventional attenuation equation $A = A_0e^{-\mu d}$ (which is the authors' Eq. 1) is inadequate and should be modified to $A = B(\infty)\ A_0e^{-\mu d}$. $B(\infty)$, the buildup factor or scatter correction, is relatively constant for various source volumes simulating LV dimensions, ranging in value from 1.21–1.27 with a mean of 1.23 (*2*). In this recent study the authors have corrected for scatter by multiplying the attenuated activity A by 0.81 leading to a final equation $0.81\ A = A_0e^{-\mu d}$ or $A = (1/0.81)A_0e^{-\mu d}$. This is equivalent to $A = 1.23\ A_0e^{-\mu d}$, where 1.23 is equivalent to our calculated buildup factor. Incidentally, we have also reported that the attenuation equation for LV volumes using whole-frame counts should be $A = 1.15\ A_0e^{-0.12d}$ which is the same results that these authors have reported (*2*).

In summary, while the search for a "better μ" appears to continue, we hope that this letter again calls to attention the importance of using a direct measure of attenuation for each individual rather than using any single lumped value for μ.

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Jeffry A. Siegel
Alan H. Maurer
Temple University Hospital
Philadelphia, Pennsylvania

REPLY: We appreciate Dr. Seigel's and Dr. Maurer's interest in our paper "Direct Determination of the Attenuation Coefficient for Radionuclide Volume Measurements" (*1*). Drs. Siegel and Maurer have previously stated that they believe that the controversy over the proper choice of a universal attenuation coefficient, μ, in the calculation a transmission factor (TF) with $TF = e^{-\mu d}$ is superfluous (*2*). We are not yet persuaded, however, that such is the case. We acknowledge the considerable contributions of Drs. Siegel and Maurer and their associates for their work with ventricular volume determinations using values for μ, individually determined with an esophageal source (*3*), and in development of their buildup factor method (*2,4–5*). We suspect, however, that the vast majority of left ventricular volume determinations are now performed with a single left anterior oblique (LAO) view with correction for absorption with a regression equation or with application of a universal attenuation coefficient and measurement of depth of the ventricle in the thorax. Therefore, we believe that our main point, directed to those who correct with $TF = e^{-\mu d}$, is correct and still stands; namely, 0.12/cm is a more accurate universal μ than 0.15/cm. We acknowledge that our average value of 0.12/cm is just that, an average, and furthermore that the use of μ = 0.12/cm in calculating left ventricular volumes is not a panacea. The depth of the ventricle in the thorax, the shape of the ventricle, the size of the region of interest, and amount of background activity relative to ventricular activity have been identified recently as potential sources of error in studies with phantoms (*6*). New ways to compensate for these errors in each individual case deserve investigation in human subjects. It is our belief that a search for better left ventricular volume determinations from a single, LAO view will continue because of the overall quality of the image in this view and its ease of application. Individual calculation of μ with an esophageal source (*3*) is likely to be superior to use of a universal value for μ, but is not, in our view, a practical technique for routine clinical studies. Fur-

thermore, it is noteworthy that Drs. Siegel and Maurer and their co-workers found that the TF calculated individually by this means were often at considerable variance with the TF determined by their buildup factor technique. (*5*). We have some reservations in accepting the notion that use of the buildup factor technique should supplant techniques using only the LAO image. The buildup factor method requires obtaining an LAO image and an orthogonal right posterior oblique (RPO) image and calculations of net counts in both ventricular regions of interest. Isolation of the left ventricle from other structures, especially an enlarged left atrium, and accurate definition of the left ventricular region of interest in the RPO view is apt to be difficult in some patients, even with the help of a first-pass RPO image as applied by the Temple group. Background subtraction is required from both views in this technique; thus, errors from background subtraction might be further amplified. The extra time required for the two RPO images is a major practical disadvantage. The laborious calculation requiring computer assistance is also a practical disadvantage, albeit a minor one. We hope that active investigation and spirited discussions in this area of interest continue. Accurate determination of ventricular volumes by an easily applied noninvasive technique is so important that the search for the best possible technique should continue.

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Thomas C. Smitherman
Theodore R. Simon
Gregory J. Dehmer
Medical and Nuclear Medicine Services
VA Medical Center
Southwestern Medical School
The University of Texas Health Science Center at Dallas
Dallas, Texas

Cerebral Perfusion Imaging

TO THE EDITOR: Little is known about the mechanism of retention of the "chemical microspheres" for cerebral perfusion imaging with single photon emission computed tomography: iodine-123 iodoamphetamine ([^{123}I]IMP) , [^{123}I] N,N,N′-trimethyl-N′-(2-hydroxyl-3-methyl-5-iodobenzyl)-1, 3-propanediamine, thallium-201 diethyldithiocarbamate ([^{201}Tl]DDC), and technetium-99m (^{99m}Tc) HM-PAO. Hypotheses have included an amine receptor, pH-shift trapping, and a change in chemical form, either metabolic or spontaneous (*1*).

TABLE 1
Stability of [^{201}Tl]DDC and [^{99m}Tc]HM-PAO in Chloroform

Minutes after extraction	% Bound* [^{201}Tl]DDC† N = 6	% Bound* [^{99m}Tc]HM-PAO N = 5
0‡	94.6 ± 1.1	88.6 ± 2.6
2	31.8 ± 16.4	92.6 ± 5.7
5	12.9 ± 5.8	94.5 ± 2.8
10	10.8 ± 3.2	85.9 ± 8.5

Each value is mean ± s.d. for N determinations.
* Determined by chromatography using methods described in Refs (*2*) and (*4*), respectively.
† Results from Ref. (*3*).
‡ Bound in aqueous solution before extraction.

In their recent paper, van Royen et al. (*2*) speculate that "the [^{201}Tl]DDC complex falls quickly apart in vivo and distributes as the ^{201}Tl ion." We have obtained in vitro evidence which supports this suggestion (*3*): when [^{201}Tl]DDC is extracted into chloroform, it decomposes rapidly (half-time <2 min) and spontaneously into a polar species which behaves like the ^{201}Tl ion on chromatography (Table 1). [^{201}Tl]DDC is quite stable in aqueous solution but appears to fall apart rapidly in lipid medium.

It has also been suggested that technetium-99m d,l-hexamethylpropyleneamine oxine ([^{99m}Tc]HM-PAO) (*4*) is retained in the brain due to a rapid change in chemical form (*1*). However, we have shown that [^{99m}Tc]HM-PAO undergoes little decomposition when extracted into chloroform (unpublished results from this laboratory). In fact, the behavior of [^{99m}Tc]HM-PAO is the opposite of that of [^{201}Tl]DDC: [^{99m}Tc] HM-PAO decomposes fairly rapidly in aqueous solution but is quite stable in lipid medium. Therefore, it seems unlikely that the trapping of [^{99m}Tc]HM-PAO in the brain is due to rapid spontaneous decomposition, and thus may alternatively involve metabolic alteration or receptor binding.

References

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James R. Ballinger
Karen Y. Gulenchyn
Ottawa Civic Hospital
Ottawa, Canada