EDITORIAL

Of Theoretical Derivations and Empirical Evidence

The method proposed by Hansen and Siegel is based on the reasonable postulation that "the attenuation of thallium (photons) could be determined from the ratio of detected counts from the low and high energy (photons)."

In support of the postulation, they derive and then verify (by narrow beam attenuation measurement) that such a (linear) relationship exists. An empirical design is finally proposed in support of the final assumption, namely that the ratio of counts in the reconstructed image is related to the density underestimation by the same linear relation which was shown to relate them in a narrow beam geometry attenuation measurement.

It is the final assumption that requires verification on theoretical grounds, while the evidence in support requires reevaluation.

The (projection) data from which the tomographic images are reconstructed are the integral of the counts originating from activities along the projection rays, modified along the way by attenuation:

$$c = \int_0^d a_x \cdot e^{-\mu x} \cdot dx.$$

Chang (*1*) proposes that as a first approximation the pixel value in the reconstructed image can be seen as modified by the arithmetic mean of the absorption along the M rays defined by the projection data:

$$\frac{1}{M} \sum_{i=0}^{i=M} e^{-\mu x_i}.$$

Consider in contradistinction the relationship proposed by the authors between absorption and the ratio of counts, but rewritten in terms of the absorption coefficient and the distance:

$$\ln[e^{\mu_l \cdot d}] = 7.9 - 4.2 \ln \left[\frac{A_l \cdot e^{-\mu_l \cdot d}}{A_h \cdot e^{-\mu_h \cdot d}}\right].$$

The derivation provided in the Appendix does not apply to a situation where the (low counts-to-high counts) ratio is defined by a sum of exponentials, and therefore the case proven for narrow beam geometry remains unproven for reconstructed data. In effect, the authors substitute a summation term for a single term, without theoretical validation:

$$\ln \left[\frac{M}{\sum_{i=0}^{i=M} e^{-\mu_l \cdot d_i}}\right] = 7.9 - 4.2 \ln \left[\frac{A_l \sum_{i=0}^{i=M} e^{-\mu_l \cdot d_i}}{A_h \sum_{i=0}^{i-M} e^{-\mu_h \cdot d_i}}\right].$$

Whether an analogous functional relation could be derived from the first principle would have been moot in the presence of strong empirical evidence that there is one. One could use a homogeneous phantom, reconstruct for the high and low energy and see if the correction needed to obtain a homogeneous image with the low-energy data is predicted by the ratio of the densities in the low- versus high-energy image.

This "direct" evidence is not provided. Rather, the authors show how the correction factor derived from narrow beam geometry "improves" a cardiac phantom image. Depending on one's faith in the reliability and reproducibility of photographic displays, one would accept Figure 2 as evidence. The profiles in Figure 3 seem to be more reliable (with one reservation discussed in the next paragraph). As they are, however, they fall short of being convincing: The profile of the corrected image differs in detail from the "air' image profile, and it is difficult to gauge if, following global normalization, the difference between "air" and "uncorrected" would be any more significant, but the claim of improvement does not seem eccentric.

The reservation is again based on theoretical considerations: to avoid stochastic noise effects on the low-to-high ratio, the authors use a lower frequency filter for the reconstruction of the images used to produce the ratios. However, filtering influences the modulation transfer function. Such an influence on the modulation transfer function can be thought of as displacing counts. When different filters are used for reconstruction and for ratio definition, the ratio value computed in a location (x,y) cannot be said to apply to densities at (x,y). The effect would be most noticeable at the edges of the image object, if edges are defined as regions of high local contrast. If such an effect exists, it would not be revealed by the (maximum density) profiles shown in Figure 3. They are not obvious in Figure 2, but one cannot really tell.

In conclusion? A reasonable approach has been proposed to at least compensate for, if not correct for, selfabsorbtion in ^{201}Tl SPECT images. The theoretical derivation is exact, as far as it goes, but fails to include the (necessary) transition to the situation of multiple (angular) measurements. The empirical support could have been improved. However, contrary to the editorial stated by Dr. Strauss (*2*), in this case, the editorial does not have the last word.

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REFERENCES

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2. Strauss HW. The artful dodger. *J Nucl Med* 1992;33(4):3A.

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