than the slow component). It influences the early data fit only, leads to a lower initial PODV (Vo) estimate, and practically vanishes by the end of the first minute.

However, it is evident that the first minute of the externally recorded cardiac curve is not reliable and should not be used in any fit procedure. After intravenous injection, the right and left cardiac chambers are successively seen in the cardiac ROI and summed in the same first 15-sec frame (overestimating the plasma value). One minute is probably necessary before reasonable bolus mixing is reached. We thus never used the first minute data to fit the PODV curves.

The initial PODV, Vo, was introduced in the fit function for "automatic correction of the residual plasma component" (*7*), which always remains after classical background correction. As Vo estimation is largely dependent on the function used in the fitting procedure, its validation with monoexponential fit function is presented in the second paper (*6*). Indeed, Vo is estimated with the PODV decomposition algorithm in the renal (VoK) and background (VoBG) ROIs after ^{99m}Tc-DTPA injection and their ratio is shown to be, if not equal, very close to the renal (QK) to background (QBG) ROIs activity ratio in a blood-pool agent study with ^{99m}Tc-HSA.

It can be argued that if an identical percent error alters both VoK and VoBG, their ratio will not be affected. However, as VoK is estimated by an unequivocal linear PODV fit (our unpublished data show evidence that the integrated-plasma to plasma ratio is an unequivocal linear function of time with ^{99m}Tc-DTPA up to 20 min), a systematic 40% error in VoK is very unlikely.

The systematic 40% overestimation of VoBG suspected by Peters and Bell should therefore strongly affect the VoK/VoBG ratio, which then would not match the QK/QBG ratio. This is actually not the case.

The second point raised by Peters and Bell, i.e., the identification of the extravascular signal in the renal ROI, was already discussed in our second paper (*6*). The choice of our background ROI (BGROI) is justified by anatomical criteria only, assuming in addition that the virtually empty peritoneal cavity and the gastrointestinal tract seen together with muscles and fat tissue in BGROI have no significant contribution (i.e., no more significant than the 'large bag" seen in the kidney ROI).

We are aware that this assumption, as others, has its limitations and that the ideal solution is still to be found.

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P. Decostre
Y. Salmon
Hôpital Civil
Jumet, Belgium

Maximum-Likelihood Estimation: A Mathematical Model for Quantitation in Nuclear Medicine

TO THE EDITOR: As we are working on contrast detectability and quantitation in SPECT (*1*), we found great interest in the paper by Müller et al. (*2*). To understand the mathematical developments underlying the work, we consulted early published papers (*3–5*), where authors derive an expression for the noise power spectrum.

In all of these analyses, however, we found various errors, especially based on misuses of nonstationary process approximations to a stationary one which led to invalid results.

Indeed, if we assume that noise in projections is an uncorrelated process, its covariance function, γ, is defined by:

$$\gamma(1_1, \theta_1; 1_2, \theta_2) = \sigma^2(1_1,\theta_1)\, \delta(1_2 - 1_1, \theta_2 - \theta_1), \qquad \text{Eq. 1}$$

where $\sigma^2(1, \theta)$ represents the noise power density at point $(1, \theta)$ of the detector and δ is the DIRAC distribution.
If we denote the reconstruction filter W in the Fourier space as follows:

$$W(R) = |R|\, H(R), \qquad \text{Eq. 2}$$

where R is the radius and H(R) is the filtering function, with $H(0) > 0$, the noise power spectrum density Γ of the reconstructed image therefore is (*6*):

$$\Gamma(R_1, \theta_1; R_2, \theta_2) = S(R_1 - R_2, \theta_1)\, H(R_1)\, H^*(R_2)\, \delta(\theta_2 - \theta_1), \qquad \text{Eq. 3}$$

where $S(R, \theta)$ is the Fourier transform of $\sigma^2(1, \theta)$ along the 1-axis.

If, in order to study noise behavior, we use the approximation of the average power spectrum, M [APS] (*7*), we do not obtain:

$$M(R, \theta) = S(R, \theta)\, |R|\, |H(R)|^2 \qquad \text{Eq. 4}$$

as calculated in (*3–5*). In such a case, the value of the APS would be null at the origin of the Fourier space, and this condition results in the equality:

$$\int\int \sigma^2(1, \theta)\, d\theta\, dl = \theta \qquad \text{Eq. 5}$$

which implies

$$\sigma^2(1, \theta) = 0 \qquad \text{Eq. 6}$$

for all points of the detection space.

Thus, noise would be null, in the mean square sense over all projections. According to Poisson process properties, it would be null with probability one (*8*) (i.e., projections would be noiseless). Therefore the APS in tomography cannot behave as the ramp function for low frequencies, as mentioned in (*3–5*).

In fact, the true formula is:

$$M(R, \theta) = \Gamma(R, \theta; R, \theta) = S(0, \theta)\,|H(R)|^2 \text{ for } R \neq 0$$

and

$$= \int \sigma^2(0, \theta)\, d\theta\, |H(R)^2 \text{ for } R = 0 \qquad \text{Eq. 7}$$

In conclusion, the non-null d.c. component of the noise power spectrum is a characteristic of the measured physical process, but it is not dependent on discretization artifacts (aliasing) as argued in (*3*).

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F. Aubry
Y. Petegnief
H. Benali
U66-INSERM, Institut Gustave Roussy
Villejuif Cedex, France

REPLY: The comments of Aubry et al. (*1*) regarding work on the noise power spectrum (NPS) of CT images by our group (*2,3*) and by others are not convincing. They state that NPS [or average power spectrum (APS)] "cannot behave as the ramp function for low frequencies" because a zero-valued zero-frequency component would imply that the integral over the projection space of the projection noise is zero and, consequently, the projection noise itself must be zero everywhere. In fact, the value of the NPS at zero frequency is equal to the integral over all space of the *autocorrelation function of the image noise.* A NPS which is zero at zero frequency implies that the autocorrelation function must have negative values in order that its integral be zero. It is common knowledge that the noise in images reconstructed by filtered backprojection is characterized by these negative correlations. A NPS whose zero-frequency component is zero does not imply, as Aubry et al. apparently believe, that the projection noise is zero. Aubry et al. offer without proof an alternative formula for the NPS (their Equation 7) which does not have (what they believe to be) the objectionable property of being zero-valued at zero frequency. Since the source for this equation, apparently Aubry's doctoral dissertation, is not available to us, we cannot determine how they arrived at an expression so different from ours.

Aubry et al. attribute "errors" in three papers they reference (*2–4*) to "misuses of nonstationary process approximations to a stationary one, which lead to invalid results." Only one of these papers (*3*) even considered nonstationary noise, and Aubry et al. do not specify what these "misuses" are. In this paper, the "expected noise energy spectrum" near the center of a uniform, cylindrical phantom was calculated theoretically by explicitly treating the nonstationary projection noise as well as photon attenuation and its compensation. The calculation was tested by computer simulation. The other two papers referenced by Aubry et al. pertain to the NPS of CT images, for which the stationarity of the projection noise is a good approximation. Their statement that "the non-null d.c. component of the noise power spectrum . . . is not dependent on discretization artifacts (aliasing) as argued in [our paper (2)]" is also without foundation. We demonstrated by analysis and confirmed by computer stimulation and physical measurement that aliasing was the source of the nonzero d.c. component.

Credit for the recognition of the negative correlations that characterize CT noise and their source in the reconstruction process, attributed to us by Aubry et al., is actually due to Riederer, Pelc, and Chesler (*5*), who in an elegant and influential paper outlined the essential characteristics of the CT NPS and discussed their implications. Our contribution (*2*) was to demonstrate that when the digital nature of CT data was taken into account the spectrum deviated from that of Riederer et al. (*5*) in two respects; it was rotationally asymmetric and contained additional very low-frequency components due to aliasing. Our more recent paper (*3*) considered effects specific to nuclear medicine CT data, as described above. We do not defend the paper by Faulkner and Moores (*4*), grouped with ours by Aubry et al.; our differences with them are detailed in our paper (*2*) and in a subsequent exchange of letters (*6,7*).

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Marie Foley Kijewski
Harvard Medical School and Brigham and Women's Hospital
Boston, Massachusetts
Stephen C. Moore
Worcester Polytechnic Institute
Worcester, Massachusetts
Stefan P. Mueller
Universitaetsklinikum Essen
Essen, Germany
B. Leonard Holman
Harvard Medical School and Brigham and Women's Hospital
Boston, Massachusetts