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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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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