already been used extensively to study attenuation correction. Likewise, this would be appropriate for scatter correction in loosely realistic situations. Where we differ with Dr. Wackers, though, is that once a method is shown to be working effectively, we should accept that and move on to the next possible source of error. Constantly focusing on attenuation correction alone may be missing the point. For example, in London we are currently examining the impact of adjacent sources of radioactivity (roughly simulating a hot liver) on cardiac phantoms to gain some insight as to the regional artifacts that this causes. The motion issue may be answered by gating the data.

There has been so much novelty in the design and implementation of transmission scanning devices and the algorithms for the correction and reconstruction of the data that Dr. Wackers' plea for rigorous testing should be endorsed. The journal and its reviewers have a role to play in this, as do the manufacturers and the users. We may be in danger of throwing the baby out with the bath water, though, by constantly questioning whether attenuation correction is working properly.

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REPLY: We appreciate the comments of Drs. Bailey and Meikle regarding Dr. Wackers' editorial (*1*) and our article (*2*). We agree completely that our results should not cast doubt on the relevance of attenuation correction itself. Rather, we think that the results show that attenuation correction alone, as currently included in some commercially available γ camera software, should be used with caution because, under some conditions (e.g., transmission with ^{99m}Tc and for the patient population we considered), it can produce deleterious effects in the territory of the left anterior descending artery. Such results do not mean that the attenuation correction method does not work (several reports have shown that it actually does). The results mean that other issues that can interfere with attenuation correction should be considered before attenuation correction can be confidently included in routine practice. Two points should be considered:

First, an attenuation correction algorithm has been shown to work, assuming the attenuation map has been estimated properly (is not truncated, is registered properly with the emission data when using sequential transmission/emission imaging, and contains appropriate μ values). How truncation of fanbeam-acquired attenuation maps affects the result is controversial (*3*). It has been shown that misregistration can yield severe artifacts (*4*), whereas the effect of inaccurate μ values still needs to be clarified.

Second, other phenomena, such as scatter, motion, or depth-dependent collimator response, can be neglected when attenuation is not compensated because attenuation is the major degrading factor in cardiac imaging. However, when correction is made for attenuation, the artifacts created by these phenomena can be magnified and become a significant source of errors.

Our results, therefore, should not prompt the nuclear medicine community to reject attenuation correction but, rather, should stimulate further research about other effects that interfere with attenuation correction. Attenuation correction is definitely a huge step toward accurate quantitation in SPECT and is not a farce like the emperor's new clothes (*1*). However, we should all be aware that some other issues must be resolved to achieve reliable quantitative SPECT imaging.

Concerning the crucial point raised by Dr. Wackers of how to validate the development of artifact-free imaging methods, we suggest adherence to the recently published guidelines for evaluation of image processing procedures (*5*). By following these recommendations, clinical trials will become necessary after experimental validations have been made. More complete correction packages for SPECT myocardial perfusion imaging should undergo this type of evaluation process to guarantee that the emperor will be dressed appropriately and that his new clothes will be seen and appreciated by most nuclear physicians.

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Hybrid PET/CT Machines: Optimized PET Machines for the New Millennium?

TO THE EDITOR: We are moved to write to the journal by the recent Newsline article (*1*) concerning the development of single-gantry hybrid PET/CT machines. We wish to express our opinion regarding 1 particular issue: Should the quality of the CT images be maximized to equal the best stand-alone CT images?

The switch from high-energy-photon transmission data obtained from a current germanium- or cesium-source PET scanner to hybrid machines using low-energy, x-ray photon sources has 3 potential advantages for a patient. The first is generation of images that provide anatomic detail, which is related to the machine's