other, rather than on different days, the standard error of the scintigraphic estimate of left ventricular end-diastolic volume was similar (15.8 ml) to that found by Verani et al. (16.4 ml). We were encouraged, however, that the analysis of our subgroup of patients with angiographically normal wall motion provided a standard error of only 5.1 ml. Mean interobserver variability was 5.4 ml.

We hypothesized that ventricles which best conform to ellipsoid shape as assumed in area-length volume calculations provide the best correlation with our optimized count-based scintigraphic left ventricular volume measurement. If this is so, we may further hypothesize that greater differences which occur between contrast and scintigraphic volume measurements of abnormal ventricles might be on the basis of errors in the area-length technique. While contrast ventriculography remains the most accepted procedure for measurement of left ventricular volume, I feel that a statement of the inability of count-based left ventricular volume measurement to detect small physiologic or pathologic changes is unwarranted, in that our more automated method at least compares favorably with contrast ventriculography.

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REPLY: Dr. Burns reminds us that the use of water as attenuating and scattering medium is an oversimplification of the complex structures present in the thorax. We would certainly acknowledge that this is so. However, as emphasized by Links et al. (*1*), the choice of water seems justified because of the similarity between the attenuation coefficient of water, blood, and soft tissue. The photons emanating from the left ventricle must, of course, transverse the path that includes lung tissue, air, and the chest wall. Although the latter is denser than water because of its bone content, the inflated lungs are less dense than water, thus in effect the average attenuation coefficient is close to that of water. The derivation of the attenuation coefficient as performed by Burns et al. in patients with open heart surgery (*2*) may not be applicable to ambulatory patients because of the presence of blood in the pericardium and mediastinum in the former patients.

Dr. Burns states that his technique, using a semi-automatic edge detection, is more reproducible than manual techniques. There is no doubt that the more automatic the technique, the more reproducible it is. However, the issue here is not simply reproducibility, but rather accuracy. We believe that the commercially available software for semi-automatic detection of ventricular edges, based on a four quadrant threshold, is not very accurate in delineating the true ventricular edges, particularly in regions such as the septal and basal left ventricle. Using a similar semi-automatic technique, based on a second derivative and count-threshold algorithm, Links et al. (*1*) found a consistent underestimation of the ventricular volumes, as opposed to the manual determination, which correlated better with the angiographic volumes. It must be remembered, too, that the regional thresholds are determined by the operator, based on a subjective visual assessment of the best edge tracking.

Dr. Burns suggests that the reproducibility of the left ventricular depth may be improved by using a computer algorithm. We agree with that statement and, in fact, had suggested it in our paper as a potential means to improve the technique. Incidentally, although not clearly stated in their paper (*2*), apparently Burns et al. also determined the left ventricular and marker centers manually. Dr. Burns suggests that, using his technique, a superior correlation was found with contrast ventriculography. However, in his study, the radionuclide angiographic technique consistently overestimated the contrast volumes with a highly significant difference between the two techniques ($t = 7.8$, $p < 0.001$ by paired t-test). In this small series of 18 patients, all but two had normal left ventricular volumes. In these two patients with large left ventricular volumes, the overestimation by the radionuclide technique was very substantial.

Dr. Burns also points out that some of the discrepancies between contrast and radionuclide angiography may lie with deficiencies in the contrast techniques, all of which are well known, such as the ellipsoid assumption, single plane limitations, presence of dyssynergy, etc. We would suggest that in Burns' study, another limitation may have been a poor opacification of the left ventricular cavity due to the small amounts of contrast injected (as little as 25 ml). Although we agree with the potential, theoretical, and practical pitfalls of contrast ventriculography we feel it is a self serving argument to use contrast ventriculography as the "gold standard" and then justify the discrepancies in correlation by denigrating the "gold standard."

Thus, although Dr. Burns takes issue with our statement that "it is unlikely that the radionuclide technique will have enough accuracy to detect small, physiologic, or pathologic changes of left ventricular volumes" it is clear from any published data, including our own as well as Dr. Burns', that this is a realistic statement and may also apply to other techniques used to measure ventricular volumes including contrast angiography.

References

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