
The authors reply:
To the Editor:

The authors of the letter would have done well to add the word “our” to their opening sentences; viz., “In the light of our current practice . . .” They thus speak only for themselves, as it would be difficult to find many cardiologists who share their view that stress tests are meaningless and obsolete.

The authors state that they were astounded by the recent editorial of the NIH group (Redwood et al.); their response may be unique, as the cited editorial merely confirmed and emphasized what is well known — namely, that in certain populations the incidence of false positive stress tests is high, and that false negative tests do occur. To draw the conclusions from these findings that stress tests are worthless and should therefore be abandoned shows not only lack of comprehension as to the purpose of stress testing, but also ignores the fundamental process by which a clinician applies laboratory tests to the patient’s problem.

Drs. Bianco and Shafer are not correct in accusing us of ignoring the results of isotope studies. We are well aware of these results, but did not mention them because they had no relevance to the subject of our editorial. Nuclear cardiology is a new and very promising area of cardiac diagnosis which must still prove itself not only in terms of reliability, but also in terms of cost-effectiveness. The pioneering zeal of Drs. Bianco and Shafer is understandable, but they should be aware that most new diagnostic (and therapeutic) procedures often appear to be the best because sufficient time has not elapsed to recognize their limitations. We remember the enthusiasm generated by ballistocardiography and vectorcardiography which promised to solve all cardiology problems, and, on the other hand, we also remember the slow, almost reluctant acceptance of echocardiography as a worthwhile diagnostic adjunct. Initial judgments are not necessarily the correct ones. It is hoped that critical study, this time of myocardial imaging, is undertaken in the not too distant future by the NIH team or someone else.

We, and others, are able to obtain highly reliable diagnostic information from standard stress testing. Our proposed classification is intended to make the test even more reliable. For example, it is highly doubtful that among the false positive stress tests in the NIH study one would find a test classified by us as +++. In addition, we, too, have considerable experience with myocardial imaging and radionuclide ventriculography, but use these tests selectively in what we believe to be judicious manner; that is, when other tests are inconclusive and the information potentially gained is directly relevant to the patient’s management. We vigorously oppose the implication of Drs. Bianco and Shafer that all exercise tests should include nuclear imaging. Not only is this unnecessary, but it would also inevitably lead to further skyrocketing of the already astronomical costs of medical care.

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Aortic Stenosis with Heart Failure

To the Editor:
We read with interest the paper by Smith, McAnulty and Rahimtoola regarding aortic stenosis (AS) with heart failure.† These authors observed improvement of impaired left ventricular performance after aortic valve replacement, but raise the possibility that myocardial dysfunction might become so far advanced in some patients that it might not improve after valve replacement. We have reported a similarly gratifying response in patients with AS. Since ventricular dysfunction was more advanced in our series than in Smith’s, comparison of results helps to answer the question of irreversibility.

Ours was a consecutive series of 12 patients receiving aortic valve replacement for AS, with or without minor aortic regurgitation. All had clinical and radiographic evidence of heart failure, and most had secondary renal, hepatic and/or cerebral dysfunction. The mean left ventricular end-diastolic volume index was 189 ml/m² and the calculated ejection fraction (EF) ranged from approximately 0-0.20 (only two of Smith’s 18 patients had EF < 0.30). The transaortic gradients were often small (range 16–70 mm Hg peak-to-peak) despite small aortic valve areas, re-emphasizing Smith’s point that measurement of cardiac output and calculation of valve area is often crucial to recognition of the correct diagnosis. Five patients with associated coronary artery disease (CAD) received aorto-coronary grafts at the time of valve replacement. There were three early deaths attributable to continued left ventricular (LV) dysfunction, all among the CAD patients. An additional CAD patient died because of a strike early after operation. All eight survivors moved from class III–IV to class I–II. Postoperative treadmill exercise tolerance was 50–105% of normal. In six patients, there was marked diminution of ventricular size and improvement of EF (postoperative mean = 0.45, range 0.34–0.75).

We conclude that without CAD, even the most advanced LV dysfunction associated with AS can be expected to improve markedly with aortic valve replacement. When coronary disease accompanies LV dysfunction in AS, the mortality risk of surgery rises dramatically, presumably because there is more extensive scarring of the left ventricle than in “pure” AS. The long-term reversal of heart failure in one of our CAD patients, however, makes us reluctant to conclude that surgery should not be performed in this setting. Because of the hopeless prognosis of these patients without operation, perhaps even a small chance of success should be pursued.

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