

CORRESPONDENCE



Surgical Ventricular Reconstruction

TO THE EDITOR: Jones et al. (April 23 issue)¹ discount the possibility that the selection of patients was a factor in the negative outcome of the Surgical Treatment for Ischemic Heart Failure (STICH) trial (ClinicalTrials.gov number, NCT00023595). Their argument, however, would be much strengthened by providing information on the clinical profile and number of patients who were eligible for the trial but were not enrolled. Such information is sorely needed to determine the generalizability of the findings of a trial in which the yearly recruitment averaged only three patients per site.

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Patients with large aneurysms are rare in developed countries, mostly because of the use of advanced revascularization techniques. However, in developing nations, many patients still have this condition. In our center, 7% of patients undergoing CABG had dyskinetic aneurysms, and one fifth of the aneurysms were large. The operative rate of death in patients with large aneurysms was 4.1%; 1-year and 5-year survival rates were 95% and 91%, respectively.

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TO THE EDITOR: Jones et al. report that the addition of surgical ventricular reconstruction to coronary-artery bypass grafting (CABG) did not improve survival. Patients who were enrolled in this trial had dominant anterior left ventricular dysfunction. But no information was given on the percentage of patients who had large, dyskinetic left ventricular aneurysms, and no subgroup analysis was carried out. Patients with such aneurysms have poor intraventricular hemodynamics with increased preload and wall stress, resulting in a totally different natural history. Most recent studies have reported that 5-year survival is only 47 to 70% in patients with a medically managed left ventricular dyskinetic aneurysm.¹ Therefore, the conclusions of this trial may not be consistent in patients with akinesia, as compared with dyskinesia.

TO THE EDITOR: The report on the STICH trial does not define the role of surgical ventricular reconstruction in the management of heart failure because of the study's poor design and execu-

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tion. The outcomes of patients who underwent surgical ventricular reconstruction in this trial cannot be compared with those of more than 5000 patients in registries who had regional necrosis of 35% or more from previous infarction, a left ventricular end-systolic volume index of at least 60 ml per square meter of body-surface area, and an ejection fraction of 35% or less.¹⁻³ In the STICH trial, patients were randomly assigned to undergo surgical ventricular reconstruction only if they had dominant anterior dysfunction of the left ventricle and an ejection fraction of 35% or less, with no viability data confirming left ventricular necrosis or extent of damage (13% of the patients had no history of infarction). Only 50% had either akinesia or dyskinesia.

The original study submission called for the measurement of left ventricular end-systolic volume in all patients with the use of cardiac magnetic resonance imaging.⁴ However, this measurement was performed with the use of echocardiography and in only 38% of the patients.⁵ Jones et al. report that patients who were assigned to undergo CABG with surgical ventricular reconstruction had a 19% reduction in the end-systolic volume index, as compared with a reduction of at least 40%, as reported in multiple trials of surgical ventricular reconstruction. Therefore, such procedures were not performed according to grant-accepted guidelines and were performed on improperly selected patients.

A valid trial of surgical ventricular reconstruction requires the selection of patients with at least 35% anterior necrosis on single-photon-emission computed tomography, an end-systolic volume index of at least 60 ml per square meter, and a postoperative reduction in the end-systolic volume index of at least 30%. Patients with insufficient volume reduction must be excluded from the analysis because of an inadequate procedure.

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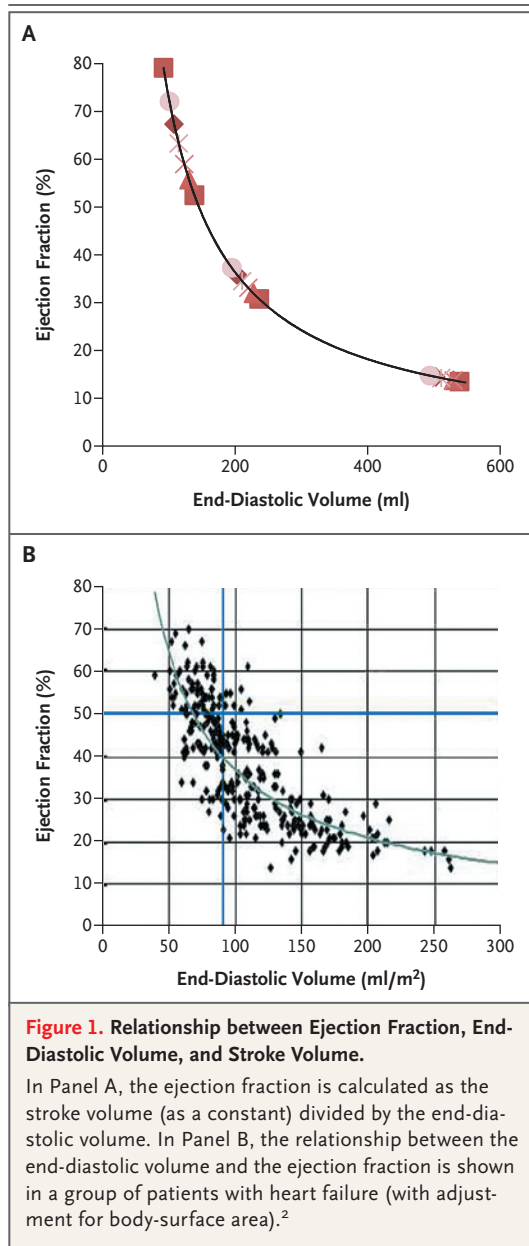
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Drs. Athanasuleas and Buckberg report coholding a patent on a product for ventricular reconstruction and receiving royalties from and holding stock options in Somanetics. Dr. Wechsler reports receiving consulting fees from and holding stock options in Bioventrix. No other potential conflict of interest relevant to this letter was reported.

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TO THE EDITOR: Ventricular reconstruction surgery is based on the concept that remodeling is an adverse event that should be prevented. The left ventricular stroke volume is often normal in patients with chronic heart failure and is a function of the metabolic needs of the body. In contrast, the ejection fraction is contingent on myocardial contractility and left ventricular wall thickness.¹ If the stroke volume is held constant, the end-diastolic volume must change with the ejection fraction (Fig. 1A), as is illustrated in patients with heart failure (Fig. 1B).² Changes in end-diastolic volume may reflect the need to normalize the stroke volume as an adaptive physiological mechanism. After myocardial damage, mechanisms such as fluid retention and increased filling pressures result in ventricular remodeling and a normalization of the stroke volume. This paradigm could explain the disappointing results of the STICH trial, as well as the Batista operation³ and extracardiac support mesh implantation.⁴



A reduction in the end-diastolic volume by surgical means would be predicted to cause an acute fall in stroke volume, with potentially harmful consequences. A greater understanding of the pathophysiology of heart failure is imperative before embarking on further such trials.

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terns of structural and functional remodeling of the left ventricle in chronic heart failure. *Am J Cardiol* 2008;102:459-62.

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THE AUTHOR REPLIES: Ghali's question about the influence of clinical-site enrollment performance on the generalizability of our trial results can be addressed from analysis of extant data that have yet to be published. It is possible to quantify the baseline risk spectrum of all patients who underwent randomization according to characteristics of the study center and country.

Hu et al. question whether our results apply to patients with large left ventricular aneurysms. Core laboratory data that were carefully interpreted without knowledge of randomized study-group assignment are currently being analyzed to determine whether specific descriptors of global and regional left ventricular function and left atrial filling identify patients who do better or worse after surgical ventricular reconstruction.

Athanasuleas et al. misunderstand and incorrectly report the simplification of criteria for Hypothesis 2 enrollment.¹ Clinical sites reported that 83% of patients in Hypothesis 2 had dysfunction involving more than 35% of the anterior wall, and 84% of patients had an end-systolic volume index of more than 60 ml per square meter on randomization. In addition, the correspondents overstate the body of interpretable data from nonrandomized studies that have measured postoperative volumes. These studies used multiple techniques at widely disparate time points after the procedure and cannot serve as a reliable end point for comparison.

MacIver emphasizes the importance of an increase in the end-diastolic volume to respond to augmented stroke-volume needs as an explanation for the lack of benefit from surgical reduction in left ventricular size. The heart augments end-diastolic volume to retain stroke volume at high heart rates in high-performance athletes,² in patients recovering from acute myocardial infarction,³ and in patients who undergo a brief interval of cardiopulmonary bypass during CABG.⁴ The heart appears to preferentially use preload reserve to augment cardiac output more than contractil-

ity reserve when there is limited oxygen available to the myocardium. Perhaps surgical reduction of left ventricular chamber size only changes the position, but not the area, of the left ventricular pressure–volume loop as cardiac-output needs fluctuate during activities of daily living in patients with ischemic cardiomyopathy.

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Valsartan and Recurrent Atrial Fibrillation

TO THE EDITOR: Disertori et al. (April 16 issue)¹ describe the effects of valsartan on the recurrence of atrial fibrillation. The disappointing results of the trial may be explained by two important limitations. First, no data were provided concerning how long patients were known to have had either atrial fibrillation or underlying heart disease. We would expect that the extent of remodeling would become more severe and even irreversible in patients with a longer history of atrial fibrillation or underlying heart disease. In patients with a shorter history, however, remodeling processes are less advanced, providing more opportunities for blockade of the renin–angiotensin–aldosterone system (RAAS) to be effective.² Second, RAAS blockade was probably started too late in the trial — namely, when sinus rhythm was already obtained. Upstream therapy requires more time to influence remodeling processes, and it would have been better if valsartan had been started several weeks before instead of at least 2 days after obtaining sinus rhythm.³ Thus, the question still remains whether RAAS blockade is effective in maintaining sinus rhythm if it is started as soon as possible after presentation with atrial fibrillation.

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to maintain sinus rhythm in patients with long-lasting persistent atrial fibrillation: a prospective and randomized study. *Circulation* 2002;106:331-6.

TO THE EDITOR: Disertori et al. report that they found no significant reduction in the incidence of recurrent atrial fibrillation among patients receiving valsartan, as compared with those receiving placebo. The study is basically a secondary prevention trial. Previous studies have presented positive results for the use of angiotensin II receptor blockers (ARBs) for the secondary prevention of atrial fibrillation in a relatively small number of patients.¹ In contrast, previous trials and meta-analyses involving more than 50,000 patients have suggested more pronounced effects of angiotensin-converting–enzyme (ACE) inhibitors or ARBs for the primary prevention of atrial fibrillation.^{2,3} In primary prevention trials, ACE inhibitors and ARBs might prevent the occurrence and progression of structural and electrical remodeling as the substrate for atrial fibrillation,⁴ but the remodeling process might be completed and irreversible in the secondary prevention setting. Thus, a large, randomized, prospective, placebo-controlled, multicenter trial to test an ARB for the primary prevention of atrial fibrillation would be mandatory before concluding that ARBs are not effective in preventing this condition. In such a trial, an evaluation of changes in cardiac-chamber dimensions would be useful in assessing the mechanism of prevention of atrial fibrillation.

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