the SVR group, significantly less than the greater than 30% figures reported in multiple registries.

The extent of left ventricular volume reduction is thought to be crucial for an optimal outcome.² Isomura and associates³ reported an 84% 5-year survival when left ventricular end-systolic volume index was reduced to less than 90 mL/m² and volume reduction was greater than 30%, especially when the elliptical, conical shape of the heart was restored after the SVR operation. Excellent survival has been achieved when residual volume is less than 60 mL/m². Michler and colleagues⁴ reported that 44% of patients in the STICH data did not have adequate volume data for preoperative and postoperative analysis. Furthermore, 18.5% of the patients undergoing SVR had no left ventricular volume reduction. What was done to these patients? Without postoperative volumes, all STICH data must be skeptically viewed.

Unfortunately, STICH was poorly implemented. The study morphed from the original design, perhaps because of difficulty in recruiting patients. Too many centers with inexperienced surgeons chose patients poorly and did an inadequate operation. Conversely, registry data have been amazingly consistent across multiple continents, leading to the European Society of Cardiology's recommendation that SVR be added to CABG if postinfarction left ventricular necrosis is present with left ventricular end-systolic volume index greater than 60 mL/m² in centers with a high level of surgical expertise.⁵

The STICH reports are confusing, because each seems to focus on one variable that drives analysis, and conclusions are often out of context. The data we need are simple and should be revealed.

STICH investigators should report outcomes of patients who had a previous anterior infarction, heart failure, an ejection fraction lower than 35%, and anterior scar identified by viability testing, preferable gadolinium magnetic resonance angiography. Preoperative and postoperative volumes can sort out the extent of left ventricular volume reduction and its effect on outcomes: simple, but thus far elusive.

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A BALANCED ASSESSMENT OF THE STICH TRIAL

Reply to the Editor:

We appreciate the letter from Athanasuleas and colleagues regarding our substudy of the surgical ventricular reconstruction arm of the Surgical Treatment for Ischemic Heart Failure (STICH) trial, which showed that myocardial viability testing does not discriminate patients who would derive a mortality benefit from adding surgical ventricular reconstruction (SVR) to coronary artery bypass grafting (CABG). All studies have limitations, and we have acknowledged ours. We would, however, like to correct some inaccuracies in the letter of Athanasuleas and colleagues.

First, they claim that having a viability study was a requirement for inclusion into the study; however, viability testing was never a requirement of the SVR arm of STICH. The 276 patients (of 1000) who had viability testing had these tests performed for various reasons at the discretion of the recruiting physicians. Further, Athanasuleas and colleagues suggest that the 46 patients with anterior or apical viability should have been excluded from SVR; however, demonstration of nonviability was not an inclusion criterion for SVR. Instead, ventricular size and regional function were determinants. We agree that such viability may have influenced the outcome of CABG plus SVR; this in fact was the principal reason that our analysis was done. We disagree, however, that this is a group of patients who would have been excluded in previous studies of SVR or surgical registries. Indeed, viability studies were not a requirement for inclusion in any of these studies. The fact that Athanasuleas and colleagues mention surgical registries illustrates that SVR had not previously been rigorously studied, as it was in STICH. We cannot comment on ventricular volumes assessed by single-photon emission computed

tomography after SVR, because we did not systematically acquire that data.

Athanasuleas and colleagues also state that registry data led to “the European Society of Cardiology’s recommendation that SVR be added to CABG if postinfarction left ventricular necrosis is present with left ventricular end-systolic volume index greater than 60 mL/m² in centers with a high level of surgical expertise.” These guidelines actually state that CABG with SVR “may be considered” for such patients, and this is a class IIb recommendation.¹ The updated 2014 European Society of Cardiology guidelines on revascularization are similar but add the comment that CABG with SVR may be considered in such patients “especially if a postoperative LVESV [left ventricular end-systolic volume] index <70 mL/m² can be predictably achieved” (again class IIb).² The requirement for a postoperative left ventricular end-systolic volume index less than 70 mL/m² comes from STICH data.³

We welcome critical appraisal of our findings, but we think that it should come from an accurate and balanced assessment of the data.

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INFANT EUTHANASIA IS MORALLY UNACCEPTABLE

To the Editor:

I read with interest the recent editorial by Udo Schuklenk¹ on the justification of euthanasia for infants with serious anomalies. I offer the following response.

First, Dr Schuklenk is careful with his statistics, because the situation in Europe is not reassuring but rather alarming. His claim that pediatric euthanasia is infrequent reflects, as he admits, replacement of lethal injection by food and water deprivation and an uncertain amount of underreporting. In 2011, Pereira² showed that despite mandatory reporting, 205 to 40% of Dutch and almost 50% of Belgian adult euthanasia cases were unreported. Violations of protocol were statistically more common in unreported cases. Written patient consent was documented in just 12% of unreported cases and in only 72% of reported cases. High-profile cases, such as the euthanasia of Belgian twins who were going blind and the killing of a Dutch woman who declined transfer to a nursing home, underscore the dangerous subjectivity of “unbearable suffering.” Wim Distelmans, one of Belgium’s premier euthanasia providers, also serves as copresident on the Euthanasia Control and Evaluation Committee, which oversees appropriateness of cases,³ and there is evidence that the 2-physician review process in euthanasia consults is pointless.⁴ I hasten to add that although euthanasia for patients older than 12 years became legal in The Netherlands in 2002, Dutch physicians have been openly killing patients since the mid-1980s.⁵ Euthanasia is still technically not legal in The Netherlands for children younger than 12 years.

The slope is already slipping. Physicians have been operating outside the law; protocol violations, underreporting, and conflicts of interest are rampant. Consequently, many Dutch persons fear being pointlessly killed if they are admitted to the hospital.⁶ Extending such an uncontrolled system to children is unconscionable. Only half of Dutch physicians agree that parents should even be involved in the decision to end their child’s life,⁷ and there are concerns regarding the developmental appropriateness of involving children in euthanasia decisions.⁸

Second, consider a congenital repair that is now routine, atrioventricular canal repairs in infants with Down syndrome. In the 1970s, these were not done; standard opinion was that quality of life and surgical risk was too hopeless.⁹ Dr Schuklenk then might have advocated euthanasia for such infants, with the same arguments he made in his 2014 article¹: futility of treatment, suffering, and economic