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Reply to the Editor:

We appreciate Ying-Fu Chen and colleagues for their interest in our article,¹ and we are delighted with their thoughtful insights into our results. We also thank the Editor for the possibility to reply.

Previous studies have shown the clinical features and difference of acute aortic dissection (AAD) between China and other countries.² However, it is necessary to keep in mind that the previous studies in China were sporadic rather than multicenter registered research. To better understand the current situation of patients with aortic dissection in China and the differences between China and developed countries, the Registry of Aortic Dissection in China (Sino-RAD), which is the first registry system in China designed to provide an unbiased representative population of patients with AAD, was established including 15 large cardiovascular centers. The preliminary investigation disclosed the differences between Sino-RAD and the International Registry of Acute Aortic Dissection (IRAD), as shown in the article. Of note, the ratio of patients with hypertension was lower than in other reports from China.^{3,4} This may be due to a partially different database. Another interesting finding is the lower in-hospital mortality of type A AAD. This may be due to the group of younger patients and new “standard” therapy for type A dissection, which is based on morphologic characteristics of Chinese people.⁵

However, as mentioned in our article, the IRAD data and Sino-RAD data are at different periods. In addition, most of the Sino-RAD data are from Chinese Han people, which cannot reflect Asian people. With the development of medical technology, the disease shows different features at different times. There may be a certain degree of bias to conclude the differences between Sino-RAD and IRAD. We have to admit that this is a great limitation of the article. The real differences need to be clarified by using the same period database. This is what we are trying to do next. We are trying to conduct a common research with IRAD, German Registry for Acute Aortic Dissection Type A, and other databases. As the search goes on, more detailed research data will be illustrated, such as characteristic of type A AAD, type B AAD, intramural hematoma, and so forth. The optimal approach would be to make a thorough evaluation of AAD.

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THE STICH TRIAL DATA: KEEP IT SIMPLE

To the Editor:

Elegance is simplicity; confusion is complexity. The Surgical Treatment for Ischemic Heart Failure (STICH) Trial investigators have reported another analysis focusing on viability.¹ One thousand patients were randomly allocated to undergo coronary artery bypass grafting (CABG) or CABG plus surgical ventricular reconstruction (SVR) for anterior wall severe left ventricular dysfunction. They concluded that assessment of myocardial viability by single-photon emission computed tomography did not discriminate patients who would profit from the addition of SVR.

Several deficiencies of the STICH trial must be reviewed. Only 276 of the 1000 enrolled patients had a viability test, a central requirement for inclusion in the original study design. Holly and colleagues¹ report that 46 of these patients (17%) had anterior or apical viability and were randomly allocated to CABG or CABG plus SVR. Why was SVR done in these patients? They should have been excluded. SVR was done because of visual regional dysfunction assessed intraoperatively. Unsurprisingly, there was no difference in outcome. Maybe this was hibernating ventricle. Clearly this was not a group for SVR, as reported by numerous surgical registries.

The big news from this new study compares patients with anterior scar who underwent CABG versus CABG plus SVR. No difference in outcomes was noted. Preoperative volumes are reported, and they are large, but lacking is the crucial measurement of postoperative volume. Previous STICH data showed a left ventricular reduction of 19% in

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