and reversed saphenous vein grafts to the right and circumflex coronary arteries. The right, left anterior descending, and left circumflex coronary arteries were grafted in that order with proximal first technique for the reversed saphenous vein grafts. This actually led to immediate improvement of the right ventricular ejection fraction, as confirmed on echocardiography. Then with the finger-dip technique we passed a 16-gauge needle through a pledgeted purse-string 4-0 polypropylene (Prolene) suture on the right ventricular anterior wall to insert a guide wire across the septal tear into the left ventricle. The 20-mm Amplatzer septal occluder device was then deployed through a 9F vascular sheath entirely into the necrotic cavity in the septum. The device deployment did not appear satisfactory initially because of the nature of the large septal necrotic cavity. It soon became clear, however, that the device had entrenched itself nicely, was well trapped in the septal aneurysm, and had nearly completely obliterated the large 3:1 shunt. In 2008, we had a 13-month follow-up with stable device intracardiac and septal position with New York Heart Association class I functional status.

Unlike Love and colleagues,¹ we deployed the device totally intracardiac and were able to discharge the patient on the 9th postoperative day. The patient exhibited no evidence of subsequent myocardial failure and continued to show improved contractile function despite a continued small residual ventricular septal defect. We believe that placing a large disk on the exterior anterior right ventricular surface may contribute to late myocardial failure because of fibrosis on a device primarily meant for intravascular placement. This kind of technique would also be limited in application if a right ventricular infarction is present, as in our case, because of the risk of erosion into the infarcted myocardium.

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CAN WE IMPLANT VALVED MITRAL PROSTHESIS WITHIN ALL KINDS OF MITRAL RINGS THROUGH A TRANSAPICAL APPROACH?

To the Editor:

We appreciate the admirable efforts of Holzhey and colleagues¹ to treat a patient who had double valve disease and coronary heart disease with poor left ventricular function. The patient had undergone mitral valve reconstruction and coronary artery bypass grafting 12 years previously and was in New York Heart Association functional class IV. The echocardiographic examination revealed structural degeneration of the repaired mitral valve, with moderate to severe mitral stenosis and grade II insufficiency. Further echocardiographic findings included severely stenosed aortic valve disease and moderate tricuspid insufficiency. Moreover, 2 de novo lesions of the proximal right coronary artery were detected by coronary angiography. Summarizing

briefly, it was a complex case for intervention. Nonetheless the patient was treated successfully.

Anson and associates² performed transcatheter mitral valve-in-valve implantation in circular-shaped prosthesis in 11 patients. They concluded that proper anchoring of the balloon-expandable valve within the sewing ring of the mitral prosthesis ensured stability and minimized paravalvular leakage. We think that some points about the procedure performed in the mitral position have to be clarified to avoid misunderstanding.

First, what kind of ring had been implanted in previous surgery: rigid, semirigid, or flexible? We know that some of the flexible rings, which are easily deformable, have polyester cores. The Carpentier-Edwards classic ring (Edwards Lifesciences Corp, Irvine, Calif), which is hardly deformable, has a titanium core, and Carpentier-McCarthy-Adams IMR ETlogix ring (Edwards Lifesciences), which is nondeformable, also has a rigid titanium core.³ The structures of the rings are important even if the patient hasn't undergone coronary artery bypass grafting. If the ring has rigid structure such as a titanium core, balloon valvuloplasty or balloon inflation to expand the valved prosthesis may not result in circular mitral ring. This may lead to paracommissural leaks between the prosthesis and the ring. Holzhey and colleagues¹ claim that there were 2 trivial jets between the prosthesis and the ring. Was the previously implanted ring flexible? Another point to be clarified is the balloon pressure during balloon valvuloplasty or expansion of a valved prosthesis in the mitral position. In such cases, how much is the pressure that we can apply to make the ring in circular shape?

In conclusion, this is an appreciable study. We as readers thank Holzhey and colleagues¹ for sharing their knowledge and experience about such a complicated case.

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IS THE CAUSE OF DEATH IN HYPERTROPHIC CARDIOMYOPATHY LOW PRESSURE GRADIENT IN LEFT VENTRICULAR OUTFLOW?

To the Editor:

We congratulate Schaff and colleagues¹ on their recent study. We wish, however, to emphasize some points that we believe they should have taken into consideration.

We could not figure out how to correlate the preoperatively used drug with the survival rate (see original Table 2¹). To agree with the results, one should standardize the last echocardiographic data of the lost patients with those of the living patients. We think that preoperative echocardiographic status of the patient could not be correlated with the survival of the 6th postoperative year. We could not clearly understand whether it was postulated that the wall thickness of 22 mm (see original Figure 4¹) did not have an impact on survival or whether one would bring the survival to an expected level with the use of drugs? We think

that the original Figure 2 is in contradiction with the original Figure 4.¹

Ommen and associates² previously stated that the pressure gradient does not have an impact on (cardiac) survival because their mortality was also 6% in the nonobstructive group. Nevertheless, there was limited information because of the lack of postoperative echocardiographic data in the myectomy group with respect to postoperative pressure gradient, wall thickness, and left ventricular diameters.

The standardization of the groups after inclusion of the final echocardiographic data of the lost patients would yield the definitive results of this study.

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EARLY SURGICAL TREATMENT FOR INFECTIVE ENDOCARDITIS: DOES IT COME AT THE EXPENSE OF WORSE NEUROLOGIC OUTCOMES?

To the Editor:

With much interest we read the recent article of Funakoshi and colleagues,¹ suggesting superior long-term outcomes for patients with infective endocarditis after early surgical treatment relative to a purported conventional treatment strategy.

Although the results seem in line with general consensus, several methodologic issues arise, affecting the validity of the study's conclusions.

First, we would like to comment on the definition of the study groups. The conventional treatment group includes patients who have been treated with antibiotics only, as well as patients who have been operated on at a later stage. In our opinion, these patients should not be combined into a single treatment group. To illustrate this, the in-hospital mortality is 5% for both the early and late surgical groups. On the other hand, the mortality of the conventional treatment group seems higher (5% vs 13%; $P = .08$), possibly caused by unfavorable results in the nonsurgical group. It would have been more informative to have 3 groups for comparison: patients after early surgical treatment, patients after late surgical treatment, and patients who have been treated with antibiotics only. Correspondingly, it is problematic to interpret the result of the propensity-matched comparison, because it remains unclear how many patients in the conventional treatment group have undergone surgical treatment. To assess the role of early surgical treatment in infective endocarditis adequately, it would be more instructive to compare the results of early surgical treatment with those of late surgical treatment.

Second, the chosen period of 14 days after the initial diagnosis as cutoff point for early versus late surgery seems questionable. Several studies show that the risk of embolic events dramatically drops as early as 1 week of antibiotic treatment.² Accordingly, the European guidelines³ on this specific subject advise that early surgical treatment should be performed within several days after the diagnosis, instead of after 14 days, whenever there is an indication for early operation.

Another point of concern is the postoperative neurologic outcome. Among patients who had complicating stroke on admission, 30% had