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<http://dx.doi.org/10.1016/j.jtcvs.2012.05.084>

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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<http://dx.doi.org/10.1016/j.jtcvs.2012.03.089>

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