Letter by Subramanian et al Regarding Article, “Comparison Between Transcatheter and Surgical Prosthetic Valve Implantation in Patients With Severe Aortic Stenosis and Reduced Left Ventricular Ejection Fraction”

To the Editor:

We read with interest the recent article by Clavel et al.1 With respect to their findings, we have the following comments and questions.

First, transcatheter valve implantation (TAVI) relieves valvular and subvalvular obstruction, whereas surgical aortic valve replacement (SAVR) relieves only valvular obstruction, which may explain the observed differences. We liberally use subaortic myectomy as an adjunct to SAVR for treating subvalvular obstruction and aortic root enlargement for valvular obstruction to left ventricular ejection; perhaps this is the fair comparison with TAVI.

Second, stentless aortic valve replacement provides superior reduction of transvalvular gradients.2 It is notable that none of the surgically implanted valves were stentless valves. The authors have previously shown that the follow-up mean gradient after percutaneous aortic valve implantation is lower than that of surgical patients who receive a stented bioprosthesis, but comparable to those who receive a stentless bioprosthesis.3 Because postoperative gradients were being examined in this study, an inclusion of stentless valves would have been appropriate.

Third, larger valves were implanted in smaller aortic roots with TAVI, perhaps because of balloon valvuloplasty. How did the authors use, and is it modified for patients with a reduced ejection fraction?

Fourth, the SAVR group is heterogeneous, because they were treated with 9 different aortic valve prostheses! The significantly higher incidence of severe patient-prosthesis mismatch in the SAVR group (29% versus 16% in the TAVI group) further suggests that this study is somewhat of an apples-to-oranges comparison. It underscores the importance of appropriate valve choice, root enlargement strategy, and implantation technique.

Fifth, the details of the myocardial protection strategy, which influences postoperative cardiac function, are not mentioned, with the exception of the use of mild hypothermia. What strategy do the authors use, and is it modified for patients with a reduced ejection fraction?

Sixth, the 30-day mortality in patients treated with open SAVR was quite high, at 12%. Chikwe et al4 reported a mortality rate of 4% for aortic valve replacement and 8% for aortic valve replacement + coronary artery bypass grafting in patients with an ejection fraction of <30%. A brief comment on the higher mortality rate would be useful.

Seventh, Quere et al5 have shown that absence of a contractile reserve is related to high operative mortality, but does not predict the absence of ejection fraction recovery. The latter finding was corroborated in the present study. Was there a correlation in this study between the absence of contractile reserve on dobutamine stress echocardiography and perioperative mortality?

And finally, 90% of the patients in the SAVR group had complete revascularization or did not need any revascularization, compared with 43% in the TAVI group. Was MRI used to assess myocardial viability? Clearly, patients with varying extents of nonviable myocardium on MRI would not be expected to have the same degree of cardiac functional recovery.

We fundamentally agree that TAVI is a reasonable option for high-risk, elderly patients with multiple comorbidities. However, SAVR with adjunctive myectomy and/or aortic root enlargement continues to provide excellent outcomes, even in patients with a reduced left ventricular ejection fraction. We remain skeptical about whether “TAVI is associated with better LVEF recovery,”6 but agree that it is “an interesting alternative to SAVR in patients considered at high surgical risk.”7

Disclosures

Dr Misfeld serves on the advisory board for Bayer and the speakers bureau for St. Jude.

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References

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