Letter to the Editor

Aortic valve repair for aortic insufficiency in adults: a contemporary review and comparison with replacement techniques

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I recently read the review article by Carr and Savage [1], which focuses on the aortic valve repair (not the aortic valve-sparing replacement) and its late outcome involving different techniques for patients with different etiologies. They should be commended for this work and I thank them for drawing attention to the aortic valve repair, which has received considerably less attention than mitral or tricuspid repairs in cardiac surgery.

However, I believe that early and late outcomes after the repair should be tabulated and identified according to the etiology of the aortic valve disease. In their review, many articles containing 50% or more patients with rheumatic disease are included for review to compare postoperative early and late outcomes with valve replacements or Ross procedure. On the other hand, they admitted that patients with rheumatic disease have an increased incidence of recurrence after repair in the conclusion. It is not fair to include cases of rheumatic etiology in the repair group and compare its outcome with the replacement group. Readers should be aware of this inconsistency in this review to better understand the present outcome after aortic valve repair.

Another important point which needs correction or clarification is the analysis of reported article cited in the reference. In their analysis, number of patients receiving aortic valve repair in Shafer et al.’s [2] article is 156 (Table 1). However, by carefully reading the article, Shafer et al. reported 68 patients who underwent aortic valve repair + valve-preserving surgery and 88 patients who underwent only valve-preserving surgery. Also in Table 1, the authors (Carr and Savage) showed that the percentage of leaflet plication or triangular resection in Izumoto et al.’s [3] report is 76%. However, if one carefully reads the article, the percentage of such procedure is far less than 76%. By roughly analyzing this Table 1, I do believe that the tables in their review or the discussion needs correction or further clarification for the purpose of precision.

However, I really thank the authors to update and spotlight this old but still evolving field in cardiac surgery at a time of drug-eluting stents, when the cardiac surgery seems drifting back towards valve surgery.

References


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Reply to the Letter to the Editor

Reply to Izumoto

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I would like to thank Dr. Izumoto for his thoughtful comments regarding aortic valve repair. As he pointed out, many of the articles cited had a large percentage of rheumatic valves that were repaired. As rheumatic heart disease is a progressive and relentless type of pathology, the results with repair were much worse in this cohort than in those without the disease. Ideally, Dr. Savage and I would have loved to separate out all those with rheumatic heart disease and compare the outcomes of repair to those without the condition, and that would be an exceptionally good publication! Unfortunately, since the individual articles that were included in our manuscript did not separate out the results by type of pathology in every instance, this was simply not possible. However, several articles, such as those by