We thank the editor for the opportunity to respond to the Letter to the Editor by Castelvecchio et al. [1] and appreciate their critical appraisal of our article Early and late outcome of left ventricular reconstruction surgery in ischemic heart disease: a systematic review of the literature [2]. We performed this systematic review of the literature on outcome after left ventricular reconstruction surgery by a MEDLINE database search combined with a manual search of major cardiothoracic and cardiology journals (January 1980 to January 2005) and found a weighted average early mortality of 6.9% (62 studies; 12,331 patients). Cumulative 1-, 5- and 10-year survival were 88.5%, 71.5% and 53.9%, respectively. Endoventricular reconstruction showed a reduced risk for both early (RR = 0.79, \( p < 0.005 \)) and late (RR = 0.67, \( p < 0.001 \)) mortality compared to the linear repair (early: RR = 1.38, \( p < 0.001 \); late: RR = 1.83, \( p < 0.001 \)). Early and late mortality were mainly cardiac in origin, with as predominant cause heart failure in respectively 49.7% and 34.5% of the cases. Concomitant CABG significantly decreased late mortality (RR = 0.28, \( p < 0.001 \)) without increasing early mortality (RR = 1.018, \( p = 0.858 \)). Concomitant mitral valve surgery showed both an increased risk for early (RR = 1.57, \( p = 0.001 \)) and late mortality (RR = 4.28, \( p < 0.001 \)). A pooled analysis, when well designed and appropriately performed, is a powerful tool to combine in a single conclusion the results of different studies conducted on the same topic. Random effect models were used to control for within-study and between-study variability (random effects modeling). In addition, meta-regression analysis was used to adjust for the influence of patient demographics and prognostic indicators that co-varied with the dependent variable. Despite the advantages of a pooled analysis, such as increased statistical power of a comparison and improved estimation of the effect of a treatment, there are several limitations of our analysis. Publication bias may have influenced our results, since observational studies with a poor outcome may not have been published in full-length papers. Second and most important, surgical techniques and approaches have improved over time, which affects the current results. Third, since to date no prospectively randomized controlled trials have been published concerning LV reconstruction surgery, all studies included in this analysis were observational reports. Therefore, we agree with Castelvecchio et al. that biases limit the results of our systematic review of the literature.

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**Letter to the Editor**

**Think twice while inserting a transannular patch**

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Congratulations to the authors for this valuable study [1]. The combination of a ventriculotomy and free pulmonary insufficiency is implicated in the deterioration of right ventricular function and the development of arrhythmias after tetralogy of Fallot (TOF) repair [2]. Recently, early outcomes of TOF repair are expected to be excellent irrespective of surgical technique and timing of repair. Therefore, the aim of the surgical treatment should be the avoidance of long-term complications and reoperations. The use of a transannular patch may result in free pulmonary insufficiency. This causes right ventricular dilation that in turn compresses the left ventricle. The functional reserve and myocardial contractility of the right (and possibly left) ventricle decreases. Chronic volume overload on right ventricle after repair of TOF causes dilation predisposing to life-threatening ventricular arrhythmias and sudden cardiac death.

It is well known that infundibular stenosis may develop secondary to VSD [3]. An infundibular patch was recommended to patients with pulmonary annular stenosis with a z-score less than –3 [4]. We conducted a study previously emphasizing the necessity to apply the diagnostic criteria of TOF more carefully in clinical practice [5]. After all these findings we felt it to be necessary to evaluate this study, since there were 18 cases with pulmonary valve replacement in group 1 whereas there were none in group 2.

a. Why were the cases with a z-score < –1.5 included in this study?

b. Right ventricular wall thickness reaching the values equal to that of left ventricle and infundibular wall hypertrophy secondary to VSD could have regressed postoperatively. Regarding this; after closure of VSD and relief of pulmonary stenosis, the need for infundibular patch would obviously decrease (particularly for those with a z-score < –1.5) since right ventricular and infundibular thickness would have regressed. From this point of view, would this affect the surgical treatment guidelines?

c. We conducted a study regarding the dextroposition of aorta (true dextroposition) [5]. This study made us consider that infundibular patch is necessary in case of true dextroposition.

References


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Reply to the Letter to the Editor

Reply to Kestelli et al.

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We thank Dr Kestelli et al. [1] for their comments on our study [2]. We fully agree with the point that surgical repair of tetralogy of Fallot (ToF) should aim nowadays at limiting the damage to the right ventricular outflow tract (RVOT) and pulmonary regurgitation (PR). Therefore, we proposed a valve-sparing surgical strategy that aims to use transannular patches (TAP) only in patients with a very small pulmonary annulus (z score ≤4) and to enlarge the pulmonary annulus to low normal values (z score in the range of −2) in case a TAP is deemed necessary [2]. We presented that this strategy is feasible resulting in a marked reduction in TAP rate and without an increase in right ventricular (RV) pressure.

We admit that our threshold for using a TAP is high (pulmonary annulus z score ≤4) but could demonstrate that a pulmonary valve-sparing approach is possible in patients with a z score of −4 or larger as long as the repair also consists of inspection of the valve and commissurotomy. Accordingly, the incidence of commissurotomy was higher adhering to the valve-sparing strategy as opposed to a historic cohort of patients operated before this strategy was implemented.

We also agree that it is necessary to carefully apply the diagnostic criteria of ToF when conducting a study on these patients. We therefore reported comprehensively on all patients with this diagnosis who underwent repair in the period from 1997 to 2006. Only patients with double outlet right ventricle, pulmonary atresia and absent pulmonary valve and those in whom Fallot repair consisted of pulmonary valve replacement (PVR) were excluded. Patients were not excluded from the study on the basis of the preoperative pulmonary annulus size as suggested by Kestelli et al. since it was intended to investigate a surgical strategy applied to Fallot patients with the full spectrum of RVOT and pulmonary valve anatomy.

We take issue with Kestelli et al. on the subject of the incidence of PVR in our two groups. By study design, no patient received PVR as part of surgical repair. However, the incidence of PVR for residual PR was higher in our historic cohort (group 2; see Table 1 of the manuscript) [2]. This result might relate to a higher incidence of PR in this group. However, the longer follow-up period of these patients might also explain this difference. Further follow-up studies are necessary to confirm that restrictive RVOT repair truly limits the incidence and severity of PR.

We fully agree that any postoperative RVOT obstruction might decrease when RV hypertrophy regresses with time. This has to be considered when judging the immediate postoperative result. The University of Alabama group showed a decrease in right-to-left ventricular (RV/LV) pressure-ratio with time after Fallot repair [3]. Nollert et al. found no association between a RV/LV-pressure-ratio of 0.7 and early or late survival [4]. Hirsch et al. found that a RV/LV-pressure-ratio >0.7 is a risk factor for reoperation after neonatal repair [5]. We therefore accept an RV/LV-pressure-ratio of up to 0.7 immediately after RVOT reconstruction before deciding to insert a TAP.

References


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