Reply to Letter to the Editor

Reply to Lentini et al.

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We appreciate Dr Lentini and co-workers [1] for their comments on our report [2]. Infective endocarditis (IE) involved aortic valve nowadays remains to be associated with high morbidity and mortality [3]. Eradication of infection and correction of the associated haemodynamic abnormality are fundamental to patients indicated for surgical treatment. Valve replacement is the preferred choice of most surgeons for its easy manipulation, but it carries the risk of prosthesis-related complication. We successfully performed vegetectomy for a patient with IE involving the aortic valve [2]. The patient had no IE recurrence and had a normal-functioning aortic valve at 19 months follow-up. The main advantages of vegetectomy can include preservation of the normal, native valve; preservation of normal haemodynamic function; and abolition of the risks of anticoagulative medication. This procedure is indicated in the aortic valve when IE is in the early stage without an evidence of annular abscess, perforation of valve leaflet or destruction of subvalvular apparatus. The case in our report is not a common phenomenon in the clinic.

In our patient, although all vegetations were completely excised with low-strength electrocautery and repeatedly scrubbed with iodophor, normal blood sedimentation rate and repeated negative blood culture could be obtained until antibiotic therapy was continued for 12 weeks [2]. In our view, an in-depth eradication of the infected area by vegetectomy in the three leaflets could be incomplete during surgery. A larger-impact blood flow to the aortic valve than to the other valves can also contribute to recurring infection. Therefore, we agree that vegetectomy may carry the risk of recurrent infection, and, in a selected patient, vegetectomy with valve sparing may be a viable option in early-stage IE involving the aortic valve.

We fully agree that vegetectomy can also be used for patients with IE involving the mitral valve. A larger excision of the infected area should be performed during surgery, and therefore a combined surgery such as mitral valve repair is also necessary, as previously reported by Lukács et al. [4]. The authors described that a valve repair was completed by reinforcement and reduction of the posterior ring, and by commissuroplasty. No prosthetic ring, or any other prosthetic material, or polyfilament sutures were used. We appreciate these finer technical aspects. However, duration of 12 months follow-up is inadequate for this patient. According to our experience, the patient would benefit better from implantation of prosthetic ring in mitral valve repair in the long term.

References


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Letter to the Editor

The importance of methodological rigour in quality-of-life studies

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I have read with interest the article of Loup and co-workers on the quality of life (QOL) of grown-up congenital heart disease patients after congenital cardiac surgery [1]. I applaud the authors for their interest in QOL. Indeed, the increased life expectancy of patients with congenital heart disease has urged clinicians and researchers to focus on issues beyond the quantity of life. Furthermore, it is praised that these authors question the widespread assumption that patients with congenital heart disease have a lower QOL.

The findings of Loup are, to a large extent, in keeping with the results of our QOL studies. Previously, we also found that the QOL in adults with congenital heart disease is good, and sometimes even better than that of healthy counterparts [2,3]. However, when QOL is measured in terms of functional status, as was done in the Loup study, the scores of patient groups are mostly lower than those of normative groups [4]. In concordance with Loup and co-workers, we found only marginal differences between diagnostic groups [5], and age was not associated with good, moderate or poor QOL [4].

However, I have concerns about the rationale of the authors to use the SF-36 and their method of analysis. First, the conceptual basis to use the SF-36 to measure...
(health-related) QOL is poor. Too often, the SF-36 is used as a health-related QOL instrument, whereas it says more about the perceived health status of the respondents. Second, the authors correctly explain in the methods section that the raw scores for each of the eight dimensions of the SF-36 have to be transformed into a score ranging from 0 to 100 [1]. However, in the results, they report SF-36 scores that are higher than 100. In the discussion section, they argued, ‘the authors did not transform the raw SF-36 values, allowing age- and gender-matched comparison [1].’ It is surprising that the authors use this comparison as an argument for not transforming the dimension scores. There is a plethora of articles in which the dimension scores were transformed, but still compared with matched data from normative groups. The fact that the scores were not transformed to a scale from 0 to 100 might be responsible for their findings being aberrant from the previous studies that have used the SF-36 in adults with congenital heart disease. Third, the SF-36 contains eight dimensions. These dimensions can be used to compute a physical component and mental component score. In the Loup study, also an overall QOL score was calculated. Computing such an overall SF-36 score is not advocated and seems inappropriate because constructs that are conceptually different are mixed. All these factors have potentially hampered the internal validity of the study.

I join the authors in their statement that QOL research is needed to design, adapt and optimise the specific needs and complex follow-up of this special group of patients [1]. QOL studies require, however, a firm conceptual background and the use of rigorous research methods.

References


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