Aortic Valve Repair With Bovine Pericardium

Zohair Al-Halees, Begonia Gomciza and Carlos M. G. Duran

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Furthermore, there were too few patients in their series with leads in place longer than 6 years. The mean duration of implantation was closer to 3 years, which means that half of the leads might have been removed by traction alone, without the need for extraction tools. (The implant duration of 82 years in 1 patient was clearly a typographic error.) Leads in place longer than 4 or 5 years tend to be more difficult to explant, and leads in place for more than 10 or 15 years may be entrapped so tightly that even removal under direct vision may be extremely challenging. To provide a more realistic picture of the problems entailed in explanting leads, there should be a substantial number of leads that have been in place for 10 years or more.

I am concerned that the favorable conclusions are not appropriate, even based on the Telfer and associates' data. Readers should not be lulled into security if they are tempted to embark on a program of lead extraction. It is a dangerous enough procedure to demand special training, and once embarked upon, it demands great caution, experience, and judgment.

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Reference

1. Telfer EA, Olshansky B, Cadman C, et al. Telectronics 330-801 atrial lead extraction via the subclavian approach. Ann Thorac Surg 1997;64:175–80.

Retrosternal Fat Pad for Prevention of Suppurative Sternitis

To the Editor:

We read with interest the article by Kejeriwal and Paterson [1]. In their series of 101 patients, 23.8% were diabetic, and we agree that the incidence of suppurative sternitis or mediastinitis is increased significantly in diabetics, particularly after bilateral internal mammary artery use. We would like to know the incidence of mediastinitis in their similar type of cases before they started doing this technique. We would also like to know why the term "suppurative sternitis" was used instead of mediastinitis? Was the study double blinded? We would like clarification about their reexplored patients: were they the same patients who had sternal dehiscence or superficial wound infection? Our last question is, were any of their patients reoperated on, and how easy or difficult was the dissection?

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Reference

1. Kejeriwal NK, Paterson HS. Retrosternal fat pad for prevention of suppurative sternitis. Ann Thorac Surg 1997;63:1484–5.

Reply

To the Editor:

I thank Drs Kapadia and Russo for their interest in this technique. The series presented is only intended to help illustrate the benefit of the technique. It was a retrospective review of the first 101 patients undergoing this procedure at Westmead Hospital. A comparable series of patients undergoing bilateral mammary artery grafting without the use of the retrosternal fat pad is not available. Since the initial series there have been occasional patients with suppurative sternitis, but the incidence remains less than 1.5%. Where the fat pad has remained adherent to the back of the sternum there has been no mediastinal suppuration. Hence, suppurative sternitis and mediastinitis are not necessarily synonymous. Our policy is to perform urgent reoperation on any patient with a purulent discharge from the presternal wound, before apparent sternal dehiscence. The patients with mechanical dehiscence in this series had obstructive lung disease and experienced bronchospasm in the postoperative period. This was managed with bronchodilators and systemic steroid therapy in view of the severity of their bronchospasm. Adequate sternal stabilization was achieved at rewiring, and no wound infection occurred in these patients. The difficulty associated with reentry and mobilization of adhesions at late reoperation is extremely variable regardless of the techniques of closure used at the primary operation. In my very limited experience I have encountered no difficulties. The knowledge that the right ventricle should not be adherent to the sternum is assuring. The experience of others (Donald Ross, Sydney) has demonstrated that the fat pad can be easily peeled off the right ventricle as the pleural surface was applied to the epicardial surface at the time of original closure. As my experience with this technique grows I become more convinced of its benefits.

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Aortic Valve Repair With Bovine Pericardium

To the Editor:

With great interest we read the article "Aortic Valve Repair of Congenital Aortic Stenosis With Bovine Pericardium" [1]. The idea is appealing and obviously effective. However, we have a great concern about the use of bovine pericardium for the repair.

Early in our experience with aortic valve reconstruction we used bovine pericardium [2]. Results were initially encouraging. However, as longer follow-up became available, it was clear to us that early calcification in bovine pericardium is a real problem at approximately 8 years of follow-up. Of 27 patients who had aortic reconstruction performed with bovine pericardium, 6 required reoperation (infection, 1; fibrocalcification, 5). Actuarial survival and freedom from structural degeneration at 8 years were 82.2% ± 9.6% and 76.2% ± 10.7%.

Based on this experience, our advice would be not to use bovine pericardium in this aortic repair, particularly as the patient population that would benefit from this procedure is young.

As to the best material to use, one can say that whatever we

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have available is far from ideal. However, we think that glutaraldehyde-treated autologous pericardium is probably better. In our own patients who have undergone aortic valve reconstruction with autologous glutaraldehyde-treated pericardium, actuarial freedom from structural deterioration at 8 years is 96.8% ± 2.25%.

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References

1. Tolan M, Daubeney P, Slavik Z, Keeton B, Salmon A, Monro J. Aortic valve repair of congenital stenosis with bovine pericardium. Ann Thorac Surg 1997;63:465–9.
2. Duran C, Kumar N, Gometza B, Al-Halees Z. Indications and limitations of aortic valve reconstruction. Ann Thorac Surg 1991;52:447–54.

Reply

To the Editor:

My colleagues and I appreciated the comments of Drs Al-Halees, Gometza, and Duran and continue to be enthusiastic about our technique of repairing bicuspid aortic valves. We have been using bovine pericardium for a variety of intracardiac repairs for about 8 years and have not had any problems with calcification as yet.

We were well aware of Dr Duran's previous work, and it is very disappointing to hear that the bovine pericardium used for valve repair has become calcified. We therefore share their view that it would probably be better to use glutaraldehyde-treated autologous pericardium as the long-term results with this seem better. We plan to change our practice accordingly.

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Left Ventricular Aneurysm Repair

To the Editor:

Regarding their article "Repair of Left Ventricular Eneurysm: Long-Term Results of Linear Repair Versus Endoaneurysmorrhaphy," Shapira and associates [1] listed several limitations of their study, such as failure to perform a prospective, randomized trial. There is, however, a much more serious problem. The study was actually sequential, with an older technique having been abandoned in favor of the newer technique in midstudy. The primary author, who performed most of the operations, asks the reader to believe that there were no improvements in surgical techniques, myocardial protection methods, improved surgical ability based on greater experience, introduction of new medications, or improved medical management with the passage of time between 1987 and 1995. During this same 8-year period, the results in all areas of cardiac surgery have improved significantly. The results with linear aneurysmectomy might also have improved during this time span, had Shapira and associates persisted with that technique. Endoaneurysmorrhaphy may indeed be a better procedure than linear aneurysmectomy for the treatment of this condition, but one must be very cautious in the interpretation of data obtained from any retrospective, nonrandomized, (and especially) *sequential* series of operations.

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Reference

1. Shapira OM, Davidoff R, Hilkert RJ, Aldea GS, Fitzgerald CA, Shemin RJ. Repair of left ventricular aneurysm: long-term results of linear repair versus endoaneurysmorrhaphy. Ann Thorac Surg 1997;63:701–5.

Reply

To the Editor:

My colleagues and I thank Dr Fishman for his interest in our article. We fully agree that the retrospective nature, the relatively small sample size, and the fact that the groups were operated on in series and not in parallel are all significant limitations. However, there was no difference in the preoperative risk profiles and indications for operation, and the vast majority of the operations (85%) were performed by the senior author. Therefore, there were no significant differences between the groups with respect to operative technique, method of myocardial protection, cardiopulmonary bypass and aortic cross-clamp times, and the degree of myocardial revascularization. Although there could have been more subtle differences in the perioperative and postoperative management (mainly reflecting increased surgeon and team experience), we do believe that because patient and operative profiles were similar, the comparison of outcomes between the groups is valid. These limitations can be avoided by comparing the techniques in a large-scale prospective, randomized study. With the decreasing number of left ventricular aneurysms this probably can be accomplished only in a multicenter study. Multicenter studies have their own inherent limitations, particularly with regard to the quality control, uniformity of surgical techniques, and perioperative management [1, 2].

In summary, despite the study limitations, we believe that the data are strong, the statistical analysis is sound, and both support the conclusions. Carefully designed, well-analyzed retrospective case-control studies still have a role in clinical research.

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