Risk factors influencing outcome after surgical treatment of destructive endocarditis

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We thank Drs Al-Attar et al. for their positive reaction to our article. Our constant aim is to further improve the results of surgical treatment during ongoing endocarditic infection accompanied by aortic root destruction. We believe that there are three main possibilities of achieving this.

First and foremost, early operation is of the essence and accurate preoperative diagnosis is essential to optimize the timing. We found that double-valve disease usually spreads from the aortic valve to the mitral valve when aortic valve infection with the potential danger of spread to the mitral valve is not recognized early enough. Prompt recognition often means that only the aortic valve requires surgical attention. In our most recent work, the following predisposing factors for mortality were identified: septic shock (OR 3.44, CI 0.85-13.9) and false diagnosis (unrecognized root abscess with severe damage) in the referring hospital (OR 11.667, CI 3.127–43.522) (as yet unpublished data).

Secondly, valve selection plays an important role in preventing reinfection after operation. While there is no ideal infection-resistant valve prosthesis, in our experience the best choice at present is the homograft or the Shenhig No-React prosthesis.

Thirdly, the choice of the optimal surgical valve replacement technique will always greatly influence the result achieved when severe damage means that valve reconstruction is not possible.

Dr Al-Attar and colleagues reported their favorable experience with translocation of the aortic valve for severe prosthetic valve endocarditis, ventriculo-aortic disconnection and aortic root abscess. The surgical technique used means that the coronary ostia are closed. Theoretically, there is a possible life-threatening complication because the myocardial blood supply is entirely dependent on a venous bypass while the coronary ostia are oversewn. This was already published by Danielson in the early 1970s—see book chapter by Hetzer et al. [1] where a case is illustrated.

We agree that inadequate excision of the infected local tissue during operation can cause the surgical procedure to fail. Therefore, excision of the infected part of the aortic wall and debridement of all infected adjacent tissue is essential regardless of the surgical technique or type of material used. We congratulate the colleagues from France on their excellent results. At our institution we have not used this technique, but prefer to use the ‘abscess exclusion’ technique as presented by Knoossal et al. [2].

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References


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Letter to the Editor

Arterial versus vein graft patency in coronary artery bypass grafting patients with ischemia-directed repeat angiography

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We read with great care and interest the recent article by Dr Shah and colleagues in the May 2005 issue of EJCTS [1]. First, we concur with the authors that the in situ right internal thoracic artery (RITA) graft is probably underutilized by surgeons as is implied in their title. The >93% arterial grafting in their re-angiography coronary artery bypass grafting (CABG) series indicates a high quality...
program that emphasizes arterial grafting. It was noteworthy that, despite their liberal definition of graft failure (≥50% stenosis, occlusion or string sign), the authors reported only 47 (or 7%) anastomotic failures out of 679 total grafts in 219 symptomatic patients with ischemia-directed angiography. These excellent graft patency results—at 3-plus years mean follow-up—were generally true for isolated ITA, radial and vein conduits as well as overall. That said, this article also poses important questions that require further elucidation on the authors’ part, and seems to make at least one unjustified conclusion.

The reader is not provided with how many of the 219 patients had failed grafts and if any had multiple failed grafts. Yet, one can safely conclude that the ischemia was unrelated to graft failure in at least 172 (or 79%) of the 219 restudied patients. This finding is substantially different from other related reports in symptomatic patients—and it is unclear why that would be [2,3].

The low 5% recurrent ischemia restudy rate following CAGB with radial (>3 years mean follow-up) may in part result from incomplete capture of re-studied patients and/or a very stringent threshold for ischemia-directed repeat angiography. In either case, this will limit the generalizability of the reported findings. To what extent are the authors satisfied that their re-angiography capture is complete? Next, to put the data in proper perspective, it is critical to provide readers with additional information. What fraction of the 4782 baseline CAGB with radial cohort were actual Radial/ITA graft patients (presumably a large majority)? This will help ascertain the true rate of repeat angiography after radial/ITA CAGB—since all 219 patients had both radial and ITA grafts. What fraction of the asymptomatic versus symptomatic radial/ITA patients received additional vein grafts? Here, despite the small number of available vein grafts in the present study, it remains of interest to compare radial versus vein patency specifically in the subset of patients that received both these conduits.

We contend that the results reported by Dr Shah and colleagues can reasonably be used to paint a significantly different picture. Specifically, inasmuch as the angiography follow-up for this CAGB with radial series is complete, the low ischemia-directed angiography (<2% of patients per year) reported by shah et al. [1] may be the strongest evidence to date in support of a greater role of radial conduits in CAGB. Indeed, this conforms to our group’s recent report comparing closely matched vein and radial patients (all of whom received left ITA to left anterior descending pedicle graft) [2]. There, we showed that using radial as a second arterial conduit as opposed to vein resulted in improved late survival especially after the third postoperative year [2]—a finding that closely rivals survival following CAGB with bilateral ITA grafting [4]. In that same study, it was also shown that for patients receiving both radial and vein conduits (i.e. each patient served as own control), absolute radial graft failure was significantly less than that of vein grafts [2].

Based on all the above, we suggest that the authors’ concluding statement that “When additional grafts are required, there is no evidence to suggest that either radial artery or saphenous vein is superior.” may not be objectively derived from the presented data. In fact, this conclusion—which is largely based on their multivariable linear mixed model analysis—seems at odds with their own cumulative patency data (Turnbull Method) in Fig. 3 [1]. Given that these results are based on data from a series where radial grafting dominates (53.5%) and vein grafting is exceptionally rare (6.6%), we suggest that: (1) the odds are necessarily stacked against radial, (2) it is reasonable to question whether the results are a consequence of over-modeling, and (3) a more appropriate vein comparison group may be to analyze ischemia-directed angiography results from the vein grafting sub-cohort of the authors’ remaining 1674 primary CAGBs over that same time period.

References


Reply to the Letter to the Editor

Reply to Zacharias et al.

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We find much to agree with the comments of Zacharias et al. in the Letter to the Editor. Our differences are largely a matter of interpretation.

Regarding multiple grafts: 50 subjects had two grafts, 106 had three, 51 had four, 10 had five and one had six. There were 180 patients (82%) who did not have a graft failure