I want to thank Dr Saha for his kind remarks regarding our recent study [1, 2]. His observations are relevant, since there are varying philosophies regarding the configuration of the internal mammary arteries (IMAs) in coronary artery bypass graft surgery (CABG).

Our study represents the experience of four different surgeons over 17 years (1994–10). In response to whether the IMAs were used ‘in situ’ or as a ‘Y’-graft, practitioners favoured different IMA configurations. Generally, the right IMA (RIMA) was used as an ‘in situ’ conduit when grafting the ramus intermedius or a very proximal obtuse marginal branch. When grafts were necessary in more distant territories off the left circumflex artery, the RIMA was either anastomosed to the ascending aorta (AA) or to the hood of a vein graft on the AA when the IMA was of small caliber. The majority of RIMAs were used to graft the circumflex territory. The ‘Y’-configuration represented a minority of cases in our series.

In Table 5 of our study, the stroke rate for the off-pump left IMA (LIMA)-saphenous vein graft and bilateral IMA (BIMA) groups was 0%. We use a true no-touch technique of the aorta when the AA appears to harbour significant atherosclerotic disease by transesophageal echocardiography (TEE). Intraoperative TEE was in routine use at the beginning of our study. When significant disease was found on the AA, the RIMA was either anastomosed to the LIMA in a ‘Y’-type construct or anastomosed to the AA using a non-clamping occluder type of device. Propensity matching was performed using the preoperative characteristics of both groups; no pre-screening for stroke was undertaken in the construction of the different cohorts. Several techniques can be used to avoid stroke, but none of them appears to be infallible [3]. As we all know, stroke is multifactorial and can rarely be attributable to one etiology. The most successful approach appears to be careful preoperative screening of CABG patients, so that all the different sources of embolic disease can be evaluated and different intraoperative strategies can be utilized to prevent them.

I read the technique described by Dr Saha et al. [4] of their in situ IMA graft construct, and it appears to be easy to implement. Nevertheless, a technique should be tailored to the surgeon performing the procedure, so he/she feels comfortable with it. I am not sure any particular strategy has been conclusively proven to be superior to another, as it is related to the experience the group has developed in that particular approach.

Finally, I agree with Dr Taggart [5] that there exists robust published data debunking the old theory that the use of BIMA carries a higher rate of early mortality and/or significant morbidity. Obviously, clinical judgment must be exercised when deciding to use these conduits, particularly in such patients like the morbidly obese and poorly controlled diabetics. However, this cannot justify the low use of BIMA in USA (4%) and Europe (10%) [6]. The LIMA use became the standard of care through three decades of evidence-based research. I believe that we have reached a point where BIMA use should become a quality metric to assess the performances of different surgical units undertaking CABG.

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**REFERENCES**


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**LETTER TO THE EDITOR**

**Is partial decalcification of posterior mitral annular bed logical in all mitral valve replacement procedures?**

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The publication by Nezic et al. [1] highlights a surgical approach for partial decalcification of the posterior mitral annular bed in mitral valve replacement. They suggest partial debridement of the calcium bulk in the posterior mitral annulus and resection of the affected portions of the posterior mitral leaflet, so as to facilitate valve insertion and prosthetic leaflet opening. They then detached the anterior mitral leaflet (AML) from the annulus and relocated it posteriorly to completely cover and buttress the partially decalcified mitral annular bed. For this purpose, they inserted 2-0 polyester pledgeted mattress sutures, first through the AML near its anatomical free edge, then through ventricular myocardium and fibrous areas in the calcium bulk remnant and, finally, either through the posterior mitral leaflet remnant at annular level or through annular tissue.

In the paper, we think that there may be a few matters to be clarified. First, we understand from Figure 1 that the authors have inserted the 2-0 polyester pledgeted mattress sutures through the calcium remnant. We readers know that nearly all cardiac surgeons have been using round-tipped needles in cardiac surgical procedures, not sharp-tipped ones. In inserting 2-0 polyester sutures into the calcium remnant using round-tipped needles, surgeons may experience moments of risk, since several penetrations of the partially-debrided calcium bulk with round-tipped needles may dislodge it from the posterior annular bed. This issue may cause early postoperative posterior wall rupture, which carries quite a high mortality rate. If the posterior annular plane is not appropriate for suture placement, we may rupture, which carries quite a high mortality rate. If the posterior bed, this issue may cause early postoperative posterior wall complication.

In conclusion, it is a valuable study. We, the readers, thank the authors for sharing their experience and knowledge of surgical approaches for mitral valve replacement in the case of posterior annular calcification. The explanation of the point described above may necessitate further investigation into overcoming possible postoperative complications.

REFERENCES


LETTER TO THE EDITOR RESPONSE

Reply to Tavlasoglu et al.

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We would like to thank Dr Tavlasoglu et al. [1] for their interest in our study, which was recently published in the European Journal of Cardio-Thoracic Surgery [2]. Their major remark on our technique of mitral valve replacement (MVR) is that passing of the suture needle through the calcium remnant (within the posterior mitral annular bed) might dislodge some calcium