particular, current smokers versus former smokers and lifetime non-smokers. (2) Information on intraoperative recruitment maneuvers was not reported. Such maneuvers may have implications during the perioperative period. (3) Atelectasis was diagnosed clinically and not by a radiologist, and possible influences of preoperative and postoperative factors were not taken into account. We have responses to these points.

First, our trial was randomized. This created intervention and control groups that were balanced with regard to demographic, anthropometric, and respiratory factors. Randomization does not rule out effect modification (point 1); however, it does ensure that an overall treatment effect is not biased (point 3).

Second, subgroup analyses are controversial. International guidelines focus on prespecified subgroup analyses to avoid selective reporting.¹ Our trial was not powered to detect subgroup effects, and we did not specify any in advance other than preoperative chronic obstructive pulmonary disease (we originally aimed to stratify by chronic obstructive pulmonary disease, but there were insufficient patients with this comorbidity). If we were to carry out subgroup analyses, the findings would be very difficult to interpret. Failure to demonstrate statistical significance might well be due to low power, whereas observation of statistical significance might well be due to chance. For example, Esquinas and colleagues mention the potential influence of body weight: 17 patients with body mass index greater than 30 kg/m² had bilevel positive airway pressure, whereas 17 had usual care.

Third, our trial was designed pragmatically to estimate the effect of bilevel positive airway pressure in a real-life setting. Noninvasive ventilation was applied for a median of 11 hours. In the trial, as in usual care, "lung recruitment" (point 2) typically constituted lung reexpansion

and the application of positive end-expiratory pressure. Assessment of atelectasis by the senior clinical team was also pragmatic (point 3). From a specialist respiratory perspective, such assessment might be considered insensitive; however, it reflected accurately the information on which management decisions are made in practice. Moreover, we did show a substantial difference in atelectasis frequency; better sensitivity would be expected to increase, not decrease, the observed difference.

Finally, we would like to point out that their summary of the findings of our trial ignores our primary outcome (prespecified in our analysis plan) and our explanation of the reason for failing to observe differences between groups in predicted forced expiratory volume in 1 second and other outcomes that one would expect to differ. As we acknowledged and tried to explain in the article, we had expected the respiratory outcomes to parallel the clinical ones. Because this expectation was not met, we recommended a larger multicenter trial at the end of our Discussion section. This is one point on which we agree. Just as one swallow doesn't make a summer, one trial by itself very rarely brings about (or warrants) a change in practice. We recommended that a larger multicenter, randomized trial of bilevel positive airway pressure be carried out to decide whether the promise suggested in our trial can be substantiated.

Emad Al Jaaly, MD, MRCS^a
Francesca Fiorentino, PhD, MSc, BSc^b
Barnaby C. Reeves, MSc, DPhil^c
Philip W. Ind, MA, FRCP^d
Gianni D. Angelini, MD, MCh, FRCS, FETCS, FMedSci^b
Scott Kemp, MBBS, BSc, FRCA^e
Robert J. Shiner, MRCS, FRCP^d
^aCardiothoracic Surgery
 Hammersmith Hospital
 Imperial College NHS Trust
 London, UK

^bCardiothoracic Surgery
 National Heart and Lung Institute
 Hammersmith Hospital
 Imperial College
 London, UK

^cClinical Trials and Evaluation Unit
 University of Bristol
 Bristol Royal Infirmary
 Bristol, UK

^dRespiratory Department
 National Heart and Lung Institute
 Hammersmith Hospital
 Imperial College
 London, UK

^eDepartment of Anaesthesia
 Hammersmith Hospital
 Imperial College NHS Trust
 London, UK

Reference

- Schulz KF, Altman DG, Moher D, CONSORT Group. CONSORT 2010 statement: updated guidelines for reporting parallel group randomized trials. *Ann Intern Med.* 2010;152:726-32.

<http://dx.doi.org/10.1016/j.jtcvs.2013.07.004>

MITRAL VALVE REPLACEMENT IN THE PRESENCE OF MASSIVE CALCIFICATION

To the Editor:

I was pleasantly surprised to read the novel technique reported by Husain and colleagues.¹ I reported on the technique used at the All India Institute of Medical sciences way back in 1988,² which my group used when confronted with a heavily calcified mitral annulus. We used a piece of autologous pericardium to cover the raw surface of the brittle and sandy remnant after limited débridement of calcium and to prevent calcific emboli. This patient was followed up and seen again in 2001, at which time I reported the favorable result of 13 years free of embolism.³ He was last seen in 2009 and was well. Others⁴ have also reported the successful use of this technique.

Although Hussain and colleagues' technique¹ is indeed novel, it complicates mitral surgery by the need for aortic approach and adds a considerable amount of foreign material. It is indeed surprising that our publications have not been referred to in their article.

Arkalgud Sampathkumar, MCh
Department of Cardiothoracic and
Vascular Surgery
Pushpanjali Insititute of Cardiac
Sciences
Ghaziabad, India

References

1. Hussain ST, Idrees J, Brozzi NA, Blackstone EH, Pettersson GB. Use of annulus washer after debridement: a new mitral valve replacement technique for patients with severe mitral annular calcification. *J Thorac Cardiovasc Surg.* 2013;145:1672-4.
2. Sampathkumar AS. Massive mitral and myocardial calcification—technical maneuvers for successful valve replacement. *Int J Cardiol.* 1988;20:279-82.
3. Kumar AS. Massive mitral and myocardial calcification: technical maneuvers for successful valve replacement. *Int J Cardiol.* 2001;78:103-4.
4. Güden M, Kazimoglu K, Sanisoglu I, Sagbas E, Yaman R, Akpinar B. The use of autologous pericardium for complicated mitral valve annulus. *Asian Cardiovasc Thorac Ann.* 2004;12:7-10.

<http://dx.doi.org/10.1016/j.jtcvs.2013.06.054>

Reply to the Editor:

We thank Dr Kumar for his comments and appreciate the opportunity to clarify the objectives of our technique.¹ We also acknowledge the technique described by Dr Kumar with autologous pericardium in a patient with severe mitral annular calcification and his favorable results at 13 years of follow-up.^{2,3} Because the *Journal* limits references in reports on surgical technique to 5, we were unable to acknowledge this case report as well as the work of others who have published about mitral annular calcification.

Dr Kumar's technique focuses on avoiding calcium embolization, whereas ours focuses on prosthetic valve size, prosthesis seating, and avoiding periprosthetic leak. We debride calcium until it is possible to implant a prosthesis of acceptable

size. The polytetrafluoroethylene felt washer sandwiched between the annulus and prosthesis¹ is an extension of the sewing ring, making it generous and soft, to distribute stress more evenly on all sutures and to help prevent periprosthetic leakage with softer tying of the sutures. We believe that our approach also reduces the risk of atrio-ventricular disruption or ventricular rupture and valve dehiscence.

Although we agree that the amount of foreign material should be minimized, the risk of infection is very low. Interestingly, Dr Kumar and his group⁴ used polytetrafluoroethylene felt instead of standard annuloplasty rings in 710 of 818 patients who underwent mitral valve repair and reported only 3 cases of infective endocarditis at a mean follow-up of 44.9 ± 33.2 months. We have seen no cases of endocarditis and 1 minor perioperative stroke in our series. It is possible that Dr Kumar's concept of covering the raw, débrided area with autologous pericardium reduces the risk of embolism; however, this remains to be proved. Of course, the techniques could be combined.

Use of an annulus washer when replacing the mitral valve does not require an aortotomy. The aortotomy is added when there is a large amount of calcium on the ventricular side of the annulus anteriorly and in the areas of the two trigones, because this allows exposure of these areas and facilitates débridement of calcium and placement of valve sutures. The aortotomy does not complicate the operation; rather, it makes it easier, faster, and safer.

We greatly appreciate the opportunity to address the queries in Dr Kumar's letter.

Syed T. Hussain, MD^a
Jahanzaib Idrees, MD^a
Nicolas A. Brozzi, MD^a
Eugene H. Blackstone, MD^{a,b}
Gösta B. Pettersson, MD^a
^aDepartment of Thoracic and
Cardiovascular Surgery

Heart and Vascular Institute
Cleveland Clinic
Cleveland, Ohio

^bDepartment of Quantitative Health
Sciences
Research Institute
Cleveland Clinic
Cleveland, Ohio

References

1. Hussain ST, Idrees J, Brozzi NA, Blackstone EH, Pettersson GB. Use of annulus washer after debridement: a new mitral valve replacement technique for patients with severe mitral annular calcification. *J Thorac Cardiovasc Surg.* 2013;145:1672-4.
2. Sampathkumar AS. Massive mitral and myocardial calcification—technical maneuvers for successful valve replacement. *Int J Cardiol.* 1988;20:279-82.
3. Kumar AS. Massive mitral and myocardial calcification: technical maneuvers for successful valve replacement. *Int J Cardiol.* 2001;78:103-4.
4. Choudhary SK, Talwar S, Dubey B, Chopra A, Saxena A, Kumar AS. Mitral valve repair in a predominantly rheumatic population. *Tex Heart Inst J.* 2001;28:8-15.

<http://dx.doi.org/10.1016/j.jtcvs.2013.07.010>

SURGICAL MANAGEMENT OF CEREBRAL MALPERFUSION WITH ACUTE TYPE A AORTIC DISSECTION

To the Editor:

We read with great interest the article by Abe and colleagues¹ about aortocardiobypass surgery for malperfused left carotid artery complicated by acute aortic dissection. Their patient was a 57-year-old man brought to the hospital with right hemiparesis and loss of consciousness. Computed tomography of the chest showed acute type A aortic dissection, and the left carotid artery was occluded by a thrombosed false lumen. First, they performed the ringed graft anastomosis to the left common carotid artery. Then, this patient underwent ascending aorta replacement under hypothermic circulatory arrest and selective cerebral perfusion. After that, the ringed graft was anastomosed to prosthetic graft. The patient recovered well without any neurologic deficit.

We saw a similar type of acute aortic dissection. A 41-year-old man