pulmonary artery catheters in cardiothoracic donors is well less than 10%.

We of course agree with the well-established efficacy of using individual pulmonary vein gas sampling to sort out unilateral from bilateral lung dysfunction when single-lung transplant recipients are available.

Perhaps it was not as clear from the written protocol as it could be, but we of course also always seek to shorten cardiac ischemic times by prioritizing the immediate transport of the heart to the respective implant center. From a practical standpoint, when both heart and lungs are being procured we routinely have 1 surgeon, upon removal of the heart, immediately package it for transport while the second surgeon administers the in situ retrograde pulmonary vein flush.

We appreciate the concern of the Newcastle group regarding the necessity of “partial inflation” of donor lungs before tracheal stapling. Our routine extubation before stapling is purely pragmatic in its application. It is simply the nature of the beast that the majority of the procurement procedures seem to be carried out in the middle of the night, in unfamiliar territory and with an unfamiliar anesthesia team. We have often found it hard enough just to get the donor extubated, let alone insist on a partial tube withdrawal with partial lung inflation. It is simply more pragmatic to just tell them to extubate the patient. We would also note that if any atelectasis does in fact occur, it occurs after the preservation measures (topical cooling and pulmoplegia infusion) have been carried out. Regardless, the lung fully deflates anyway when the airway is open and being anastomosed at the recipient implant procedure. In our experience, any sequelae resulting from the atelectasis that may have occurred from extubation before stapling has been negligible, if present at all.

The proximity of the esophagus to not only the airway but also the pulmonary veins, combined with the marginal exposure of the deep posterior mediastinum, prompted the development of

a thoracic procurement procedure that lessens the possibility of injury to these vital structures. No doubt that experienced operators, such as those from the Newcastle group, have no difficulty dissecting the lung block away from the esophagus as they describe. However, because the majority of the procurement procedures carried out in the United States over the last 2 decades were performed by inexperienced cardiothoracic fellows—and these too often without a qualified first assistant—we sought to make this part of the procedure safer by simplifying it. By simply dividing the esophagus, the operator is then enabled to dissect the entire lung bloc directly off of the spine—giving a wide berth to the vital airway and pulmonary veins. We do not believe that any significant contamination of the operative field occurs with this method—certainly no more than with the routine division of the trachea after stapling. We believe that the simplification of the procedure by this maneuver renders the highest likelihood of even a single operator under the worst of conditions delivering a perfect lung bloc to the implant team. The esophagus and aorta are of course easily resected from the lung bloc under direct, well-illuminated conditions on the back table in the implant operating room. We have used this technique in more than 1000 lung harvests and cannot recall a single problem of any kind related to the inclusion of the esophagus in the harvested double-lung bloc. We have to believe that we have avoided many injuries to the donor pulmonary veins and airway by less-experienced operators in the often hurried, poorly exposed, and poorly illuminated posterior mediastinal procurement environment.

The inclusion of a procedural checklist is another great suggestion by the Newcastle group and can only improve consistency in the procurement procedure, which was the primary aim of the manuscript in the first place.

Certainly none of the points proposed by the surgeons of Mr Dark’s

superb group pose any major problems in the standardization of the procurement procedure. They only serve to improve and clarify, making the standardized procedure more robust. Their suggestions are greatly appreciated.

Most important of all, the Newcastle group’s confirmation of the advantages of the establishment of a standardized protocol is appreciated. As they point out, the progress of many areas of the world toward the region-based sharing of transplant procurement responsibilities far exceeds that in the United States. The described advantages in efficiency, cost-effectiveness, safety, and patient outcomes provided by the adoption of their “Zonal Retrieval Scheme” should provide ample incentive for the adoption of a standardized procurement protocol in every nation where thoracic organ transplantation is undertaken.

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HOW CAN WE PREVENT FUNCTIONAL MITRAL STENOSIS AFTER SURGERY?

To the Editor:

The recent report by Kubota and colleagues¹ gave new insight on the mechanism of “functional mitral stenosis (MS)” after mitral annuloplasty (MAP) for ischemic mitral regurgitation (IMR). Persistent subvalvular leaflet tethering in the presence of down-sized MAP causes functional MS, which is related to heart failure symptom after surgery. Down-sized MAP has been a standard procedure for IMR and believed to be safe and effective for 10 years. In 2008, Magne and colleagues² first reported functional MS after MAP. However, its mechanism was not clarified in this study. On the other hand, Kubota and colleagues examined subvalvular

apparatus during exertion and indicated that not only MAP but also further mitral tethering causes functional MS. This study has a considerable impact on the surgical strategy for IMR in the future.

In 2003, we developed mitral complex reconstruction, which consists of MAP, papillary muscle (PM) approximation,³ and (since 2005) suspension (PMS),⁴ and have aggressively adopted this procedure for patients with heart failure and functional mitral regurgitation. In PMS, we place a subvalvular expanded polytetrafluoroethylene (ePTFE) suture between the site of the chordal attachment of the approximated PM and the mitral annulus. We considered that posterior PMS, in which ePTFE suture is sewn to the middle of the posterior annuloplasty ring, causes further anterior mitral valve tethering in diastole. Therefore, we recently changed from posterior to anterior PMS, in which ePTFE suture is sewn to the middle of the anterior annuloplasty ring. Table 1 shows the postoperative transmitral gradients and mitral complex parameters (n = 8, for each group). The left ventricular (LV) inflow line was measured using Doppler color flow mapping at the time of maximal early diastolic rapid filling by an apical long-axis view. The angle between this line and the annulus was measured as the LV inflow angle. PM angles were also measured with the line between the tip of the PM and the septal side (anterior) or posterior side (posterior) of mitral annulus in end diastole. LV inflow angle was significantly larger and posterior PM angle was significantly smaller in the anterior PMS group. Mitral peak pressure gradient was significantly smaller in the anterior PMS group (5.8 ± 1.9 mm Hg) than in the posterior PMS group (10.0 ± 2.6 mm Hg), which is comparative to that of Kubota and colleagues' report¹ (10.6 ± 6.2 mm Hg).

Although our parameters are different from those of Kubota and colleagues' report,¹ LV inflow and PM angles would correspond well to the

TABLE 1. Postoperative transmitral gradient and mitral complex parameters

	Posterior PMS	Anterior PMS	P value
E (m/s)	1.6 ± 0.2	1.2 ± 0.2	.004
Peak pressure gradient (mm Hg)	10.0 ± 2.6	5.8 ± 1.9	.004
Papillary muscle angle (anterior)	47 ± 10	55 ± 8	.131
Papillary muscle angle (posterior)	106 ± 10	93 ± 12	.042
Left ventricular inflow angle	60 ± 6	78 ± 9	<.001
Stroke volume index (mL/m ²)	28 ± 5	37 ± 11	.054

PMS, Papillary muscle suspension to annuloplasty ring. Values $\pm$ standard deviation.

parameters determining mitral valve tethering in their report. We consider that mitral valve tethering during diastole could be ameliorated by anterior PMS, which can relocate the PM toward the septal side of the ventricle but not the posterior side. Langer and colleagues⁵ recently reported a better mitral regurgitation recurrence rate with a subvalvular procedure termed "RING + STRING." We should further examine the relationship between these subvalvular procedures and functional MS after operation.

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Reply to the Editor:

We appreciate the comments by Drs Shingu and Matsui. Our investigation showed that functional mitral stenosis (MS) after surgical annuloplasty for ischemic mitral regurgitation (MR) is not rare.^{1,2} Mechanistically, diastolic leaflet tethering to restrict its opening in the presence of surgical reduction of annular size causes this functional MS.² The degree of functional MS is proportional to the degree of recurrent MR after the surgery, and both are closely related to subvalvular tethering. Therefore, functional MS and recurrent MR can be interpreted as a single pathophysiology, presenting in diastole and systole as a different form. Procedures to reduce subvalvular tethering seem reasonable to address functional MS and recurrent MR.

In this context, the data presented by Drs Shingu and Matsui are interesting. They compared 2 surgical techniques to address subvalvular tethering by shortening the distance between both sides of the papillary muscle (PM) tips and mitral annulus: technique number 1 to displace the PM tips toward the posterior mitral annulus and technique number 2 to shift the PM tips toward the anterior annulus. The degree of functional MS is significantly less with PM displacement procedure toward the anterior annulus. The favorable results by PM displacement toward the anterior mitral annulus seem reasonable, because the procedure is likely to reduce tethering in both systole and diastole, whereas PM displacement toward the posterior mitral annulus may impair the systolic closure of posterior leaflet and diastolic opening of the anterior leaflet.