Figure 1. The Robicsek–Thubrikar “tailor-made” sinus graft.

However, even if someone may be able to accomplish an anatomic match between sinuses and leaflets, normal compliance of the aortic wall, a prerequisite to significant stress reduction, will remain elusive. Our experience indicates that positioning the “grooves” of the Dacron prosthesis at the level of the neosinuses vertically will indeed provide some compliance, but it will not reach that of a normal aortic root. Also, with the passage of time even this disappears. It is indeed important to try to imitate Mother Nature in our efforts to restore and maintain aortic valve function by creating neosinuses. We agree with the authors that the onion configuration itself provides very little stress reduction and that the presence of 3 neosinuses may reduce the stress on the aortic leaflets; however, it also needs to be emphasized that this will occur only if the sinuses are appropriately matched with the leaflets, and unless long-lasting compliance is provided, the stress reduction will be temporary and miniscule.

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Reply to the Editor:

We thank Dr Robicsek for his comment on our article “Six Stitches to Create a Neosinus in David-type Aortic Root Resuspension.” He has extensive experience in this field. We agree that a rigid tube will never achieve the same compliance as the native aortic root, and that this increases the stresses on the leaflets. However, the question is still to be answered if this has clinical impact, especially whether the higher leaflet stresses have more impact on long-term durability than an optimal geometric reconstruction. Because the aortic sinuses and leaflets dilate, stretch, and reshape to an individual configuration in many patients, each leaflet and sinus has to be recreated to achieve an optimal coaptation. Clinical reports do not show perfect or sub-optimal long-term results for the remodeling operation.¹ In David and colleagues’ personal comparison,¹ reimplantation had significantly better long-term results than remodeling.² In our opinion, this is because it is easy to overcorrect the aortic valve in the David procedure, avoiding any sagging and stabilizing the slightly dilated annulus. We fully agree that the onion shape is a suboptimal solution because it does not support the naturally straight course of the commissures. This is optimized by the technique described in our article.

It still has to be proven that the optimal leaflet stress reduction is achieved in the manner suggested by Dr Robicsek, that is, the sinotubular junction is kept rigid by the Dacron tube and the elasticity of the remaining aortic root is preserved. The latter will be reduced by the scarring process in the long term.

Individual matching between sinuses and leaflet is easily accomplished with our technique because the reduction at the sinotubular junction is only done with the valve already inside the tube and thus can be easily positioned at the optimal spot.

Because the total load on the leaflets is determined by the product of area, time’s pressure overcorrection reduces the mechanical stress on the usually enlarged leaflets. There is currently no proof that optimizing root remodeling with respect to compliance and thus possibly reduced leaflet stresses achieves better long-term results than does an optimal straightforward geometric repair with slight overcorrection.

We thank Dr Robicsek for his important thoughts.

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If older donors are acceptable, then older recipients should also be acceptable

To the Editor:

I read with interest the article by De Perrot and colleagues,¹ in the February 2007 issue of the *Journal*.

The article examines the outcome of lung transplantation using lungs obtained from donors aged 60 years or more. The average age of donors was 65 years; the average age of recipients was 49 years.

Considering that the life expectancy of a 60-year-old, just as that of a 50-year-old, exceeds the “predicted life” of the transplanted lung, if elderly people are good enough to be donors, why aren’t they good enough to be recipients? Are we facing age discrimination?

I am most interested in the author’s reply.

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doi:10.1016/j.jtcvs.2007.04.056

Reply to the Editor:

We thank Dr Robicsek for his interest in our article.

The average age of our overall donor population was 38 years in this series; 65 years is the median age for the group of donors aged 60 years and older.

We agree that the predicted life expectancy of a 60-year-old can be excellent. In fact, we do not have any age limitations for the selection of recipients in our program. At the time of this publication, the average age of our recipients was 45 years. However, 78 recipients were 60 years and older, and the oldest recipient was 74 years old at the time of the transplant in our experience.

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Independent lung ventilation in adult single-lung transplantation: Is it time for fast-track anesthesia and early tracheal extubation?

To the Editor:

I read with great interest the recent article by Pilcher and colleagues¹ detailing their experience with independent lung ventilation (ILV) in adult single-lung transplantation for chronic obstructive pulmonary disease (N = 170; 1990–2005). The incidence of ILV was 12% (20/170); the 2 multivariate predictors were preoperative percentage predicted total lung capacity (odds ratio 1.04; confidence intervals 1.01–1.07; $P = .032$) and postoperative reduction in the arterial oxygen tension/inspired oxygen fraction ($\text{PaO}_2/\text{FiO}_2$) ratio (odds ratio 0.97; confidence intervals 0.95–0.99; $P = .005$). The mortality rate in the ILV subcohort was 40% (8/20) as compared with 4.7% (7/150) for the rest of the cohort ($P < .001$).

Which interventions might minimize the serious complication of ILV? What about mechanical ventilation in the pathogenesis of

early graft dysfunction, given the inherent compliance mismatch? High preoperative total lung capacity would aggravate the compliance mismatch and consequent donor lung stretch and injury. Postoperative reduction in the $\text{PaO}_2/\text{FiO}_2$ ratio reflects lung edema and injury. Is there evidence for a link between mechanical ventilation and graft function in this clinical scenario?

The duration of perioperative mechanical ventilation in single-lung transplant recipients contributes to early graft edema and dysfunction.² Early tracheal extubation is feasible in single-lung transplantation: a successful extubation protocol includes formal criteria, short-acting anesthetics, thoracic epidural analgesia, and intermittent application of noninvasive positive-pressure ventilation as required.^{2,3}

As a result, it would be most interesting to see what effect this consideration would have on the data sets in the study by Pilcher and colleagues. Was there an evolution in anesthetic technique over the 15 years of the study? Was the incidence of ILV influenced by anesthetic technique and timing of tracheal extubation? Perhaps the answers to these questions could be the focus for a subsequent study.

I congratulate Pilcher and colleagues on a thought-provoking study. Their research, taken together with earlier studies, suggests the following future directions for clinical outcome studies in this field: (1) validation of the safety and success of early tracheal extubation in single-lung transplantation and (2) determination of the effects of fast-track anesthesia with early tracheal extubation on graft function in the setting of pulmonary compliance mismatch.

If the effects of fast-track anesthesia with early tracheal extubation are favorable, it will progress to become the standard of care for single-lung transplant recipients, especially in the setting of compliance mismatch and chronic obstructive pulmonary disease.

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doi:10.1016/j.jtcvs.2007.04.058

Reply to the Editor:

We thank Augoustides for his comments and thoughts about our article.¹ He notes the high mortality rate of those undertaking independent lung ventilation (ILV) and suggests that trials assessing early extubation with appropriate anesthetic modification may be a solution. Clearly in this retrospective case series we cannot assess causation; that is, we cannot say whether the use of ILV was actually harmful, nor whether in balance it increased mortality. We would contend that when ILV was initiated, patients had reached a point at which they could not be sustained without ILV (or in later years extracorporeal membrane oxygenation).

Mechanical ventilation is only justified when its benefits outweigh its risks. Inappropriately high tidal volumes are known to be harmful to lungs with adult respiratory distress syndrome, with an adverse effect on mortality.² This is also likely to be true for lung transplants. It has been our clinical experience that increased mechanical ventilation in the single-lung transplant with excessive dynamic hyperinflation and primary graft failure leads to increasing dynamic hyperinflation within the native lung and subsequent cardiovascular collapse. However, it is possible that there is also direct injury to the transplanted lung from inappropriate mechanical ventilation.

Delineating this can be difficult because, owing to the contribution of the native lung, a healthy patient with a functioning single graft has a lower arterial oxygen tension/inspired oxygen fraction ($\text{PaO}_2/\text{FiO}_2$) ratio than that of a "similar patient" with a double-lung transplant. At extubation, single-lung transplants have lower $\text{PaO}_2/\text{FiO}_2$ ratios than double-lung transplants.³ The intermediate outcomes in these 2 groups are similar. It has been suggested that the definition of primary graft failure should be changed to reflect this.⁴

Since the program began at the Alfred Hospital, there have been many advances in all aspects of lung transplantation. An-