guarantee against secondary disruption or leakage. Except for the discomfort experienced by the patients, the cumulative risk for the multiple-stage approach can reasonably be presumed to be higher than that reported by Dr Kouchoukos et al and ourselves for the single-stage approach. For these reasons I strongly recommend Dr Kouchoukos and colleagues' suggestions to trust the single-stage techniques in selected cases whenever indicated.

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Reply to the Editor:

My coauthors and I are indebted to Dr Minale and colleagues for reporting their initial experience with the single-stage technique for extensive thoracic aortic resections in 1994,¹ which stimulated us to use and evaluate it in our surgical practice. We also appreciate the update of their experience with this technique, which indicates outcomes very similar to those that we have reported.² We prefer, however, to cool to systemic temperatures lower than 25°C to provide additional protection in the event that the ischemic intervals for the brain and spinal cord are prolonged beyond the usual safe intervals. We do not believe that the lower temperatures are associated with increased morbidity. We have found that an incision through the fourth intercostal space bilaterally provides excellent exposure of the aortic root, entire aortic arch, interatrial groove, and most distal portion of the descending thoracic aorta.

We agree with Dr Minale that the single-stage technique is of particular value in patients who require reoperation after previous repair of an acute type A aortic dissection or who have a type A dissection after previous coronary artery or valve sur-

gery. Chronic type A dissection was the indication for use of the single-stage procedure in 30 of the 46 patients in our series, and the procedure remains our technique of choice in this setting.² It permits, as Dr Minale has emphasized, easy access to both the aortic root and the distal descending thoracic aorta.

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Total aortic arch replacement and limited circulatory arrest of the brain To the Editor:

We read with great interest the article by Kouchoukos and Masetti¹ titled "Total Aortic Arch Replacement With a Branched Graft and Limited Circulatory Arrest of the Brain," published in the August 2004 issue of the *Journal*. The article describes the authors' experience with total arch replacement (TAR) for various pathologic conditions of the aortic arch (except acute type A dissection) with branched aortic prostheses and unilateral antegrade cerebral perfusion through the right axillary artery. The article generally attests to the appropriateness of the use of branched aortic prosthesis and antegrade brain perfusion for TAR surgery. Since 1986, we have been routinely using antegrade selective cerebral perfusion and a branched aortic graft for TAR procedures.² The results we have achieved through the years give us reason to be convinced that the use of the branched grafts rather than tube prostheses and antegrade brain perfusion for cerebral protection represent the optimum surgical strategy for this complex surgery.³ The branched aortic graft allows easier bleeding control at arch-vessel anastomotic sites and has been found to be associated with a lower incidence of postoperative strokes, making it a better option than tube graft. Similarly, selective cerebral perfusion, when used to its

full potential, basically relieves the surgeon from the psychologic burden of a limited brain protection time and thus allows meticulous arch repair. It is good to see that more and more aortic surgeons all over the world are realizing the advantages of these techniques, which once were thought to be cumbersome.

Concerns have been expressed by some authorities, including Kouchoukos and Masetti,¹ regarding the potential hazards of direct cannulation of the arch vessels in the form of cerebral embolization of air or debris. In our experience, such concerns are mostly unwarranted. Our arch vessel cannulation technique involves transecting the arch vessels distant from their origins, at sites where they are free from atherosclerotic debris or dissection, and then cannulating them under direct vision. Blood flow through the arch vessels is continued until just before the cannulation with the patient in a Trendelenburg position. These measures help to avoid air embolism.⁴

For patients with acute type A aortic dissection and degenerative arch aneurysm requiring TAR in whom the ascending aorta is not suitable for arterial inflow because of atheromas, we first cannulate the right axillary artery through an 8-mm graft attached to it in an end-to-side fashion. Then, after cooling of the patient with cardiopulmonary bypass and initiation of circulatory arrest, the left common carotid artery is also cannulated, ensuring bihemispheric antegrade brain perfusion during the arch repair. Although some authors have advocated the use of unilateral cerebral perfusion for aortic arch replacement surgery,⁵ our experience with that technique is limited to cases requiring a less radical aortic repair, such as ascending aortic replacement with open distal anastomosis or hemiarch replacement. In 10 patients (acute type A dissection n = 7, chronic type A dissection n = 1, degenerative arch aneurysm n = 1, and mycotic arch aneurysm n = 1), hemiarch replacement was carried out with right axillary artery perfusion as the only brain protection method under moderate hypothermic circulatory arrest (mean rectal and tympanic temperatures 22.3°C ± 1.6°C and 16.9°C ± 2.4°C, respectively). Mean perfusion flow rate and pressure were 5.1 ± 1.9 mL/(kg · min) and 27.8 ± 9.4 mm Hg, respectively. Mean selective cerebral perfusion time was 35.8

± 14.1 minutes. There was 1 postoperative death from an unrelated cause, and no temporary or permanent neurologic deficits were seen. Although this represents a satisfactory outcome, routine adoption of unilateral brain perfusion for TAR, which usually requires a longer brain protection, may not always be equally successful. If it has to be done, however, a more substantial lowering of core body temperature might be helpful.

The arch-first strategy described by Kouchoukos and Masetti¹ may not always be advantageous. This is because it makes the distal aortic anastomosis more difficult, with the bulky aortic prosthesis coming to obscure much of the operative field. Moreover, deep hypothermia still needs to be continued during the distal anastomosis. As Kouchoukos and Masetti¹ say, a bilateral anterior thoracotomy will facilitate the distal anastomosis in these situations.

Finally, we think that despite the small number of patients studied, Kouchoukos and Masetti¹ deserve credit for the results they obtained with unilateral brain perfusion for TAR. However, preoperative assessment of the cerebral circulation as well as careful selection of cases should always be considered before deciding on a brain protection strategy.

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Bioengineered airway tissue

To the Editor:

In the brief communication "First Human Transplantation of a Bioengineered Airway Tissue," Macchiarini and associates¹ point out that tracheal replacement by tissue engineering seems to hold potential. Question is to be raised whether this report actually does provide "definitive evidence that a tissue engineered patch... can functionally... fill all requirements for an airway patch."

The patch was applied to a defect 1.5×1.5 cm after breakdown of anastomosis after carinal pneumonectomy. Omentum was applied over the patch; this was further buttressed, and the space was filled with a subscapular muscle flap. To further obliterate the space, complementary thoracoplasty was performed. The leak healed, and ciliated respiratory epithelium was eventually found to cover the repaired defect.

Closure of a defect such as this with an omental flap, especially with further use of a muscle flap plus thoracoplasty to obliterate residual space, would in most cases suffice to seal the defect successfully. Epithelium undoubtedly migrates from the respiratory epithelium of the surrounding trachea and bronchus to cover the scar that forms over the mesenchymally repaired defect. Epithelization occurs over vascularized autogenous flaps used over smaller defects in trachea and bronchi. Whether the graft actually survived or the tissue ultimately seen was partly or entirely scar that would form over the mesenchymal bed of omentum is not demonstrated.

One must also question placement of a free graft of any tissue over an area that is still contaminated, even if not grossly infected, by the bacteria that necessarily are present in such a situation, despite all cleanup treatment before repair. More to the point, however, is the fact that defects of this sort have long been closed by vascularized pedicled autogenous tissues (omentum, pericardium, intercostal muscle, and other muscle flaps). Addition of an engineered tissue graft seems superfluous.

The potential of bioengineering to produce tracheal replacement segments in the

future is a goal worthy of continued research attention nonetheless.

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Reply to the Editor:

It is a distinct privilege to have attracted the interest of Hermes Grillo, and frankly speaking, his comments were expected. In his letter to the Editor, Dr Grillo questions clinical and experimental issues that I am delighted to address.

It is certainly correct that the airway defect could have been closed without the interposition of the engineered airway tissue. However, we all know that (1) it is technically demanding and risky to free (by further devascularizing it) a leaky post-radiation (70 Gy) carinal anastomosis, and that (2) an intrathoracic transposed omentum buttressed over an airway defect is almost never immediately tight, and this jeopardizes the spillage of omentum produced fluid into the unique lung and induces a motion- (and foreign body-) temporary cough.

However, and with great respect, we disagree that the addition of an engineered airway tissue was superfluous. From a clinical viewpoint, a repeatedly thoracotomized and previously irradiated (70 Gy) operative field is unlikely to guarantee engrafting (eg, intercostals muscle) or allow harvesting (pericardium) of autogenous tissues. Hence, the closure of the defect with the tissue-engineered graft ensured a tight interface permitting a physiologic secretion transport without having all the complications related to the indirect closure through autogenous tissues. Having experienced the scenario with and without¹ this new tool, I must admit my pleasure in having an extremely powerful and valid clinical alternative.

From an experimental viewpoint, our disagreement is even more important because the lessons learned from this clinical application are very important. Because of the concise nature of the chosen article