The end question is, independently of the harvesting technique, are there segments of “biologically privileged” vein that randomly will have a good outcome in the long term?

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IS SLIDE TRACHEOPLASTY ALONE ENOUGH TO IMPROVE THE SURGICAL OUTCOME?

To the Editor:

We read with interest the report by Manning and colleagues¹ on the series of 80 patients with congenital tracheal stenosis undergoing slide tracheoplasty (STP).

They conclude that STP, with the use of cardiopulmonary bypass, is a safe and reliable technique associated with low morbidity and mortality. The title itself underlines the fact that this technique is also pliable because it could also be used with extreme anatomic variants.² As reported by Manning and colleagues,¹ the age range of patients undergoing STP can vary from a few days of life to several years, mainly

because tracheal stenosis can occur alone or associated with other anomalies (eg, cardiac, extracardiac, chromosomal) that could precipitate the symptomatology and anticipate the clinical presentation. When the tracheal stenosis is isolated, there are different anatomic variants, such as abnormal bronchial arborization or functional single lungs, that can complicate the preoperative management by requiring mechanical ventilation (27.5%,¹ up to 66%³) and sometimes preoperative extracorporeal membrane oxygenation to sustain the hemodynamics.

The use of cardiopulmonary bypass also is routinely used in our center in patients with isolated tracheal stenosis in whom the diameter of the trachea can be as narrow as 1 or 2 mm and conventional endotracheal tubes cannot fit.

The pliability of STP allows the surgeon to extend the incision into one of the bronchi when the stenosis involves segments distal to the carina.² However, in the series reported by Manning and colleagues,¹ the involvement of the bronchi in the STP has not been reported. We believe that when a stenosis involves the bronchi, this must be repaired along with the trachea.

STP has currently proved its reliability with a better surgical outcome and lower complication rate when compared with the other techniques.

We disagree with Manning and colleagues’¹ statement that the STP itself is “largely responsible” for their excellent results.

As reported by our institution, the management of these complex patients is multilevel, and intensivists, surgeons, interventional radiologists, pneumonologists, and cardiologists are equally involved. For this reason, we introduced multidisciplinary management with a significant improvement of outcome.⁴

As Manning and colleagues¹ report, 42.5% of the patients (34/80) required at least 1 additional airway intervention, including tracheal dilatation,

stenting, or reoperation. This indicates that STP alone does not always work, because there is still the need for further interventions to achieve an acceptable result.

We believe the management of patients throughout the follow-up period is a key point that must not be forgotten. A better understanding of the mid- and long-term effects of tracheal surgery, such as the function of the airway mucosa or the lung, and above all on the quality of life, will be necessary to achieve a thorough understanding of this very select population.

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LATE MORTALITY IN OFF-PUMP VERSUS ON-PUMP CORONARY ARTERY BYPASS GRAFTING: A META-ANALYSIS OF PROPENSITY SCORE-ADJUSTED STUDIES

To the Editor:

We read with great interest the systematic review and meta-analysis by Kuss and associates¹ of propensity score analyses, which found on-pump coronary artery bypass grafting (CABG) superior to on-pump CABG

in short-term mortality. A previous meta-analysis² of risk-adjusted observational studies also showed that off-pump CABG was associated with reduced early mortality. In terms of early mortality, however, meta-analyses^{2,3} of randomized controlled trials showed no significant difference. In regard to late mortality, previous meta-analyses²⁻⁴ of randomized trials demonstrated that mortality was not reduced in off-pump CABG. Our newest meta-analysis⁵ of randomized trials, however, suggested that off-pump CABG might increase late mortality over on-pump CABG. Meanwhile, a meta-analysis² of only 2 risk-adjusted observational studies showed essentially no change in late mortality. In both early and late mortality, there has been obvious discrepancy between evidence from meta-analyses of randomized trials and evidence from observational studies. Moreover, the evidence of late mortality from the meta-analysis² of observational studies is insufficient. To add evidence from observational studies that represent real-world clinical practice, we performed a

meta-analysis of late (≥ 1 -year) mortality adjusted for propensity score in observational studies of off-pump versus on-pump CABG.

Our comprehensive search (current through February 2010) identified 18 propensity score-adjusted observational studies of off-pump versus on-pump CABG, which included data of late (from 1- to 10-year) mortality on 28,405 patients assigned to off-pump ($n = 13,469$) or on-pump ($n = 14,936$) CABG. Pooled analysis demonstrated no significant difference in late mortality between off-pump and on-pump CABG in random-effects models (risk estimate, 0.91; 95% confidence interval, 0.78–1.06; $P = .21$) (Figure 1). There was significant study heterogeneity ($P = .04$) and little difference in the pooled result from fixed-effects modeling (risk estimate, 0.97; 95% confidence interval, 0.89–1.06; $P = .52$). Exclusion of any single study from the analysis did not substantively alter the overall result of our analysis. There was no statistically significant linear relationship between the log risk estimate and

the duration of follow-up in meta-regression ($P = .59$ by an unrestricted maximum likelihood model) or evidence of significant publication bias ($P = .32$ by an adjusted rank-correlation test).

The results of our analysis suggest that off-pump CABG may not reduce late (≥ 1 -year) mortality over on-pump CABG. Why is there a discrepancy in evidence of late mortality between the meta-analysis⁵ of randomized controlled trials (superiority of on-pump CABG) and the present meta-analysis of propensity score-adjusted observational studies? Patients enrolled in randomized trials, commonly with low-to-moderate risk, may not be representative of patients typically seen in real-world clinical practice. Possible superiority of off-pump CABG for high-risk patients included in not randomized trials but observational studies may contribute toward equivalent late mortality found in the present meta-analysis of observational studies. To confirm whether off-pump CABG is superior to on-pump CABG for high-risk patients in late mortality,

FIGURE 1. Forrest plot of late mortality among patients assigned to off-pump versus on-pump CABG. CI, Confidence Interval; IV, inverse variance; ATS, *Ann Thorac Surg*; Circ, *Circulation*; CJ, *Circ J*; CJC, *Can J Cardiol*; CMJ, *Chin Med J*; EJCTS, *Eur J Cardiothorac Surg*; JCVS, *J Cardiovasc Surg*; JTCVS, *J Thorac Cardiovasc Surg*; TCVS, *Thorac Cardiovasc Surg*.

randomized trials in high-risk patients are needed.

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NO MATTER HOW COMPLEX THE PROBLEM, THERE IS ALWAYS A SIMPLE SOLUTION—WHICH IS WRONG!

To the Editor:

Is it the cough that carries them off?

Asbestos-related pulmonary disease, and specifically the development of mesothelioma, continues to be a tragic health issue. There are characteristics of the whole disease entity that seem both unusual but interrelated. The inhalation of relatively long asbestos fibers is followed by their transport to the visceral pleura and subsequently to the parietal pleura and, via lymphatics, elsewhere. The fibers produce subpleural fibrosis in the lungs, but this discussion relates to the more

significant parietal pleural changes. Usually, the first and most diagnostic appearance is that of pleural plaques, with or without calcification, at chest radiograph or computed tomographic scan. There are several features of these that warrant consideration.

1. They involve the parietal pleura and not the visceral.
2. They arise in areas of greatest movement between the opposing pleural layers, with respiration, coughing, and sneezing. They do not occur over the lung apices or within the interlobar fissures where the pleural surfaces move in unison.
3. The plaques and calcifications tend to group deep to the ribs. And yes, some also form over the diaphragm and pericardium.

A simple question then arises—Is the development of pleural plaques and subsequent mesothelioma a result of chronic abrasive trauma caused by these fibers at the visceral surface abraiding the parietal pleura, akin to oral tumors resulting from “ill fitting dentures?” This would then infer a mechanical etiology rather than a chemical carcinogenesis and perhaps explain why other fibers with similar physical characteristics have also been implicated or are under suspicion. Of interest, talc had been viewed with suspicion by some because of its fiber structure, but the fact that it produces pleurodesis would negate any such effect.

If this hypothesis is worthy of consideration, several other points arise.

The negative pressure within the pleural cavity during quiet respiration, maintaining the apposition of the visceral and parietal pleural layers, is very low, and this would by inference reduce abrasive effects. However, during coughing the glottis closes while the intercostal muscles and abdominal wall muscles contract, increasing the intrathoracic and intrapleural pressure before there is an explosive release of air from the chest. The markedly increased direct pressure exerted by the ribs on the underlying pleural space may explain the

apparent predilection of pleural plaques for these regions.

If these suppositions hold weight, then an obvious question arises. Is there a place for prophylactic pleurodesis in the prevention of mesothelioma by preventing chronic abrasion between the pleural surfaces?

The risk of development of mesothelioma in those with asbestos-related disease has been quoted at 10%, a figure presumably subject to much discussion. It may be arguable whether even a procedure of low morbidity should be universally proposed as a panacea. However, other factors, possibly including plaque load, life expectancy, smoking, and chronic cough, may enable identification of higher risk groups for whom active intervention may be justified.

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ALICE IN WONDERLAND OF MEDIASTINAL LYMPH NODES To the Editor:

We read with great interest the results of the American College of Surgery Oncology Group (ACOSOG) Z0030 trial,¹ which was a randomized, multi-institutional, prospective trial of mediastinal lymph node dissection (MLND) versus sampling (MLNS) during pulmonary resection for patients with early-stage (N0 or nonhilar N1, T1, or T2) non-small cell lung cancer (NSCLC). At a median follow-up of 6.5 years, 44% died in the MLNS arm and 42% died in the MLND arm, and the median survival was 8.1 years in the MLNS group and 8.5 years in the MLND group ($P = .25$). Studies addressing the survival benefit of MLND, however, have been inconclusive. We herein performed a meta-analysis of randomized controlled trials of MLND versus MLNS during pulmonary resection for prevention of all-cause mortality in early-stage NSCLC.