monary bypass and serious complications, particularly when the nadir hematocrit decreased to less than 22%.

In these 3 studies, the age, weight, seriousness of the disease, previous cerebrovascular accident, duration of circulatory arrest, and percentage of emergency surgery were completely different. Therefore, it might be very difficult to compare the results and determine an appropriate hematocrit level during cardiopulmonary bypass, especially for surgical cases using deep hypothermic circulatory arrest. Regarding the backgrounds of the patients, our study group was considered to be the best candidates for hemodilution during cardiopulmonary bypass. Transfusion has been proven to be an effective therapy for hemodilutional anemia. However, this therapy may cause severe adverse effects. Although there were no adverse effects in our study group, we agree with Dr Jonas that a prospective randomized study should be performed not only in adults, but also in children with simple congenital heart defects to determine the appropriate hematocrit level that improves a patient's chance for the most favorable outcome.

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Extrapleural pneumonectomy for malignant mesothelioma: Should pericardium be resected routinely?

To the Editor:

We read with interest the article by Sugarbaker and colleagues¹ in the July issue of the *Journal*. This study reports their extensive experience with the management of complications occurring after extrapleural pneumonectomy (EPP) done mainly for malignant mesothelioma. It is clear from the Sugarbaker article that technical complications might arise from the resection and replacement of the pericardium with a patch, such as size mismatch, improper insertion and suturing, or inadequate fenestration. Three percent of their patients had cardiac arrest, 2.7% had constrictive physiology caused by inflammatory epicarditis, 3.6% had tamponade, and 44.2% had atrial fibrillation.

Although we recognize the importance of Sugarbaker's work in studying specific complications of EPP and reporting the largest series to date, we would like to emphasize a particular technical point that might prove useful in some patients. Recently, we have been surprised that some patients with stage I or II malignant mesothelioma receiving induction chemotherapy before EPP presented at the operation with a pericardium macroscopically free of disease. In those patients we have taken random biopsy specimens of the pericardium and have preserved them. Histopathology confirmed the absence of pericardial invasion in all patients. In such patients with a macroscopically normal pericardium, we suggest taking frozen sections of the pericardium and preserving them if the frozen sections are negative. Although we do agree that pericardium has to be resected in most patients with malignant pleural mesothelioma, it is likely that with the adoption of induction chemotherapy before EPP² and refinement of diagnostic techniques, such as positron emission tomography and computed tomography, more patients will be operated on with the possibility of preserving their native pericardium, thereby reducing the incidence of cardiac complications, reducing the operative time, and facilitating the anchorage of the diaphragmatic patch.

We would also like to mention that chylothorax is another potential complication of EPP, with a recent article reporting an 18.7% incidence.² Although no postoperative chylothorax has been reported in the series by Sugarbaker et al,¹ we would like

to re-emphasize the importance of prophylactic thoracic duct ligation during right-sided procedures.

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Reply to the Editor:

The primary goal of extrapleural pneumonectomy in mesothelioma is to perform a complete macroscopic cytoreduction. Routine pericardiectomy is an important step in achieving this goal. The issue of pericardial resection and reconstruction from the morbidity standpoint is a higher atrial fibrillation rate, the risk of cardiac herniation from a poorly constructed patch, tamponade from a tight patch, and epicarditis.

Our high rate of atrial fibrillation compared with that seen in other reports that use similar resection of the pericardium stems from our policy of monitoring all patients with telemetry until discharge from the hospital. Opening the pericardium is only one of the multiple causes of atrial fibrillation. Entry into the pericardium with multiple random biopsies would also result in a higher atrial fibrillation rate or at least not a significant decrease in those rates.

Cardiac herniation or tamponade from a poorly done pericardial reconstruction would be avoided by not resecting the pericardium. These are not common events, however, and were more prevalent during our earlier experience. However, from an oncologic perspective, it would not be prudent to risk a good cytoreduction, even in the setting of a few negative random biopsy specimens. It is this group of patients with early disease that would benefit the most from the most aggressive surgical intervention.

The operative time it takes to reconstruct the pericardium is not significant, and the diaphragmatic reconstruction is not facilitated in our experience by leaving the pericardium in.

Finally, we agree that personal experience with chylothorax should guide each surgeon as to whether prophylactic thoracic duct ligation should be used routinely.

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Technique for microsphere injection To the Editor:

We read with great interest the article by Hedayati and associates¹ recently published in the *Journal*. The authors suggested that axillary artery cannulation for cardiopulmonary bypass is cerebroprotective from aortic atheroemboli by using canine models. We respectfully would like to note some procedural inconsistencies that might have lead to bias in the authors' conclusions.

The authors assessed the distribution of microspheres simulating aortic atheroemboli shed by patients during cardiopulmonary bypass. However, the microsphere injection method was inconsistent (eg, the pump flow was not constant, and the rate of microsphere injection was unclear), and the reference blood withdrawal rate for calculating tissue blood flow was unclear. Because the distribution pattern of the microspheres can be influenced easily by the pump flow rate, microsphere injection rate, shape of the aorta, and location of the injection, more precise experimental protocols might affect this study's results.

One more point of concern in this study is the size of the microspheres. Small microspheres (15 μm in diameter) have been used for measuring tissue blood flow,² and large microspheres ($>50 \mu\text{m}$ in diameter) have been used for creating microemboli³ by several investigators. Hedayati and associates¹ used microspheres 15 μm in diameter; however, they did not use the microspheres to measure tissue blood flow but rather as microemboli in this study. Because atheroemboli are generally larger than 15 μm in diameter and because size affects the degree of cerebral ischemia and infarction,⁴ we believe that the larger mi-

crophere should have been used to analyze the risk of atheroemboli in the aorta during cardiopulmonary bypass.

In light of these considerations, the authors' conclusion that "axillary artery cannulation for cardiopulmonary bypass is cerebroprotective" cannot be fully supported on the basis of the data presented. More studies with larger microspheres or flow characterization with both particle image velocimetry and laser Doppler velocimetry could provide more meaningful results and insights on this subject to further understand this clinically important topic.

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doi:10.1016/j.jtcvs.2004.11.043

Reply to the Editor:

Thank you for your interest in our publication entitled "Axillary artery cannulation for cardiopulmonary bypass reduces cerebral microemboli." We are confident that our conclusion, that axillary cannulation reduces cerebral microemboli, is supported by our experimental evidence. We apologize for any lack of clarity with our experimental design. We agree that the distribution pattern of the microspheres might be influenced by the pump flow rate, microsphere injection rate, shape of the aorta, location of the injection, and location of the cardiopulmonary bypass cannulation site. Using each animal as its own control enabled us to provide constant conditions for

each variable, with the only difference being the cannulation site.

Our microsphere injection method was consistent for each animal. The only variable was the cannulation site for cardiopulmonary bypass. The pump flow rate was constant during the 2 cannulation techniques within each animal, although the pump rate did vary from 1.9 to $3.4 \text{ L} \cdot \text{min}^{-1} \cdot \text{m}^{-2}$ among animals to maintain constant pressure among animals. Five million microspheres (2.5×10^6 microspheres/mL) were injected over 1 minute.

Larger microspheres have been used to induce infarction or ischemia and might have been preferred if that was our intention. We were exclusively interested in the distribution of the spheres and found no evidence that the distribution of 50- μm spheres would have, in our model, altered our impressive results. Atheroemboli range in size, with the most numerous being the smallest.¹ We used 15- μm spheres, as has been done before in similar investigations.²

We agree with your suggestion that further characterization of the flow pattern through particle-imaging techniques will provide additional evidence as to the superior ability of axillary cannulation to provide cerebroprotection, and we plan to do so in the future to further corroborate the vastly reduced stroke rate we have observed when axillary perfusion was used in our patients with high-risk aortas.

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Carbodisection of the internal thoracic artery

To the Editor:

I write with reference to the article by Özkan and associates titled "A Carbon Dioxide Insufflation Technique for Prepara-