operation day or the next day depending on the protein content, regardless of the amount of daily drainage. This prospective and randomized study is currently in progress in our department. The preliminary data suggest that although the protein content and amount of drain fluid eventually correlate, the former decreases more rapidly than the latter. In this regard, we believe that surgeons can remove drains more aggressively even with daily drainage more than 450 mL.

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Reply to the Editor:

We appreciate Ozdemir and colleagues' letter concerning our article "Results of a Prospective Algorithm to Remove Chest Tubes After Pulmonary Resection with High Output." We are aware that some groups have evaluated the amount of protein in the pleural effluent to help guide the decision as to when it is safe to remove a tube. We believe this variable may be more important in the management of chest tubes in patients who present with a pleural effusion and not as important for those who have undergone elective pulmonary resection. Although chylothorax can occur after elective pulmonary resection, a clinically significant amount of chyle is usually easily detected once a patient is eating a regular diet because of the obvious milky nature of the effluent. However, as we described in our article, it may be safe to remove chest tubes after elective pulmonary resection even when the output is greater than 450 mL/d, and perhaps the protein content is a variable that should be tested along with a higher volume. We look forward to a peer-reviewed published article from Ozdemir

and colleagues that critically and carefully tests this theory and the results.

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Prospective algorithm to remove chest tubes after pulmonary resection with high output – is it valid everywhere?

To the Editor:

I consider the article published by Drs Cerfolio and Bryant¹ an important contribution to the discussion regarding the amount of maximal daily pleural drainage allowed to remove a chest drain. The thoracic community widely accepts a range from 100 to 250 mL/d as a safe amount; however, this is based on personal or historical experience rather than on published data. I appreciate the majority of theses and considerations presented by the authors but particularly the discussion clearly presenting the limitations to their study.

Encouraged by the presentation of their material in June 2007, I decided to change our common protocol based on acceptance of 200 mL/d or less as a safe fluid amount for removal of the pleural drain (obviously not milky or bloody). This protocol was verified by thousands of our patients (we perform 600–800 open thoracotomies per year). Because our regional and single center covers the whole region of Western Pomerania in Poland, we are able to offer regular follow-up for the majority of our patients because they have no other option. We cooperate strictly with 5 large pulmonology units, so we usually quickly receive detailed information about our former patients. Our follow-up system seems more unified and strict than in Alabama, so I can confirm that readmissions because of effusions were incidental.

After changing our practice to the limits presented by the authors (drain removed if the amount of pleural drainage was < 450 mL/d), we surgically treated 40 patients (20 lobectomies, 1 bilobectomy, 2 segmentectomies, and 17 wedge resections by open thoracotomy) who had pleural drainage of more than 450 mL on postoperative day (POD) 1 (some patients did not reach 450 mL/d of pleural drainage and were excluded). The chest tube was removed on POD 1 in 15 pa-

tients and on POD 2 in 25 patients; on POD 3, all patients were discharged.

Six patients (15%) were readmitted 4 to 10 days later with pleural effusion confirmed by ultrasonography. They were all treated by thoracentesis (repeated twice in 4 patients), and the average amount of fluid evacuated was 600 mL per thoracentesis. All patients were discharged 3 to 4 days later except 1 patient with pleural empyema who required drainage.

As a result of this experience, we abandoned the recommendations presented by Dr Cerfolio and coauthors and returned to our common rules (drain removal if the amount of fluid is < 200 mL/d).

We discussed the situation with our team and concluded that the pleural space is somewhat "mysterious" and that its status depends on many external conditions (eg, altitude above the sea level, air pressure, strong winds, adjacent lung conditions). Rules that are good in one location are difficult to accept in other location. Pleural surface inflammation described by the authors as a factor increasing absorption of the fluid can be easily presented in a reverse way; one of the common symptoms of "pleuritis" is the production of pleural fluid (except so-called pleuritis sicca). Disappearance of the pleural space fluid is an indicator of the healing process. Perhaps 450 mL/d is a safe amount for chest drain removal in Birmingham but not in Szczecin or elsewhere. Nevertheless, I appreciate the efforts of Dr Cerfolio's team to elucidate the mysteries of this extremely interesting space.

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doi:10.1016/j.jtcvs.2008.04.017

Reply to the Editor:

We appreciate Dr Grodzki's interest in our article.¹ We are honored that he has applied some of the concepts we presented toward his practice. He has used, as per our recommendation, the removal of chest tubes when the output is 450 mL/d or less, assuming it is not blood or chyle. However, it is

unfortunate that he has not shared our favorable experience. He attributes this failure to “different environmental factors” between Birmingham, Alabama, and Szczecin, Poland. Although the author may be correct and there indeed may be environmental factors at play, I wonder if other reasons may explain his findings, because we are aware of several different groups in different parts of the country who have used the criteria of 450 mL/d or less with good results.

The author removed the chest tubes on postoperative day (POD) 1 in 15 patients. This is not our experience or our recommendation. As shown in Table 1, no patients had a chest tube removed on POD 1. However, I agree that our article is not clear on this point. We rarely if ever remove a chest tube on POD 1 after a lobectomy or bilobectomy because we believe there is little advantage to doing so. Although there is obvious pain involved with any indwelling tube, and we have written extensively on the advantage of fast-tracking patients and that early tube removal improves ambulation and pulmonary toilet, we have not condoned the removal of tubes on POD 1 after lobectomy. How many of Dr Grodzki's patients who returned for drainage was in this group of 15 patients? Second, and most important, is that Dr Grodzki does not tell us how many of his patients who underwent pleural fluid drainage were actually symptomatic with shortness of breath from their effusion and how many were better once drainage was performed. It has been my experience in the United States that it is common to see residual fluid and postoperative changes in the lung and pleural space after lobectomy, especially after bilobectomy. These are normal postoperative findings and are all too often overtreated by our medical colleagues. We often have to convince physicians not to overtreat these “pleural space abnormalities of air or some fluid” after thoracotomy and to leave the asymptomatic patient alone. Too often the chest roentgenogram is treated as opposed to the patient. Patients often receive antibiotics for “pneumonia” and diuretics or thoracentesis for fluid that never required treatment in the first place. Thus, it would be important to know how many of his patients were symptomatic and even more important to know how many were dramatically better once the fluid was drained. Finally, because most of his patients underwent a single thoracentesis, are we to be-

lieve that 1 tap, which rarely removes all of the fluid, completely resolved a pleural space problem? In our experience it does not.

We appreciate Dr Grodzki's letter, and we agree that the pleural space is a mystery. We look forward to further prospective studies in this regard.

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Removal of left ventricular tumors by a transaortic transvalvular approach with the help of thoracoscopic instruments

To the Editor:

We read with interest the case report from Walkes and colleagues¹ describing their new technique to resect a left ventricular fibroelastoma. Indeed, a left ventricular tumor location can be a real challenge for surgeons when a transmitral approach is performed. We agree with the authors that a safe and more reasonable surgical approach can be used through the aortic valve with the

help of a thoracoscope and thoracoscopic instruments.

However, we want to highlight that this technique has already been published. We² described in 2006 this same surgical approach (Figures 1), which helped us to ensure the mass nature, its resectability, and its single location, since in a few cases multiple tumors can be found operatively but undetected by preoperative echocardiography. In 1997, Espada and colleagues³ also published the use of a thoracoscope through a left atrial approach to visualize and resect a left ventricular fibroelastoma.

We are thankful for this other report, which highlights a safe but unusual procedure to remove left ventricular tumors.

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Figure 1. Excision of a mitral valve papillary fibroelastoma through an aortotomy with the help of a thoracoscope and thoracoscopic instruments.