FIGURE 1. Stent insertion was performed on both sides of the common carotid arteries.

had sudden syncope. Neurologic examination showed paraplegia and weakness of the right hand. Computed tomography showed the presence of type A aortic dissection with supra-aortic arch branch dissection. The left common carotid artery was occluded by the compression of thrombosed false lumen. Furthermore, infrarenal abdominal aorta was also occluded through compression by thrombosed false lumen. This patient underwent emergency total arch replacement and elephant trunk insertion under hypothermic circulatory arrest and selective cerebral perfusion. Five hours after surgery, the patient was extubated with a stable circulation; however, he reported right-sided weakness. Emergency computed tomography revealed that the left common carotid artery was still occluded and compressed by the false lumen as previously. The right

common carotid artery was dissected as well, and the false lumen was patent. Other branches of the aorta were patent on this study. We performed carotid artery stent insertion on both sides of the common carotid arteries (Figure 1). The patient recovered without any neurologic deficit.

Management of cerebral malperfusion remains a surgical challenge.^{2,3} If intervention had been delayed in our case, prognosis would not have been optimistic.⁴ Our patient had multiple malperfused arteries, so we did not believe that distal malperfusion repair should be done before aortic arch surgery, because entry closure might be able to improve distal malperfusion. As Abe and colleagues¹ described in their article, central repair does not always improve organ malperfusion, as in our case. In this type of situation, stent insertion to the malperfused vessel could be another option.

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IS VIDEO-ASSISTED THORACIC LOBECTOMY SAFE AND SUCCESSFUL FOR LOCALLY ADVANCED NON--SMALL CELL LUNG CANCER?

To the Editor:

We read with interest the recent article by Villamizar and colleagues.¹ Their background is that video-assisted thoracoscopic surgical (VATS) lobectomy has been advocated as the criterion standard for early stages of non-small cell lung cancer (NSCLC), but its role in locally advanced stages remains to be determined. They therefore focused on patients treated with VATS lobectomy who had tumor size greater than 3 cm, central location, or nodal involvement to determine the predictive impact of these factors on perioperative morbidity. They compared the data from these patients with data from patients who had tumor size smaller than 3 cm, peripheral location, or N0 stage.

We congratulate Villamizar and colleagues¹ for their huge experience

of 1195 VATS lobectomies, their excellent overall results (mortality, 1.6%; morbidity, 32%), and their low conversion rate (4%). We also congratulate them for the statistical data analysis and for the propensity score analysis that created homogeneous subclasses of patients to be compared. Because the study was retrospective, however, it should be underlined that the decision to perform VATS lobectomy in cases with tumor size 3 cm, central disease, or nodal involvement could have been conditioned by the personal experience of the single surgeon. We would like to focus attention on two topics.

First, when treating lung cancer the key point is to guarantee the most curative approach. Therefore the main question should be whether VATS lobectomy is oncologically equivalent to open lobectomy, even in locally advanced disease. In a recent paper Hanna and colleagues² reported their experience with 608 lobectomies (196 VATS and 412 open) for early-stage NSCLC and concluded that overall and disease-free survival did not differ between open and thorascopic groups. Palade and associates³ compared the effectiveness of nodal dissection between open and thorascopic surgery in a small group of 66 patients. They concluded that hilar and mediastinal lymphadenectomy were equivalent. We exhort Villamizar and colleagues¹ to publish their oncologic survivals for both techniques. In fact, data obtained from such a large population of patients treated should be very useful.

The second point concerns perioperative morbidity in the N-positive group. According to Villamizar and colleagues,¹ nodal involvement was the only variable that presented a significant difference in perioperative morbidity. In particular, it affected the conversion rate (7.2% in N1–N3, 3.3% in N0) and the postoperative transfusion rate. Unfortunately, Villamizar and colleagues¹ did not report any data concerning number of nodes

involved, nodal size, and number of the stations involved (hilar or mediastinal). These data could be significant for surgeons who aim to adopt VATS for locally advanced disease. Moreover, Villamizar and colleagues¹ did not report whether the involvement was clinically or postoperatively determined in N1 to N3 cases. It is still our opinion that, independent of surgical skill, perioperative morbidity and disease-free survival could be worse in the VATS group in case of hilar disease or increased nodal size.

In conclusion, the lesson of this interesting article is that VATS lobectomy may be equivalent to open technique independent of T and N status in terms of morbidity. The operating surgeon must be always ready for conversion, however, if possible before significant bleeding.

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BRAINSTORMING ON RECURRENCE RATES AND UNDERLYING MECHANISMS OF FUNCTIONAL MITRAL REGURGITATION

To the Editor:

The recent publication by Kwon and colleagues¹ highlights the advantages of complete ring annuloplasty relative to partial ring annuloplasty in terms of increasing durability and decreasing recurrence rates. We appreciate the efforts of Kwon and colleagues¹ to figure out the recurrence rate of functional mitral regurgitation and underlying factors that may help us to overcome special issues encountered during surgical treatment of the disease. After reading the article several times carefully, we would like to make further contributions to this issue in line with the statements of Kwon and colleagues¹ but from a different point of view.

With regard to recurrence rates, the currently accepted surgical treatment option of undersized annuloplasty has not yet matured. Thus it can be assumed that there are still some missing points needing to be clarified.

It is well known that the mitral annulus is roughly circular in diastole, and it becomes kidney shaped with an anteroposterior (septolateral) diameter smaller than the transverse diameter during systole. A $26\% \pm 3\%$ reduction of mitral orifice area during systole results from displacement of the aortomitral curtain toward the center of the orifice²; accordingly, the maximum tension on the mitral annulus is at the A2 to P2 direction in diastole and is determined by septolateral dimension changes during the cardiac cycle.³ Partial rings cannot reduce the tension in the A2 to P2 direction. It may thus be speculated that they are insufficient to stop the ongoing septolateral dimension changes, not only at the annular level but also at the equatorial level of the left ventricle. Tibayan and associates⁴ have suggested that the main determinant of malcoaptation in functional mitral regurgitation is the increase in septolateral dimension. This