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% ± 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

statement by Tibayan and associates⁴ gives supporting evidence for our hypothesis. Other suggestions may also provide alternative explanations for superiority of complete rings relative to partial ones.

As Kwon and colleagues¹ mentioned, although complete rings are better for the treatment of functional mitral regurgitation, they need further advances in multiple aspects to be considered as the best treatment option, because recurrence rates are still high. We think that the complete rings are quite well in fixing septolateral dimension changes at the annular level; however, they are not adequate for fixing septolateral dimension changes at the equatorial level of the left ventricle. That is why increased preoperative left ventricular dimension with a left ventricular end-diastolic diameter greater than 65 mm is the most accepted preoperative risk factor for higher rate of recurrence after ring annuloplasty in functional mitral regurgitation, as Kwon and colleagues¹ mentioned in their article.

We believe that brainstorming regarding the underlying mechanism of recurrence will improve our knowledge and understanding of functional mitral regurgitation.

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INSERTION OF PAPILLARY MUSCLE INSTEAD OF LEFT VENTRICULAR REDUCTION

To the Editor:

We congratulate Michler and colleagues¹ on their successful surgery and excellent study recently reported online in the *Journal*. We would also like to highlight some issues.

In the article's Figure E4, A,¹ there was no reduction in left ventricular end-systolic volume index (LVESVI)

in patients undergoing coronary bypass grafting (CABG), and left ventricular ejection fraction did not increase either (as seen in the article's Table 2¹). Nevertheless, left ventricular ejection fraction did increase significantly in patients with CABG plus surgical ventricular reconstruction (SVR), and we therefore propose that volume reduction is also necessary for the initial CABG-only group.

In the article's Figure E4, B,¹ LVESVI could be seen to drop as low as 30 mL/m² (normal volume) in many patients undergoing CABG plus SVR. So although ischemia was relieved by CABG, some dilatation may remain as a result of the continuation of relative ischemia, according to Laplace's law. SVR should therefore be expanded to include the mitral annulus and ventricular base. In addition, the existence of cases with a LVESVI drop to 20 mL/m² in the article's Figure E4, A,¹ leads us to think that an off-pump procedure should be preferred.

Instead of left ventricular reduction,² the insertion of papillary muscle to the ventricular wall could be palpated from outside on the beating heart, and the apex could be plicated by inserting pledgeted sutures anteroposteriorly and mediolaterally at this level. The ventricular cavity thus could be reduced near the base externally (Figure 1).

FIGURE 1. Insertion of pledgeted sutures for external left ventricular plication.