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Reply to the Editor:

We thank Drs Khandani and Detterbeck for their insightful comments on our article. Its main conclusion was that standard uptake values (SUVs) obtained from positron emission tomographic (PET) imaging were not useful for staging lung cancers smaller than 2 cm. We note that there was no disagreement with that conclusion, but rather the suggestion that visual inspection by experienced readers is now the accepted standard. A listing of some of the technical reasons why SUVs can be misleading is then given.

We agree that several recent articles have suggested that an expert reading may be equivalent to or better than a single cutoff for SUV. However, this is by no means a generally accepted standard. In fact, clinicians overwhelmingly continue to use the maximum SUV for differentiating benign from malignant lesions. This clinical practice is influenced by early PET reports,¹ in which a remarkably high accuracy of SUV was reported for differentiating benign from malignant nodules.

The problems outlined by Drs Khandani and Detterbeck regarding the limitations of the maximum SUV also affect the capability of experienced clinicians in making evaluations. Ultimately, anything that limits the ability to make quantitative assessments will also affect semiquantitative or nonquantitative assessments as well. The latest efforts in PET imaging include adding some form of gating to improve quantitative assessments, and likely there will

again be discussion of whether this will become the new accepted standard. For now, the maximum SUV continues to be reported and to guide clinical practice. We look forward to improvements in PET technology and remain hopeful that the utility of PET for small lesions will improve; however, there are some fundamental limitations that we believe will continue to be problematic in the foreseeable future.

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Pulmonary lobectomy for cancer in patients with chronic obstructive pulmonary disease

To the Editor:

We have read with interest the report from Baldi and colleagues¹ reporting their experience with pulmonary lobectomy for lung cancer in patients with chronic obstructive pulmonary disease (COPD).¹ On the basis of their retrospective evaluation of 137 patients, they concluded that patients with mild-to-severe COPD could have a better late preservation of pulmonary function after lobectomy than healthy patients. As discussed in a very precise way by the authors, this fact is related to a general improvement of the airway caliber and elastic recoil that could be, in its turn, related to relief of hyperinflation, chest wall mechanics, or both, even if in the nonemphysematous lung. In addition, resection of dead space could have its role.

We would like to briefly comment on these figures, trying to relate them to the possible role a pulmonary rehabilitation (PR) program might have in this kind of surgical population, and kindly ask for the authors to disclose their point of view according to their experience.

Today there is body of evidence regarding the efficacy of PR in the comprehensive management of patients with respiratory

disease, and PR programs are practiced worldwide.² Positive results in terms of improvement in dyspnea, exercise capacity, and quality of life are recognized in chronic obstructive and nonobstructive pulmonary disease, including COPD, cystic fibrosis, and restrictive thoracic disease.^{3,4}

Our group has a timely established interest in the issue of PR applied to patients who have undergone (or are candidates for) resection for lung cancer, and we have reported evidence that patients who underwent PR after pulmonary resection demonstrated a better improvement than those who, at discharge from the surgical unit, did not attend any postoperative rehabilitation protocol. In fact, we have reported that an early postoperative rehabilitative intervention prevents deterioration and speeds up recovery of function, with direct effects on ventilatory, gas exchange, and hemodynamic parameters (work of breathing, lung compliance, alveolar-arterial difference, maximum oxygen consumption, arterial oxygen tension, heart rate, arterial lactate concentration, cardiac index, and pulmonary artery pressure) and little or no significant effect on static and dynamic lung volumes, according to previously outlined experiences.⁵ Further analysis (data submitted for publication) of our experience supports this evidence. Because it appears that only patients with mild-to-severe COPD obtain an improvement in terms of lung volumes (related to the preoperative COPD index also)¹ from the removal of lung parenchyma associated with resection for cancer, we would like the authors to comment on the fact that a postoperative rehabilitation program adopted in these patients could improve ventilatory, hemodynamic, and gas exchange parameters concurring to a better clinical outcome and quality of life status.

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Reply to the Editor:

We do agree with Cesario and colleagues that pulmonary rehabilitation (PR) could improve clinical conditions in patients with chronic obstructive pulmonary disease (COPD), as well as in patients operated on for lung cancer, although PR in patients with COPD takes into account different strategies compared with rehabilitation in surgical patients. Indeed, PR in patients with COPD, particularly long-term physical training programs, can ameliorate dyspnea, exercise capacity, and quality of life but not pulmonary volumes and gas exchange.¹ On the other hand, preoperative and postoperative PR, including chest physiotherapy, deep-breathing exercises, continuous positive airways pressure, incentive spirometry and so on, can reduce respiratory complications and hospital stay² by reducing bronchial sputum impaction and bronchial secretions and by alveolar recruitment. This approach is commonly adopted in most thoracic units and usually in general operations for high-risk patients.

In our study we evaluated patients submitted to lobar or limited lung resection, taking into account the late postoperative pulmonary recovery; for this reason, we have not considered the early functional outcome and postoperative pulmonary complications. The pulmonary function tests and blood gas analysis were performed after hospital discharge (usually be-

tween the 3rd and 15th month), and therefore in our study preoperative and postoperative PR has not been considered because of its limited role in this context. In our hospital we perform a short course of preoperative and postoperative PR (chest physiotherapy, incentive spirometry, huffing, coughing, and, when necessary, continuous positive airways pressure) in patients submitted to lung resection, and this contributes to reduce postoperative pulmonary complications, but I do not have experience on the effect on gas exchange and on hemodynamic and pulmonary function, although I suspect that this would occur mainly in patients with excessive bronchial secretion and postoperative atelectasis. Reports^{2,3} emphasize the importance of intensive PR (at least a 2- to 4-week course) in patients undergoing lung cancer surgery, and I think that in patients with moderate-to-severe COPD, this strategy can improve the outcome of lung resection, especially considering that these patients seem to deteriorate less compared with normal patients.⁴⁻⁸

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Dor fundoplication after myotomy for achalasia: Useful, unnecessary, or harmful?

To the Editor:

We appreciated the interesting and accurate article from Rice and colleagues¹ that was recently published in the *Journal*. In this report, the authors emphasized the fact that addition of a Dor-type fundoplication procedure to a Heller myotomy increases both resting and residual lower esophageal sphincter (LES) pressure, reducing the adequacy of myotomy itself. Our group has established experience with this issue. We reported that the basal LES pressure, measured by intraoperative manometry, was always close to 0 mm Hg after myotomy and increased after Dor fundoplication.² On the basis of our own previous observations, we presume that pressure values recorded in patients undergoing myotomy alone remain somewhat high because of an underlying incomplete incision of muscular fibers as the result of technical difficulties (62% of patients in the study by Rice and colleagues¹ had prior nonsurgical treatments). Furthermore, in the report,¹ the addition of a Dor fundoplication increased both resting and residual LES pressure but did not impair esophageal emptying. In our experience, higher pressures recorded in the Heller-Dor group could be easily related to a newly established "high pressure zone" rather than to an inadequate myotomy.

According to this evidence and that outlined in the prospective, randomized, double-blind clinical trial by Richards and colleagues,³ we advise the use of a partial anterior fundoplication after myotomy for several reasons. First, because the Dor-type procedure does not cause a significant postoperative dysphagia, and second, and most important, as also demonstrated by Costantini and colleagues,⁴