



# Minimally invasive surgery for early stage lung cancers: satisfactory, but we can do better

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*Provenance:* This is an invited Editorial commissioned by Section Editor Dr. Song Xu, MD, PhD (Department of Lung Cancer Surgery, Tianjin Medical University General Hospital; Tianjin Key Laboratory of Lung Cancer Metastasis and Tumor Microenvironment, Lung Cancer Institute, Tianjin, China).

*Comment on:* Boffa DJ, Kosinski AS, Furnary AP, *et al.* Minimally Invasive Lung Cancer Surgery Performed by Thoracic Surgeons as Effective as Thoracotomy. *J Clin Oncol* 2018;36:2378-85.

Submitted Aug 01, 2018. Accepted for publication Oct 30, 2018.

doi: 10.21037/atm.2018.10.74

**View this article at:** <http://dx.doi.org/10.21037/atm.2018.10.74>

Since the first major pulmonary resections using closed chest surgery, reported 25 years ago (1), the attitude of the surgical and oncology community has changed substantially. Not so long ago, an editorial stated the following sentence: We can no longer ignore the accumulating evidence of these recent reports that suggest VATS lobectomy is inferior oncologically to open lobectomy for clinical stage I lung cancer (2). We have gradually moved from a strong opposition (3,4) to an acceptance of minimally invasive techniques by a large part of the surgical community. The first step in this process came in 2007 with North American large series (5). At the same time, the American College of Chest Physicians (ACCP) updated its guidelines for non-small cell lung cancers (NSCLC) by stating that minimally invasive techniques were an acceptable alternative to open chest surgery (6). A further milestone was reached in 2013, again with the ACCP guidelines which this time recommended that minimally invasive techniques should be preferred for early stage tumors (7). In 2018, the penetration rate of these techniques varies from 20% to 80% depending on the country and the center (rates of more than 70% are often related to a center effect). On average, depending on the country, the rate of use of video-assisted techniques (VATS) is around 40%. Surgeons who continue to treat early-stage tumors by thoracotomy, i.e., the remaining 60%, claim these techniques are less safe, that it has not been demonstrated whether survival is equivalent

to that of traditional techniques and, above all, that radical lymph node dissection cannot reach the same radicality (2,8).

The interest and merit of the publication of Boffa *et al.* is to make an important contribution to the debate based on a very large series of stage I NSCLCs operated by thoracotomy (4,448 patients) and thoracoscopy (6,149 patients), from a registry containing only certified thoracic surgeons (9). The study shows two findings: four-year survival is equivalent and even slightly higher in the thoracoscopy group (68.6% versus 64.8%;  $P=0.003$ ), despite a significantly higher rate of upstaging in the thoracotomy group (14.6% versus 11%,  $P<0.001$ ). The survival rate is equivalent to ours (10) and to other studies (11). Furthermore, the study shows that there is no significant difference in perioperative mortality. These results lead Donington, who signs the editorial accompanying the article to write that thoracotomy to treat stage I tumors is outdated (12).

The fact that survival is equivalent in the two groups, while lymph node clearance seems less complete by thoracoscopy than by thoracotomy, can lead to two different interpretations: (I) radical lymph node dissection is eventually not a fundamental factor in patient survival or (II) survival in the thoracoscopy group would be even better if lymph node dissection were more radical. Indeed, Boffa *et al.* emphasize the fact that incomplete lymph node dissection poses the two following problems: it leaves tumor

tissue in place and it ignores lymph node metastases that would have required adjuvant chemotherapy. We can also suggest a third concern: the survival of the thoracoscopy group is probably deteriorated by the fact that N0 patients are actually N2. Yet, several studies have shown that it is possible to perform satisfactory node dissection via thoracoscopy (13) although there is a learning curve (14).

The problem of incomplete lymph node clearance illustrates the weaknesses of this type of multicentric studies, despite the quality of the statistical work and the consideration of a large number of variables, as well as the measures taken to reduce study bias. In all the variables analyzed by Boffa *et al.*, only one of them relates to surgery, i.e., the annual volume of lobectomy (this is the volume per center and not per surgeon). The recently published article on the survival of patients operated on for stage I NSCLC by lobectomy or sublobar resection (SLR) underlines this concern (15). In this study, the increased risk of recurrence was 39%, which is worrying. But the vast majority of SLR were actually wedge resections—which we know are not oncological resections—the margins of resection were invaded in 6% of cases and the average number of lymph nodes removed was only 1. Similarly, in the article of Boffa *et al.* some data raise questions: an invaded margin rate of 3% (whereas all NSCLC were stage I) and a rate of upstaging significantly lower than that of patients operated on by thoracotomy. This shows that even for certified thoracic surgeons, technical progresses must be made. In a way, if this study demonstrates that the survival of the thoracoscopy group is equivalent to that of the thoracotomy group, one might wonder if it could not be even better. Despite the sophistication of the statistical analysis, studies based on registries and databases, even with large numbers of patients—as is the case in the Boffa *et al.* study—suffer from the following limitation: they only reflect a situation at the moment T and ignore the surgeons' ability to progress and to find solutions. For example, the study by Licht *et al.* on lymph node dissection, based on the Danish registry, concluded that the rate of upstaging by thoracoscopy was much lower compared to thoracotomy (8). The results were right and the conclusion was probably adequate at the time of the study, i.e., 2013. However, any visitor to an expert center may observe that thoracoscopic LN dissection is equivalent, if not superior, to open LN dissection. We thus have 2 choices: either comply with the conclusions of these studies, or seek solutions to do as well or better with VATS as with open surgery.

The open chest and closed chest approaches differ in

one fundamental aspect: the majority of thoracic surgeons perform an open chest lobectomy in more or less the same way, because the teaching has been more or less identical. On the other hand, closed chest lobectomy techniques are extremely diverse and variable (16), not only between centers, but even between surgeons in the same institution. Among the many differences are: the use or not of a robot, a various number of ports (from one to five), very different imaging systems and instrumentation, from the most rudimentary to the most sophisticated, etc. In 2013, Douglas Wood asked the question in an editorial “What is most important in improving outcomes after pulmonary lobectomy: the surgeon or the approach?” (17). There are many indicators that this question is still relevant.

The competence and skills of the thoracic surgeon in video-assisted techniques could be a crucial issue in the coming years due to the evolution towards sublobar resections (SLR) to treat an increasing number of early stage tumors. It is indeed demonstrated and accepted that the benefit of SLR is maximum when the procedure is done by VATS. But some segmentectomies, when performed this way can be complex and challenging and require not only technical skills but also technological refinements. It is likely that not all thoracic surgeons will be equal in this evolution.

After a phase of skepticism towards minimally invasive techniques, we suddenly entered a phase of rapid development that was not always under control (18). We must now invest a time of maturity, which is one of thoughtful technical and technological development, aiming at improving our results. If surgeons do not, they expose themselves to creating a situation they fear, that is, the development of expert centers, as illustrated by the conclusion of the editorial by Donington: “Patients, pulmonologists and oncologists should seek out surgeons who can provide this level of expertise” (12).

## Acknowledgements

None.

## Footnote

*Conflicts of Interest:* The author has no conflicts of interest to declare.

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**Cite this article as:** Gossot D. Minimally invasive surgery for early stage lung cancers: satisfactory, but we can do better. *Ann Transl Med* 2018;6(Suppl 1):S86. doi: 10.21037/atm.2018.10.74