



# Video assisted thoracoscopic surgery vs. thoracotomy for lobectomy: why are we still talking about this?

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Recently published results from Long *et al.* in the February 2018 edition of the *Annals of Thoracic Surgery* report on a completed surgical trial that many thoracic surgeons may have deemed mission impossible: a trial comparing results for patients undergoing lobectomy for early stage non-small cell lung cancer (NSCLC) who were randomized by approach to resection by video assisted thoracoscopic surgery (VATS) versus axillary thoracotomy (1). Results from this multi-center trial involving five tertiary hospitals demonstrate short term outcomes for a VATS approach to be safe and reliable, and may be superior to axillary thoracotomy for operative time and blood loss. With the exception of shorter operative times for the VATS group of patients, few would describe these results as surprising. Though the results themselves may not be surprising, the challenges involved in the conduct of such a trial are interesting. Thoracic surgeons who routinely perform minimally invasive lobectomy for early stage NSCLC might cry outrage at subjecting patients to even a muscle sparing thoracotomy when minimally invasive approaches are possible. It's fair to question whether a study design like this would even achieve IRB approval at any academic institution in North America. The general presumption is that patients, if given the choice between thoracotomy of any type, compared to a minimally invasive approach, would likely opt for the less invasive approach. Supporting this presumption, is the fact that it took six years at five high volume thoracic surgery programs to enroll 425 patients

to be randomized for this trial. One of these centers alone reported case volumes between 4,000–12,000/year for major thoracic surgical procedures for lung cancer, with a total of 43,528 cases during 2009–2017 (2). Patients having surgical resection of NSCLC by thoracotomy likely aren't offered minimally invasive approaches for various reasons, but the most important one is likely based on surgeon preference, not patient preference.

Despite surgeons' feelings/preferences for specific surgical approaches, the reality is that randomizing patients to thoracotomy versus VATS for a clinical trial is not outrageous. If a thoracotomy is done with a muscle sparing axillary approach which is what was done in this trial, the expected differences in short term or even long term outcomes might be less than when compared to posterolateral approaches that sacrifice the latissimus dorsi muscle. The motivation for such a trial though is somewhat curious given the current push towards increasingly less invasive approaches such as uniportal VATS. At a time when focus is shifting towards assessing the role of minimally invasive approaches for patients with more complex tumor pathology or advanced stages of NSCLC, or discerning subtle differences between robotic assisted approaches versus VATS approaches for surgical resection of NSCLC, a reasonable question might be whether surgical approach matters as much as we think it does. As our understanding of the complex immune-biology of NSCLC increases, the importance of variables such as surgical approach for

resection may become less, especially long term outcomes. With any randomized controlled trial comparing surgical approach or extent of resection (such as the lobar *vs.* sublobar resection for NSCLC less than 2 cm in size), outcomes are likely impacted by many factors other than those being measured.

A potentially more relevant question regarding the results of this trial is whether or not they will change surgeons practice. If this trial showed superior results for axillary thoracotomy, are we going back to widespread adoption of this approach? We would not think so. Rates of VATS continue to steadily increase, though this has taken decades. More recently the same phenomenon has been noted for robotic assisted approaches. Is that because we know they are better or we think they are better? Does retrospective data show that they are better? Surgical revascularization with CABG was proven to be superior when compared to percutaneous coronary intervention (PCI) for diabetics with multi-vessel coronary artery disease, but many patients still elect to undergo the less invasive treatment approach (3). Reasons for that phenomenon are multifactorial. Despite the potential for physician bias, patient preference cannot be discounted as a factor. Who really wants a sternotomy if they can avoid it? Who really wants a thoracotomy? When given the choice between VATS and axillary thoracotomy, it would be expected that patients will choose VATS every time. Surgeons favoring minimally invasive approaches will likely stick with those, and those adverse to or reluctant to embrace VATS, now have results supporting their view that VATS is not superior (except maybe with intra-op blood loss). Long term oncologic results will be interesting, but again, might not be expected to alter surgeon practice. Retrospective data exists offering strong support for potential superiority of VATS.

Intuitively, minimally invasive approaches have been shown to really benefit older, frail patients, expanding treatment possibilities for this group. Of note, this trial excluded patients above the age of 75. It would have

been interesting (though maybe ethically questionable) to examine if differences in outcomes were noted for patients older than 75 years of age. This is precisely the group of patients where expected risk for 30-day mortality after lobectomy was shown to dramatically increase when examined using a large multi-center database, and where minimally invasive approaches may have more of an impact on short term outcomes (4). It is impressive and commendable that the authors could overcome multiple competing forces of potential bias to complete this trial.

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### Footnote

*Conflicts of Interest:* The authors have no conflicts of interest to declare.

### References

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