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DOES VIDEO-ASSISTED THORACOSCOPIC SURGICAL (VATS) LOBECTOMY REALLY RESULT IN FEWER COMPLICATIONS THAN THORACOTOMY? THE BIASES ARE CLEAR, THE ROLE OF VIDEO-ASSISTED THORACOSCOPIC SURGERY LESS SO

To the Editor:

The article by Boffa and colleagues,¹ "Fewer Complications Result From a Video-Assisted Approach to Anatomic Resection of Clinical Stage I Lung Cancer," is an excellent attempt to compare video-assisted thoracoscopic surgery (VATS) and thoracotomy. There are many more limitations, however, than those stated in the article. We must be objective and honest with ourselves and our colleagues before we begin to claim that VATS is the criterion standard.

1. The nature of the Society of Thoracic Surgeons database does

not allow the accurate assessment of VATS conversions to thoracotomy. This limitation is stated in the article, but it must be emphasized. This alone can falsely favor VATS relative to thoracotomy.

2. The patients who had VATS and thoracotomy were excluded from the comparative analysis. This methodology is incorrect. This group should be included in the VATS group as an intent-to-treat analysis.²
3. How many VATS anatomic resections resulted in pneumonectomy or some other catastrophic complication? Catastrophic complications, including unexpected pneumonectomy, are frequently overlooked.³ In a previous study, unplanned pneumonectomy occurred in 1 of every 200 cases.³ This is not captured by the Society of Thoracic Surgeons database, and Boffa and colleagues¹ did not address this issue.
4. Individual surgeons decide whether to perform VATS or thoracotomy for many reasons, including body habitus, comorbidities, dense adhesions, the presence of anatomic issues such as a left internal thoracic artery in a left upper lobe lobectomy, and learning curves. These factors were not included in this analysis. Not taking this surgical bias into account makes thoracotomy appear unfavorable to a higher degree and VATS appear better to a higher degree.

5. Propensity matching cannot control for the selection biases occurring before surgery or during surgery in patients who had thoracotomy and VATS (possible conversions). It is up to the investigator to consider all possible factors that may bias outcome in favor of VATS and against thoracotomy and to attempt to control for such factors.

There is selection bias taking place at multiple levels here. VATS is not for every patient, nor is it for every surgeon. We need to monitor alleged

benefits carefully before subjecting patients to harm from overzealous performance of VATS lobectomy when thoracotomy is warranted. We must therefore be careful of falsely overstating the benefits of VATS lobectomy.

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THE VATS ADVANTAGE: SEEING IS BELIEVING ... AND VICE VERSA

Reply to the Editor:

My coauthors and I appreciate Dr Flores's thoughtful response to our study. Dr Flores has emphasized the potential for bias related to video-assisted thoracoscopic surgery (VATS) cases that were converted to thoracotomy. Although it is not possible to identify cases of conversion directly, it is possible to identify patients that underwent a VATS on the same day as an anatomic lung cancer resection by thoracotomy (subsequently referred to as "multiple approach patients"). The multiple approach patients would include patients with conversion as well as patients who underwent a diagnostic VATS before a planned thoracotomy for anatomic resection. We have evaluated 2 ways in which converted cases could bias the results.

The inclusion of converted cases in the thoracotomy cohort would bias against the thoracotomy approach if converted cases represented those at a disproportionately high risk for postoperative complications. We therefore isolated the multiple approach patients (which include the converted cases) to evaluate the complication rate in this subset of patients. The complication rate in this multiple approach cohort was 38.5%, which was similar to that in patients undergoing thoracotomy alone (35.9%; $P = .071$) but higher than that in patients undergoing VATS alone (28.9%; $P < .001$). Converted cases thus do not appear to pose a disproportionately high risk for postoperative complications, and eliminating them from the thoracotomy cohort would not eliminate the study findings that VATS was associated with fewer complications than thoracotomy.

The exclusion of converted cases from the VATS cohort could bias in favor of VATS if the conversions were in response to VATS-specific intraoperative errors (mistakes that could have been avoided by a planned thoracotomy). Intraoperative errors can be directly and indirectly associated with postoperative complications (longer operating room times, intraoperative transfusions associated with postoperative complications). To adjust for this possibility, we combined the multiple approach patients with the VATS cohort. This would not be an intention-to-treat analysis (because not all multiple approach patients had conversions; some underwent planned diagnostic VATS followed by planned thoracotomy). The complication rate of the VATS combined group (multiple approach plus VATS only) was 31.12%, which remains significantly lower than that of the thoracotomy-only group (35.86%; $P < .0001$). Inclusion of converted cases with the VATS cohort thus does not eliminate the study findings that VATS was associated with fewer complications than thoracotomy.

We therefore believe that the more detailed evaluation of the impact of

the multiple approach patients, although an imperfect reflection of conversion, supports our assertion that the safety benefits of VATS are not an artifact of unadjusted conversion bias.

Unexpected pneumonectomy is not specified in the Society of Thoracic Surgeons database, and a disproportionate incidence would be of obvious concern. As with any significant variation in surgical approach, it is reasonable to assume the existence of a set of complications of varying severity (including catastrophic) that are directly attributable to the VATS approach (and potentially avoidable by thoracotomy). Much like common bile duct injuries with the adoption of the laparoscopic cholecystectomy, data resources must expand to capture these approach-specific complications. A more detailed understanding of the prevalence of these complications could motivate an evolution to account for these pitfalls (eg, converting challenging cases before an otherwise unwarranted pneumonectomy becomes inevitable).

Propensity matching is only able to adjust for variables that are included in the match. That being said, many of the variables that characterize the VATS candidacy are captured by the Society of Thoracic Surgeons and were included in the matching (habitus, previous thoracic surgery, comorbidities, laterality, lobar location, etc). There are, however, a number of variables that are not captured (eg, central vs peripheral tumor location). By concentrating on stage I tumors, we believe that we have minimized some of these effects (eg, clinical N1 disease) that can impact surgical approach, but we acknowledge that others persist.

To date there have been hundreds of VATS studies comparing thousands of lung cancer resections. The finding of fewer complications by the VATS approach appears to be consistent. As the volume of data increases, however, we must not ignore the persistence of potentially significant

limitations. Consistency in the repetition of a flawed approach must not be mistaken for validation. Our study was specifically designed to improve and extend previous comparative strategies, rather than simply repeating them. We believe that our study strengthens the body of literature supporting VATS safety; however, there remain unanswered questions.

Recognizing individual variability in the VATS skill set, as well as potential variability relating to oncologic end points, it is difficult to declare a new criterion standard surgical approach to lung cancer. In the absence of a clinical trial, comparative effectiveness study of VATS versus thoracotomy will likely remain an imperfect science. We must be prepared for a point when available data resources have been maximally analyzed, scrutinized, and revised, and then we must either commit to the completion of a clinical trial or accept the trends in light of the residual limitations.

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CLINICAL CHALLENGES IN THE TREATMENT OF PATIENTS WITH TRACHEOSTOMY IN A HYPERBARIC CHAMBER

To the Editor:

Anastomotic separation is a rare but serious complication of tracheal repair. In a recent report, Stock and colleagues¹ successfully used hyperbaric oxygen (HBO) therapy in patients with anastomotic complications after tracheal repair. We are pleased to see that the report by Stock and colleagues¹ supports our previous findings, which showed that HBO therapy increases healing after tracheal resection and reconstruction in rats.² We hope that these