Editorial Commentary

Comparing open and closed chest surgery for early-stage lung cancer: still relevant?

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In a remarkably short time—about 15 years—the thoracic surgeon community has moved from great skepticism about minimally invasive techniques to acceptance and progressive adoption. Long *et al.* are right to point out in the introduction to their study (1) that the dispute created by the arrival of video-assisted thoracic surgery (VATS) was unprecedented. In 1993, when some pioneers published the first results of video-assisted lobectomies (2-6), the Video-Assisted Surgery Study Group (7) wrote "*Video-assisted lobectomy remains experimental with the potential for major complications*" and concluded that "*Further studies will be required to define its precise role in thoracic surgery*". In 2010, 13 years after the first publications (2) and 4 years after the first large series (8) Anthony Yim wondered in an editorial why video-assisted lobectomies raised so much reluctance (9). Gradually, as Long *et al.* points out, the technique was considered by the American College of Chest Physicians (ACCP) as an acceptable alternative for treating early-stage non-small cell lung cancer (NSCLC) (10) and in 2013 as the preferred technique (11). In a way, thoracic surgeons have confirmed this well-known adage when a new technique appears: the reaction to this technique evolves in three stages: (I) this cannot be done; (II) this should not be done; (III) I can do it.

This preamble may seem a little far from the purpose of this editorial, which is to comment on the work of Long *et al.*, but it is necessary to address one of the points raised by this team: there have so far been only four randomized controlled trials (RCTs) comparing VATS lobectomies to open lobectomies. Their study is only the 5th and, to my knowledge, 2 others are in progress. The low number of RCTs is well explained by this paradox: on the one hand, expert centres are convinced of the superiority of closed chest surgery and would consider a controlled trial as "unethical" and on the other hand, high-volume centres, which until now have not done VATS, could not, by nature, participate in a trial.

The controlled trial whose results are reported by Long *et al.* (1) is very interesting both in its results and in the questions it raises. Let's begin with the results.

The study shows that morbidity and mortality are no different between the two groups. If we assume that the length of hospitalization is an indirect measure of the complication rate, we can observe that it is identical in both techniques (we notice that the average postoperative stay is long—14 and 15 days—compared to some western standards but this is possibly explained by the geographical distance of a part of the patients). The conversion rate is also an indirect indicator of the intraoperative complication rate (12). It is 3.7%, which is low and compares favorably to other large series. Two results reflect the fact that VATS has entered a phase of maturity: the operating time is significantly shorter in the VATS group and the average bleeding is also significantly lower in this group. It is remarkable to note that there was no major bleeding in either group, a difference from a number of retrospective

studies (12-16). Indeed, in the European survey, the vascular complication rate was 2.9% and the majority of these complications led to conversion (12). Overall, morbidity in this trial is low, also in the thoracotomy group (10%), whereas it is between 30% and 40% in most studies, including the National Cancer Database (NCDB) analysis of 120,000 patients (17).

Due to insufficient follow-up, the authors have not yet analyzed survival. But from an oncological standpoint, their results confirm the complete resection rate is identical in both groups and mediastinal lymphadenectomy is comparable with the two techniques. A few years ago, some had shown that lymphadenectomy was less radical by thoracoscopy than by thoracotomy (18), but this was probably due to a weaker experience (19) and a less efficient equipment. Most recent studies do not find any significant difference (20,21). It would have been interesting to see if the authors confirmed the results of the Boffa *et al.* study, i.e., the rate of N1 upstaging is lower after VATS than after open surgery, but this difference disappears after the learning curve (19). In total, it is likely that the study by Long *et al.* will soon confirm survival is identical, if not superior, between the two approaches, as shown in 2009 by the meta-analysis by Yan *et al.* (22), whose results were confirmed in 2014 by the study by Paul *et al.* (23).

In summary, the study confirms that the thoracoscopic approach for major pulmonary resections (MPR) does as well as the thoracotomy approach in terms of morbidity and oncological value, and even better on some points (operative time, bleeding).

But this work raises other questions for the future.

The first question is the real value of this type of study. Theoretically, the multicentric nature of the trial is an advantage since it increases the number of inclusions and its results are closer to real life than those of a unicentric study from a so-called expert-centre. But in practice, this benefit is not so obvious. Indeed, if we can assume that the lobectomy and lymphadenectomy techniques are more or less identical from one surgeon to another, we know that the lobectomy-VATS technique is very heterogeneous by the number of trocars used, by the presence or not of an access incision, by the nature of the video equipment and instrumentation used. This is without considering the potential use of a robot. Also, the comparison is not that of an orange with an apple but of an orange with a wide variety of apples, which introduces a significant bias.

The second question raised by this type of study is its relevance at a time when lung cancer surgery is rapidly evolving and changing, so that what was true yesterday is no longer necessarily true today. Thus, the study was initiated at a time when sublobar resections (SLR) were rarely performed in most centres. However, since the work only concerns early stage tumors, it should have a high rate of SLR, which is not the case. We know that the morbidity of SLRs is less than that of lobectomies (24,25), especially when they are performed by thoracoscopy (26). And we also know that they allow the same survival as lobectomies when performed for early stage tumors and when they are associated with lymphadenectomy (24,27). Knowing that performing complex closed chest segmentectomies is a real challenge for many teams, this will complicate future RCTs.

But the main issue in the near future will not be to compare surgical resection techniques with each other, but to evaluate surgery against non-surgical techniques, such as stereotactic body radiation therapy (SBRT), radiofrequency, cryotherapy and destruction techniques by electromagnetic bronchial navigation (ENB) or any other mean. The comparisons of surgical techniques could then become obsolete. Until this next step is reached, it is advisable to stay focused on the technique that is most mastered by respecting the best compromise between minimally invasive approach and oncological radicality.

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Footnote

Conflicts of Interest: The author is consultant for an instrument manufacturer (Delacroix Chevalier).

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