
Steroids and Arteriovenous Thrombosis
The Role of Underlying Disease

To the Editor:

Recently in CHEST (May 2013), Stuijver and colleagues suggested that iatrogenic corticosteroids are risk factors for pulmonary embolism as much as endogenous steroids are associated with an increased risk of thrombosis. In this context, it is useful to note that thrombotic risk with corticosteroids not only is restricted to venous circulation but also involves the arterial side. A recent study by Fardet and colleagues suggested that iatrogenic corticosteroids are risk factors for cardiovascular disease. This analysis, as in the present study, possibly reflects the increased cardiovascular risk associated with the disease conditions for which steroids are prescribed. Subclinical atherosclerosis is noted in many conditions where chronic inflammation is a pathophysiological factor. However, Fardet and colleagues noted cardiovascular events with long-term use of corticosteroids but not venous thrombosis. In these cases, long-term suppression of the underlying inflammatory state would have been necessary. In other words, steroid use in the initial period of the prescription and at higher doses is an additional risk factor for venous thrombosis, whereas smaller-dose, long-term prescriptions are risk factors for arterial thrombosis.

What does this signify? In both cases, the underlying disease condition matters as much as the steroid usage, but development of venous thrombosis also suggests high levels of inflammation associated with the underlying disease, whereas arterial thrombosis denotes low-grade inflammation and secondary atherosclerosis. In summary, the venous and arterial thrombotic risks associated with steroids most possibly reflect the severity of the underlying diseases that required these drugs, with the contribution from these drugs itself being an additive effect.

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Financial/nonfinancial disclosures: The author has reported to CHEST that no potential conflicts of interest exist with any companies/organizations whose products or services may be discussed in this article.
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DOI: 10.1378/chest.12-3096

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Dilemma in Pulmonary Metastasectomy
Video-Assisted Thoracoscopic Surgery or Thoracotomy?

To the Editor:

We read with interest the report by Eckardt and Licht in a recent issue of CHEST (December 2012). Although we congratulate them for their effort in clarifying the potential superiority of open pulmonary metastasectomy for treatment of patients with resectable pulmonary metastases, we want to share some significant concerns.

Both preoperative imaging and procedure via video-assisted thoracoscopic surgery (VATS) might miss some small metastatic nodules. The authors argued that the missed metastases might lead to higher recurrence rate and more mediastinal lymph node involvement, both of which might be detrimental to prognosis of those patients. However, VATS has been shown to be less invasive, with minor pain, trauma, and immune disturbance, and to better preserve quality of life and compliance with subsequent adjuvant therapy. In addition, metastasis of malignance is a systemic disease. Some recurrent pulmonary metastases may come from another micrometastasis other than the possibly missed lesions. Also, whether the mediastinal lymph node involvement is due to the pulmonary metastases or other lymphatic micrometastasis is still controversial.

Moreover, a few retrospective case-control studies concluded that survival after metastasectomy by VATS was not inferior to open surgery. Several studies also reported that repeated metastasectomy was performed in 10% to 20% of patients who had undergone metastasectomy for the first time, and the survival curves were equal between the first and the first repeated metastasectomy (24.0% to 56.0% vs 23.0% to 53.8%). With regard to this report, VATS should be technically comparable to open thoracotomy in resection of those 55 imaged nodules, even though some of them might not be palpable. Speculatively, we presumed that the four nonpalpable nodules via VATS were not metastatic lesions. Then the ratio of improper resection caused by VATS and thoracotomy was 18.2% (10 of 55) and 35% (28 of 80), respectively (P < .05). Therefore, we have several concerns: (1) Can the total recurrence rate be actually reduced by the relatively radical resection caused by VATS? (2) Is it definitely beneficial at the cost of more invasiveness and such a high rate of excessive treatment caused by open thoracotomy? (3) If resectable recurrent metastases do happen, what can we do for a patient who has undergone an open thoracotomy?

We argue that factors such as efficacy, invasiveness, compliance, and preservation of quality of life should be all taken into consideration when planning a palliative treatment strategy such as pulmonary metastasectomy. We are also looking forward to more prospective randomized trials with more cases included to elucidate all confusions.

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Financial/nonfinancial disclosures: The authors have reported to CHEST that no potential conflicts of interest exist with any companies/organizations whose products or services may be discussed in this article.

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DOI: 10.1378/chest.13-0258

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Response

To the Editor:

Drs Liu and Liu raise a concern about open pulmonary metastasectomy in their letter regarding our recent article in CHEST (December 2012). They argue that video-assisted thoracoscopic surgery (VATS) is preferable over thoracotomy because it is less invasive—an assumption which is shared by most thoracic surgeons even though the evidence base for this assumption is low. Drs Liu and Liu raise an important question for which there is no answer in the existing literature, including our article: namely, whether recurrence rates after metastasectomy can be reduced by VATS, a belief which is spreading in the thoracic community but which is also against current recommendations.

In our article, which is the first, to our knowledge, observer-blinded study investigating VATS vs open metastasectomy, we demonstrated that even in the 21st century, in a high-volume VATS center that uses modern surgical technology, metastatic disease is not completely removed when manual palpation is restricted by the port holes. Whether survival rates or recurrence rates are different between thoracotomy and VATS procedures is currently unknown, and we certainly agree with Drs Liu and Liu that more prospective trials are needed. We urge the thoracic surgical community to launch large prospective randomized multicenter trials on this topic.

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Lung Point
Not for Everyone

To the Editor:

We read with great interest the article by Oveland et al in a recent issue of CHEST (February 2013). We would like to thank the authors for demonstrating in an evidence-based manner the long-believed concept favoring the use of lung ultrasonography in the prediction of progression of pneumothorax. Among all the ultrasonographic signs of pneumothorax, the demonstration of lung point is the only one with 100% specificity and, unfortunately, the one with the least sensitivity.

In the seminal article by Lichtenstein et al in 2000, the “lung point” was observed only in 44 of 66 cases of pneumothorax, yielding overall sensitivity of 66%. The sensitivity increased to 75% (six of eight subjects) in subjects with occult pneumothorax. In another retrospective analysis by the same author, lung point was present in 34 of 43 cases of occult pneumothorax, yielding a sensitivity of 79% and a specificity of 100%.

The difficulty in identifying a lung point is evident even in this present study, wherein there is a significant discrepancy in the number of times the interpreting anesthesiologists were able to locate the lung point in anterior and posterior locations. Moreover, the limitations of obtaining this sign in real-world critically ill patients are many and include coexistence of subcutaneous emphysema, pleural calcifications, thoracic dressings, and obese body habitus. Although this study proved a good correlation of ultrasonographic examination compared with