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Pulmonary surfactant and cardiopulmonary bypass in infants

To the Editor:

We read with interest the recent article by Paul and colleagues¹ describing changes in pulmonary surfactant after cardiopulmonary bypass (CPB) in a group of infants having surgery for congenital heart disease.

In this article the authors report concentrations of phospholipid and, indirectly, protein in returned fluid from tracheal lavage. There is no apparent attempt to correct concentrations for variable recovery of epithelial lining fluid in these specimens. To define the concentration of surfactant components in sampled secretions, a marker of dilution should be used, allowing the result to be expressed as concentration in epithelial lining fluid.² Results expressed as concentrations in raw lavage fluid are impossible to interpret meaningfully.

Paul and colleagues do report the phospholipid/protein ratio of tracheal lavage specimens. This ratio does nothing to clarify the data and certainly cannot be interpreted as an attempt to correct for dilution, given their later statement that alveolar protein concentration is known to be increased after CPB. A useful marker of dilution of epithelial lining fluid must not be present in increased concentration in the damaged lung. For this reason protein (along with albumin and sphingomyelin) is not suitable in this population.³

Paul and colleagues report a significant fall in total phospholipid concentration immediately after CPB. In their discussion they state: "Our data support the findings of McGowan and colleagues, who demonstrated an alteration in surfactant composition in older infants and children after CPB." In fact, these findings are at odds with those of McGowan and colleagues,⁴ who found no difference in total phosphatidylcholine recovered by bronchoalveolar lavage before and after CPB. In the other published study looking at phospholipid after CPB in children, LeVine and colleagues⁵ showed no difference in phosphatidylcholine levels between a group of children who had undergone CPB and a control

group. Both of these studies involved greater numbers of patients having CPB than in that of Paul and colleagues, and both are also subject to the same criticism of not appropriately correcting results for dilution.

There may well be significant abnormalities of pulmonary surfactant that contribute to postoperative lung dysfunction in this patient population. McGowan and colleagues⁴ did find a change in the proportion of phospholipid in pulmonary surfactant subtypes after CPB (a measurement not influenced by dilution of specimens), which would have important functional implications. This subject warrants further investigation, but care must be taken to express findings in a way that will add to our understanding of the consequences of CPB on the composition and function of pulmonary surfactant.

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12/8/102476

Reply to the Editor:

We thank Millar and colleagues for their interest in our article, "The Role of Cardiopulmonary Bypass and Surfactant in Pulmonary Decompensation after Surgery for Congenital Heart Disease."¹ We concur that our data have certain limitations and must be interpreted with a degree of caution. Although we did not account for the variable recovery of epithelial lining fluid by using a marker of dilution, we made every effort to standardize the timing and technique of the lavage fluid with each procedure. Although a measurement of dilution may have been helpful, even this method has potential limitations² and, as mentioned in the letter, our method is similar to that used in other studies in children undergoing bypass for congenital heart disease.^{3,4} Millar and colleagues also pointed out the potential limitations to using total protein as the denominator. However, as stated in our conclusions, we

believe the decline in the phospholipid/protein ratio seen in infants receiving bypass is indicative of lung injury. Finally, by stating that our data support the findings of McGowan and colleagues,³ we did not mean to imply that we found the same alteration in surfactant composition that was shown in this investigation. We were able to demonstrate a quantitative difference in surfactant, whereas McGowan and associates showed a qualitative difference, albeit in a different patient population.

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Complete resection: Yes or no?

To the Editor:

We read with great interest the article titled "Lobe-Specific Extent of Systematic Lymph Node Dissection for Non-Small Cell Lung Carcinomas According to a Retrospective Study of Metastasis and Prognosis" by Asamura and associates (*J Thorac Cardiovasc Surg* 1999;117:1102-11). The authors mentioned that subcarinal lymphadenectomy is not always necessary for tumors of the right upper lobe and left upper segment. Their argument is based on the retrospective analysis of their data, which consists of more than 166 cases. However, we question whether these resections can be classified as "complete resection."

The definition of "complete resection" in the management of non-small cell lung cancer is not uniform in the literature. The definition provided by Mountain¹ has been widely accepted in the Western world. In this definition, resection can be considered complete when the highest station sampled at thoracotomy is tumor free and extranodal disease is not detected in any of the mediastinal nodes. According to the definition provided by the National Cancer Center in Japan,² all the mediastinal stations where tumor can spread

should be removed. Thus more radical node dissection is required to perform a complete resection according to the Japanese definition.

Surgical resection can be beneficial only if the resection is complete. A clearly defined "complete resection" is mandatory, not only as a selection criterion for surgery but also for the postoperative classification of the patients. Otherwise, it is impossible to detect the positive effect of surgery in the different stages of the disease. We wonder whether Asamura and associates would consider any resection to be complete without examining all mediastinal nodes.

Asamura and associates argued that subcarinal lymph node dissection is not necessary for tumors localized to the right upper lobe and upper division of the left upper segment because single-station metastases to station 7 are rare. We wonder how the authors can detect multiple-station metastases, 37% (20/54) of which occur on the right and 50% (17/34) on the left, without examining all mediastinal stations. They reported that tumor spread to station 7 was detected in 12% to 13% of their patients. We think that regardless of multiple-station or single-station disease, this is too high a percentage to be neglected. Thus, despite their conclusion, their data indicate that subcarinal lymphadenectomy should be performed, which is a relatively easy procedure during thoracotomy.

A new definition for "complete resection" is urgently needed. We agree with the authors that re-evaluation of the mediastinal dissection on the basis of the data collected may be necessary, but the concept of lung resection for non-small cell lung cancer should not be changed without the definition of complete resection being revised. We argue that lung resection for non-small cell lung cancer without systematic lymph node dissection should not be performed until a new definition is provided.

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Reply to the Editor:

We appreciate the interest expressed by Kutlu, Sayar, and Metin in our consideration of the lobe-specific extent of systematic lymph node dissection for lung cancer.¹ This has been a point of discussion among thoracic surgeons for a couple of decades.

Their question can be summarized as follows: Can a pulmonary resection be regarded as a "complete resection" if some mediastinal nodes are not examined? Indeed, we have