Reviewer’s report

Title: Thin-section computed tomography-determined usual interstitial pneumonia pattern affects the decision-making process for resection in newly diagnosed lung cancer patients: a retrospective study

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Reviewer: Teng Moua

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Ando and colleagues are commended for exploring the contribution of radiologic fibrosis to the surgical management of patients with resectable lung disease. They reviewed consecutive patients with newly diagnosed lung cancer, excluded those who could not undergo resection (stage IIIB or higher), then stratified patients by the presence of fibrosis on CT and characterized predictive features that may have lead to resection in those with resectable disease.

This may indeed represent a real world problem confronted by multidisciplinary team members, from the pulmonologist to the oncologist to the surgeon. I suspect the underlying rationale for pursuing the study is to ultimately provide rationale for why fibrosis may influence surgical management and assess the risk or consequence of fibrosis as it contributes to tolerating resection in the first place or adding to postoperative complications, which unfortunately was not presented or discussed at all in this manuscript (I suspect the authors may be reviewing this material for a future manuscript). In all, the manuscript is well written, defines the study population and stratification strategies, collated parameters, statistical comparisons, and related conclusions. There are several recommended revisions that may further serve to improve the manuscript.

1) While the combination of CPFE with lone fibrosis was done for the purposes of this study, indeed the prognosis in general of patients with combined disease (emphysema and lung fibrosis) may be worse than either one alone. It would appear that only 10 pts had lone fibrosis (74 with any fibrosis, 64 with CPFE, leaving 10 with lone fibrosis). DLCO is suspected to be lower in those with CPFE compared to lone fibrosis or lone emphysema, which may influence further decision making on whether resection is feasible (among CPFE, spirometry may often be normalized and therefore falsely reassuring). It is curious to note though that there was greater absence of DLCO measurements among those not offered surgery, can the authors speculate on why that is? Was it because they were not considered surgical candidates based on imaging along and therefore further preoperative assessment such as DLCO was not performed, or were their DLCO's less obtainable or reliable and therefore not available for that reason? Were such patients also more hypoxemic at diagnosis requiring maintenance oxygen support, which may make DLCO indeterminate?
2) In the statistical analysis, multivariable logistic regression was performed and included variables 'based on existing knowledge of the factors for proposing thoracic surgery'. For the multivariable analysis, what variables were used for adjustment? Were these selected a priori, or selected among those with positive P values with some form of addition, elimination, or stepwise approach? How was the model determined? It is difficult to tell for example that 'age' was independently predictive as no specific adjustment variables were provided (predictive after correction for all of them, or some of them?). The authors should clarify this further.

3) As mentioned in comment 1), I suspect presenting spirometry and DLCO likely contributed to the decision making in whether surgery was performed. Noting that many who did not undergo DLCO were not offered resection, was DLCO% predictive of whether surgery would occur or not (Table 4)?

4) Would rename the title, as the way it is written, a conclusion that UIP CT pattern affects decision-making is not actually correct based on the methods presented (decision-making cannot be assessed retrospectively, an event or finding can be predicted, but not the decision-making that led to it as this was not actually studied nor can it be studied in retrospect given the number of possibilities leading to anyone decision). Should also rename Table 4) as 'Univariable and multivariable predictors of surgical resection'. Retrospective studies assessing 'decision-making' are often by their very nature prone to confounding and bias, as causation can never be determined retrospectively. It would be more appropriate to describe factors or predictors of surgical resection than refer to any sort of decision-making process. The authors should revise any text in the paper that suggests the study is assessing decision-making but instead suggest the assessment of differences between those offered resection compared to those not, among only those that were potentially eligible. The authors should describe in their limitations paragraph of the Discussion section (second to last paragraph) that as a retrospective study, not all factors could be reasonably obtained and studied to decipher the greater likelihood of surgery (for example, active smoking history, which may delay or defer surgery due to concern for wound healing complications, or patient deferral due to severity of clinical illness (more dyspnea, cough, or poor weight), deconditioning, or any other reason for why surgery was not pursued).

5) As mentioned in the preface above, the addition of data on surgically related complications would shed significant light on the importance of these findings. The authors make a tentative conclusion that fibrosis appears to influence decision-making in terms of offering perhaps less surgical resection to patients with fibrosis as compared to COPD or normal patients. While statistically this difference is noted, the smaller number of eligible patients for resection in the fibrotic group were still resected greater than 50% of the time. What happened to these patients compared to the normal and lone emphysema group? Were there more postoperative complications, increased exacerbation of underlying ILD, poor wound healing, worse immediate survival, greater recurrence of cancer disease, incomplete resection, etc. It would appear that clinicians chose not to perform surgery more often in those with fibrosis, yet with all other things being
considered, were they right to do so? (because indeed, those patients really did have worse perioperative morbidity and surgery did not offer any survival benefit)

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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