

## Peer Review File

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### Reviewer A

#### Comment 1

Concerning the methodology: The description of the M and M is good, no major concerns. Can you give more details of the utilization of ICG by thoracotomy? With a MI approach ICG is “easy” to use, but with an open approach It’s quite more difficult, and you need to protect people in the OR from the laser source and light.

**Reply 1:** We thank the reviewer for this valuable comment. When we perform surgery via the open approach, ICG marking was performed while the lights in the operating room were dimmed and the open chest wound was covered with gauze or sheets. The camera was inserted through the hole made for placing the drainage tube. We believe that this will provide you with better vision of the demarcation line and protect other individuals in the operating room. We added the following sentence in the Methods section.

#### Change in the text:

Page 7, line 119: “When thoracotomy was performed, we performed ICG detection while the lights in the operating room were dimmed and the open chest wound was covered with gauze or sheets.”

#### Comment 2

Line 134 “Although lobectomy was traditionally performed”. As you have mentioned with the ref no 18, A limited resection of the sequestered lung is mandatory in order to preserve lung function. It’s been a “long time” that we try to perform the tailored and limited resection for this disease.

**Reply 2:** Thank you for highlighting this. We completely agree with your statement; therefore, we deleted the word “traditionally” in the Discussion section.

#### Change in the text:

Page 11, line 181: “Although lobectomy is performed to ensure complete resection of the sequestered lung...”

#### Comment 3

Line 162 when you reported the difference concerning the hospital length of stay, it’s impacted by the surgical approach more than by the use of ICG. You said “it should be

taken into account”, but it’s a result reported in “all article” comparing MI Surgery and open surgery.

**Reply 3:** We thank the reviewer for this opinion. According to the other reviewer’s suggestion, we performed additional subgroup analysis to compare six patients who underwent segmentectomy using ICG with five patients who under segmentectomy without the use of ICG. The results were similar to those presented in Table 3. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (median 5 days vs. 10 days;  $P = 0.03$ ). However, a significant difference in the surgical approach was observed between the two groups, and we considered that this difference significantly contributed to the difference in the postoperative hospital stay. Moreover, the difference in the surgical approach may depend on the surgical era. According to our response to Comment 2 of Reviewer G, our analysis and results were immature; therefore, we reported this as a brief report. We added the forementioned results and comments in the Results and Discussion sections with Supplementary Table S1. Furthermore, we added the limitation of this study in the Discussion section as follows.

**Change in the text:**

Page 10, line 171: “Furthermore, we performed additional subgroup analysis to compare patients who underwent segmentectomy using ICG ( $n = 6$ ) with those who underwent segmentectomy without ICG ( $n = 5$ ) (Supplementary Table S1). The ICG group included significantly more video-assisted thoracic surgeries (83% vs. 0%;  $P = 0.015$ ) than the non-ICG group. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (5 days vs. 10 days;  $P = 0.03$ ).”

Page 13, line 222: “Additionally, we performed subgroup analysis to compare patients who underwent segmentectomy using ICG with those who underwent segmentectomy without the use of ICG (Supplementary Table S1). However, the results of the subgroup analysis were similar to those presented in Table 3. We considered that the difference in the surgical approach significantly contributed to the difference in postoperative hospital stays. Moreover, the difference in the surgical approach may majorly depend on the surgical era. Therefore, further investigation of more cases using ICG in the modern era is necessary.”

Page 15, line 253: “A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

#### **Comment 4**

Do you have any data about the specific cost of the utilization of ICG for these procedures?

**Reply 4:** In this study, we did not analyze the cost of ICG. However, the cost of ICG (Diagnogreen for injection®) that we usually use is inexpensive (534 yen  $\doteq$  3.4 euro).

**Change in the text:** No change.

#### **Reviewer B**

##### **Comment 1**

The authors should have clarified to the reader that the results in Figure 3 include historical comparisons. Also, if a comparison was to be made, the authors should have described a comparison of 6 patients who underwent segmentectomies using ICG and 5 patients who had a sublobar resection without the use of ICG.

**Reply 1:** We thank the reviewer for this opinion. According to the other reviewer's suggestion, we performed additional subgroup analysis to compare six patients who underwent segmentectomy using ICG with five patients who under segmentectomy without the use of ICG. The results were similar to those presented in Table 3. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (median 5 days vs. 10 days;  $P = 0.03$ ). However, a significant difference in the surgical approach was observed between the two groups, and we considered that this difference significantly contributed to the difference in the postoperative hospital stay. Moreover, the difference in the surgical approach may depend on the surgical era. According to our Reply to Comment 2 of Reviewer G, our analysis and results were immature; therefore, we reported this as a brief report. We added the aforementioned results and comments in the Results and Discussion sections with Supplementary Table S1. Furthermore, we added the limitation of this study in the Discussion section as follows.

##### **Change in the text:**

Page 10, line 171: "Furthermore, we performed additional subgroup analysis to compare patients who underwent segmentectomy using ICG (n = 6) with those who underwent segmentectomy without ICG (n = 5) (Supplementary Table S1). The ICG group included significantly more video-assisted thoracic surgeries (83% vs. 0%;  $P = 0.015$ ) than the non-ICG group. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (5 days vs. 10 days;  $P = 0.03$ )."

Page 13, line 222: “Additionally, we performed subgroup analysis to compare patients who underwent segmentectomy using ICG with those who underwent segmentectomy without the use of ICG (Supplementary Table S1). However, the results of the subgroup analysis were similar to those presented in Table 3. We considered that the difference in the surgical approach significantly contributed to the difference in postoperative hospital stays. Moreover, the difference in the surgical approach may majorly depend on the surgical era. Therefore, further investigation of more cases using ICG in the modern era is necessary.”

Page 15, line 253: “A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

## **Comment 2**

The authors described the border between normal lung and lesion as "boundary," "demarcation line," "plane," etc. It would have been desirable to unify these terms.

**Reply 2:** Thank you for highlighting this. In response to the reviewer’s suggestion, we have used the word “boundary” here.

### **Change in the text:**

We changed following all words of “demarcation line” and “plane” into “boundary” in the manuscript.

Page 2, line 28: the boundary between normal...

Page 3, line 39: visualize the boundary between normal...

Page 3, line 46: delineating boundaries with...

Page 5, line 73: visualization of the boundary between the normal...

Page 7, line 117: the boundary between normal...

Page 8, line 137: visualize the boundary between normal...

Page11, line 197: The boundary between normal...

Page12, line 200: identification of the boundary using...

## **Comment 3**

The authors should have specifically indicated "sequestration complications" in the discussion section. Does this indicate a specific symptom?

**Reply3:** No, we just only wanted to express the recurrence of the lesion. We modified the sentence as follows to prevent the reviewers and readers from being confused.

**Change in the text:**

Page 11, line 191: “whereas incomplete resection of the sequestered lung may cause disease recurrence.”

**Comment 4**

The authors stated "no sequestered lungs were left behind". Is there any support by pathological examination for the fact that no lesions remained?

**Reply4:** Yes, we confirmed that each lesion was completely resected based on the pathological examination. Therefore, we added the following sentence in the Results section. However, according to our response to Comment 4 of Reviewer G, we could not evaluate postoperative CT images and perform a long follow-up. Therefore, we cannot answer whether the sequestered lung was completely resected. We added the limitation of this study in the Discussion section as follows.

**Change in the text:**

Page 9, line 156: “The pathological examination confirmed that each lesion was completely resected.”

Page 15, line 256: “Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease.”

**Reviewer C**

**Comment 1**

I would like to thank the colleague for this extraordinarily good work. However, the use of robots is steadily increasing for many indications, including the resection of pulmonary sequestration. I will recommend the author to mention this in the discussion.

**Reply1:** Thank you for your suggestion. According to the reviewer’s suggestion, we have added the following sentences in the Discussion section.

**Change in the text:**

Page 12, line 203: “Furthermore, Kim et al. reported a case of intralobar PS and demonstrated the usefulness of ICG in distinguishing between normal and sequestered lungs in robotic sublobar resection (12).”

Page 14, line 250: “Furthermore, overlay images are feasible and useful in robotic surgery (12), and the expansion of its indication for PS is expected along with the spread of robotic surgery.”

## **Comment 2**

In addition, it is not clear why most patients underwent thoracotomy, size alone can not be the reason as we can read in the literature. It will therefore be important to highlight where the open approach is appropriate.

**Reply2:** Thank you for sharing this perspective. The most likely reason is the surgical era. As described in Table 1, open thoracotomy was performed in 12 patients. Among them, nine underwent surgery before 2015, whereas the remaining three patients underwent surgery after 2016. The reason for thoracotomy of the three patients after 2016 was medical history of thoracic operation for funnel chest, comorbid lung cancer, and size of >8.0 cm. According to the reviewer's viewpoint, we have added the following statement in the Methods section to highlight where the open approach is appropriate.

### **Change in the text:**

Page 6, line 108: "Open thoracotomy was considered if the lesion was large or intense adhesion was expected because of a history of previous thoracic surgery or inflammation."

## **Reviewer D**

**Comment:** I would suggest to report also different surgical techniques of resetting PS like VATS and robotic approach.

**Reply:** Thank you for your suggestion. According to the reviewer's suggestion, we have added the reference and following sentences in the Discussion section.

### **Change in the text:**

Page 12, line 203: "Furthermore, Kim et al. reported a case of intralobar PS and demonstrated the usefulness of ICG in distinguishing between normal and sequestered lungs in robotic sublobar resection (12)."

Page 14, line 250: "Furthermore, overlay images are feasible and useful in robotic surgery (12), and the expansion of its indication for PS is expected along with the spread of robotic surgery."

## **Reviewer E**

### **Comment 1**

All 6 cases utilizing ICG are reported to have undergone segmentectomy. However, in the patients and methods section, it seems that after dissecting the aberrant artery, the authors used ICG to mark the demarcation line, and then performed a resection of the sequestered

lung. Would this not equate to a wedge resection? I believe that a clear distinction between segmentectomy and wedge resection should be made in the context of sequestered lung resection.

**Reply1:** We thank the reviewer for the careful suggestion. We perform segmentectomy, indicating that ICG administration was performed after dissecting the aberrant artery and segmental vein. Those were mainly segmentectomy of S10 (n = 8), which was similar to lesion resections. However, in cases in which the size of the lesion was large, basal segmentectomy was performed (n = 3), which included both lesion and normal segments. We added and modified the following sentences in the Methods section. Furthermore, we revised Tables 1 and 3.

**Change in the text:**

Page 7, line 114: “Furthermore, the veins that drained the sequestered lungs were also dissected. After dissection of the aberrant arteries and veins, an anesthesiologist administered ICG at a dose of 5.0–7.5 mg/body.”

Page 7, line 123: “If ICG was not used, the dissection line was determined macroscopically.”

**Comment 2**

Over time after administration, ICG may seep into the resected side. If marking is performed at this infiltrated location, there is a concern of potential incomplete resection. In these 6 cases, how long after the ICG administration did the staining start and at what timing was the marking done?

**Reply2:** Thank you for this thoughtful comment. ICG staining starts usually approximately 10–15 s after ICG administration; subsequently, we immediately mark the demarcation line using electrocautery. We could confirm and mark the demarcation line immediately in all six cases in this study. However, we sometimes cannot identify the line using ICG in segmentectomy for lung cancer even if we wait for a long time after administration or the repeat use of ICG. As we described in the Discussion section, we believe that these results are related to disease-specific features, such as the high prevalence of PS in young nonsmokers. We have added the following sentences in the Discussion section to convey the important point that the reviewer stated.

**Change in the text:**

Page 14, line 242: “On the other hand, we should immediately mark considering that ICG may seep into the resected side over time after administration.”

**Comment 3**

In addition to Pryce type III, in which the aberrant artery flows only into the sequestered lung, there exists Pryce type II, where the aberrant artery also flows into the normal lung. Were there any cases of Pryce type II? If there were, was ICG able to accurately delineate between the sequestered and normal lungs?

**Reply3:** Thank you for your question. In our study, we have no case of Pryce type II. We added the following sentence in the Methods section.

**Change in the text:**

Page 5, line 90: “All intralobar PS cases were diagnosed as Pryce type III based on the findings of preoperative images.”

**Comment 4**

The boundary between the sequestered lung and the normal lung can often be confirmed visually without the use of ICG. Among the 17 cases, in how many was this boundary visible? Including the frequency of this occurrence would likely enhance the perceived utility of this method.

**Reply4:** Thank you for your thoughtful comment. As you stated, boundaries can be sometimes seen without the use of ICG. However, they are often ambiguous, and we believe that ICG is better than the naked eye when determining the line to be cut. We retrospectively checked and analyzed the operation reports and intraoperative images. The results were as follows: visible in 4 cases, invisible in 8 cases, and difficult to judge in 5 cases. We added these results in the Results section.

**Change in the text:**

Page 8, line 133: “Although the boundary between normal and sequestrated lungs was confirmed visually without the use of ICG in four patients (24%), it was unclear or difficult to judge in the remaining 13 patients.”

**Comment 5**

The authors argue that although ICG is rapidly washed out, repeated use can resolve this issue. However, they do not clearly discuss the basis for this argument. It should be clarified exactly how long it takes for ICG to be washed out.

**Reply5:** We thank the reviewer for this valuable comment. Misaki et al. reported the details of a method for visualizing adjacent lung segments using ICG in segmentectomy for eight patients (Misaki N, et al. *J Thorac Cardiovasc Surg.* 2010;140:752-6). They reported that the area with a normal blood supply was stained within 13 s (range, 8–18 s) and the maximum staining intensity was attained 28 s (range, 20–33 s) after the injection



of ICG. According to the reviewer's comment, we added the aforementioned statement and reference in the Discussion section.

**Change in the text:**

Page 14, line 237: "Misaki et al. reported that the area with a normal blood supply was stained within 13 s (range, 8–18 s) and that the maximum staining intensity was attained 28 s (range, 20–33 s) after the injection of ICG in segmentectomy for eight patients (11)."

**Comment 6**

What kind of pitfalls were there for preoperative interventions? A more detailed description would be appreciated.

**Reply6:** Thank you for your question. As mentioned in the manuscript, we reported the pitfall of the hybrid surgery for intralobar PS (Nakanishi K, et al. Surg Case Rep. 2021;7:192). In the case report, we experienced that the aberrant artery was challenging to dissect using an energy device because of an intravascular coil placed by preoperative intervention. In conclusion, we stated that where and how to dissect the aberrant artery should be cautiously determined based on preoperative images, while considering the presence of an intravascular coil, when you perform preoperative coil embolization for intralobar PS. According to the other reviewer's comment, we deleted the following sentences because these were not the essence of this study.

**Change in the text:**

Page 6, line 101: "Since 2021, preoperative interventions, such as coil embolization of aberrant arteries, were considered only in cases with a history of infection or when aberrant arteries of a certain size originating from the abdominal aorta were identified in preoperative computed tomography (CT), based on experience at our institution (4)."

Page 9, line 148: "However, we experienced pitfalls associated with this technique (4), and its use at our institution was limited after 2021."

**Reviewer F**

**Comment 1**

Line 86 – 87 "After dissection of the aberrant arteries, 5.0–7.5 mg/body of ICG was administered by an anesthesiologist."

This description indicates that the method is primarily for delineating the sequestration lung, but was there ever a case where a segmentectomy was performed that included the sequestration lung area?

In that case, ICG administration would be done after dissecting the segmental artery and vein.

It may be easier to understand if you add a distinction to the method between cases in which only the sequestration lungs are visualized and cases in which the normal segment, including the sequestration lungs, are visualized.

**Reply1:** We thank the reviewer for the careful suggestion. We basically perform segmentectomy, indicating that ICG administration was performed after dissecting the aberrant artery and segmental vein. Those were mainly segmentectomy of S10 (n = 8), which was similar to lesion resections. However, in cases in which the size of the lesion was large, basal segmentectomy was performed (n = 3), which included both lesion and normal segments. We added and modified the following sentences in the Methods section. Furthermore, we revised Tables 1 and 3.

**Change in the text:**

Page 7, line 114: “Furthermore, the veins that drained the sequestered lungs were also dissected. After dissection of the aberrant arteries and veins, an anesthesiologist administered ICG at a dose of 5.0–7.5 mg/body.”

**Comment 2**

About Tables 1, 3 Please provide a detailed Sublobar resection.

**Reply2:** According to the reviewer’s suggestion, we revised Tables 1 and 3.

**Change in the text:**

Table 1.

Type of operation

Basal segmentectomy 3 (18)

S10 segmentectomy 8 (47)

Table 3.

Type of operation Segmentectomy 6 (100%) 5 (46%)

**Comment 3**

Line 160 – 162, "We demonstrated that postoperative hospital stays were significantly shorter when ICG was used compared to hospital stays patients where ICG was not used (5 days vs. seven days; P = 0.037). "

The authors describe the above, although there is room for further consideration.

If this sentence is to be written, it would be better to add some discussion.

**Reply3:** We thank the reviewer for this opinion. According to the other reviewer’s suggestion, we performed additional subgroup analysis to compare six patients who

underwent segmentectomy using ICG with five patients who under segmentectomy without the use of ICG. The results were similar to those presented in Table 3. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (median 5 days vs. 10 days;  $P = 0.03$ ). However, a significant difference in the surgical approach was observed between the two groups, and we considered that this difference significantly contributed to the difference in the postoperative hospital stay. Moreover, the difference in the surgical approach may depend on the surgical era. According to our response to Comment 2 of Reviewer G, our analysis and results were immature; therefore, we reported this as a brief report. We added the forementioned results and comments in the Results and Discussion sections with Supplementary Table S1. Furthermore, we added the limitation of this study in the Discussion section as follows.

**Change in the text:**

Page 10, line 171: “Furthermore, we performed additional subgroup analysis to compare patients who underwent segmentectomy using ICG ( $n = 6$ ) with those who underwent segmentectomy without ICG ( $n = 5$ ) (Supplementary Table S1). The ICG group included significantly more video-assisted thoracic surgeries (83% vs. 0%;  $P = 0.015$ ) than the non-ICG group. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (5 days vs. 10 days;  $P = 0.03$ ).”

Page 13, line 222: “Additionally, we performed subgroup analysis to compare patients who underwent segmentectomy using ICG with those who underwent segmentectomy without the use of ICG (Supplementary Table S1). However, the results of the subgroup analysis were similar to those presented in Table 3. We considered that the difference in the surgical approach significantly contributed to the difference in postoperative hospital stays. Moreover, the difference in the surgical approach may majorly depend on the surgical era. Therefore, further investigation of more cases using ICG in the modern era is necessary.”

Page 15, line 253: “A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

**Comment 4**

Line 165- 166 “Pulmonary intersegmental plane identification using intraoperative ICG is simple, safe, and inexpensive.”

ICG systems are becoming more common, but many institutions may not be able to get them because of their price. Is “expensive” an accurate word?

**Reply4:** We partially agree with your comment. As you stated, it is true that near-infrared thoracoscopy systems are expensive. This had been written in the Discussion section (Page 15, line 252). However, the running cost is not expensive once your institution purchased the aforementioned system because the cost of ICG (Diagnogreen for injection®) itself that we usually use is inexpensive (534 yen  $\doteq$  3.4 euro).

**Change in the text:** No change.

#### **Comment 5**

Is ICG currently used in all cases as a basic policy?

**Reply5:** Thank you for your question. Yes, we basically use ICG when performing segmentectomy for patients with small-size lung cancer or intralobar PS.

**Change in the text:** No change.

#### **Comment 6**

Was the ICG-determined incision line more consistent with pathology than the grossly determined incision line?

**Reply6:** We thank the reviewer’s thoughtful comment. At first, we confirmed that each lesion was completely resected based on the pathological examination. Therefore, we added the following sentence in the Results section. Furthermore, according to our response to Comment 4 of Reviewer E, we retrospectively checked the operation reports and intraoperative images to review how often we could grossly determine incision lines. The results were as follows: visible in 4 cases, invisible in 8 cases, and difficult to judge in 5 cases. Therefore, we believe that ICG is better than the naked eye when determining the line to be cut. We added these results in the Results section.

**Change in the text:**

Page 9, line 156: “The pathological examination confirmed that each lesion was completely resected.”

Page 8, line 133: “Although the boundary between normal and sequestered lungs was confirmed visually without the use of ICG in four patients (24%), it was unclear or difficult to judge in the remaining 13 patients.”

#### **Comment 7**

Table 3, please describe the Maximal size of the aberrant artery

**Reply7:** As recommended by the reviewer, we have revised Table 3.

**Change in the text:**

Table 3.

Maximal size of aberrant artery, mm Median (range) 5 (2–9) 5 (2–10) 1

**Reviewer G****Comment 1**

Structure of the abstract should be improved. Background (what is known or unknown), methods, results and conclusion should be clearly divided.

**Reply1:** Thank you for your viewpoint. However, this paper was submitted to the journal as a brief report, not original report. Thus, we made an unstructured Abstract according to the journal's submission guidelines.

**Change in the text:** No change.

**Comment 2**

What is the main purpose of this study? Is it the usefulness of ICG for intralobar PS? If you try to show the efficacy of ICG for PS by comparing to the cases who underwent surgery without ICG, the number of cases is insufficient for statistical analysis.

**Reply2:** Thank you for your comment. We totally agree with your opinion. This analysis and the results were immature; therefore, this report was submitted as a brief report. We have revised our Abstract and Conclusion to temper the interpretation of our findings as follows. Additionally, we added the limitations of this study in the Discussion section as follows.

**Change in the text:**

Page 3, line 50: "We suggested that this technique was..."

Page 15, line 262: "we suggested the..."

Page 15, line 253: "A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique."

**Comment 3**

In addition to major concern #2, the efficacy of ICG was overrated in table 3. The difference of approach of operation may be majorly dependent of the era of the surgery.

The difference of type of operation depends on the necessity of ICG because the use of ICG is not required for lobectomy, leading to the difference of postoperative length of hospital stay. Therefore, I cannot see any preferable results with ICG group from table 3.

**Reply3:** We thank the reviewer for this opinion. According to the other reviewer's suggestion, we performed additional subgroup analysis to compare six patients who underwent segmentectomy using ICG with five patients who under segmentectomy without the use of ICG. The results were similar to those presented in Table 3. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (median 5 days vs. 10 days;  $P = 0.03$ ). However, a significant difference in the surgical approach was observed between the two groups, and we considered that this difference significantly contributed to the difference in the postoperative hospital stay. Moreover, the difference in the surgical approach may depend on the surgical era. According to our response to Comment 2 of Reviewer G, our analysis and results were immature; therefore, we reported this as a brief report. We added the forementioned results and comments in the Results and Discussion sections with Supplementary Table S1. Furthermore, we added the limitation of this study in the Discussion section as follows.

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Page 13, line 222: "Additionally, we performed subgroup analysis to compare patients who underwent segmentectomy using ICG with those who underwent segmentectomy without the use of ICG (Supplementary Table S1). However, the results of the subgroup analysis were similar to those presented in Table 3. We considered that the difference in the surgical approach significantly contributed to the difference in postoperative hospital stays. Moreover, the difference in the surgical approach may majorly depend on the surgical era. Therefore, further investigation of more cases using ICG in the modern era is necessary."

Page 15, line 253: "A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine

patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

**Comment 4**

How did you decide the accurate boundary between the normal lung and PS? How did you determine whether no sequestered lung was left? I reckon that whether sequestered lung is completely resected or not depends on the evaluation of postoperative CT scan and the confirmation of no relapse of PS in the long follow-up.

**Reply4:**

We thank this reviewer’s thoughtful comment. At first, we confirmed that each lesion was completely resected based on the pathological examination. Therefore, we added the following sentence in the Results section. However, according to our response to Comment 4 of Reviewer G, we could not evaluate postoperative CT images and perform a long follow-up. Therefore, we cannot answer whether the sequestered lung was completely resected. We added the limitation of this study in the Discussion section as follows.

**Change in the text:**

Page 9, line 156: “The pathological examination confirmed that each lesion was completely resected.”

Page 15, line 256: “Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease.”

**Comment 5**

I doubt that the demarcation line distinguished by ICG is always correct. Intralobar PS is divided into several categories according to Pryce’s classification. Since sequestered lung is fed not only from abnormal arteries but from normal PA in type II, the demarcation line created using ICG after the resection of abnormal arteries may be underestimated for this type of PS.

**Reply5:** Thank you for your comment. In our study, we have no case of Pryce type II. We added the following sentence in the Methods section.

**Change in the text:**

Page 5, line 90: “All intralobar PS cases were diagnosed as Pryce type III based on the findings of preoperative images.”

**Comment 6**

Line 50: please insert appropriate papers related to your background.

**Reply6:** Thank you for your viewpoint. We added the following two references in the Background section.

**Change in the text:**

Page 4, line 66: “(5, 6).”

5. Leoncini G, Rossi UG, Ferro C, et al. Endovascular treatment of pulmonary sequestration in adults using Amplatz® vascular plugs. *Interact Cardiovasc Thorac Surg* 2011;12:98-100.

6. Hewett L, Kwon J, Adams JD, et al. Intralobar pulmonary sequestration with aneurysmal feeding vessel: use of hybrid surgical management. *Ann Thorac Surg*. 2016;102:e533-5.

**Comment 7**

Line 52-53: This sentence is unnecessary because this study is not associated to lung surgery for lung cancers.

**Reply7:** According to the reviewer’s comment, we deleted the following sentence and references.

**Change in the text:**

Page 4, line 69: “Sublobar resection for early-stage lung cancer, including segmentectomies and wedge resection, increased following the JCOG0802/CALGB140503 trial (5, 6).”

**Comment 8**

Line 53-55: You mentioned various methods for identifying segments, but only one article is cited.

**Reply8:** Thank you for your viewpoint. We added the following reference in the Background section.

**Change in the text:**

Page 4, line 70: “(7, 8).”

8. Okada M, Mimura T, Ikegaki J, et al. A novel video-assisted anatomic segmentectomy technique: selective segmental inflation via bronchofiberoptic jet followed by cautery cutting. *J Thorac Cardiovasc Surg* 2007;133:753-8.

**Comment 9**

Patients and methods: please clarify IRB number in your hospital.

**Reply9:** According to the reviewer’s comment, we added the following sentence in the Methods section.



**Change in the text:**

Page 5, line 85: “The study was approved by the Institutional Review Board of Nagoya University Hospital (approval No. 2020-0375). Individual consent for this retrospective analysis was waived.”

**Comment 10**

Patients and methods: there is no statistical information.

**Reply10:** Statistical information was described in the Methods section.

**Change in the text:**

Page 6, line 100: “The Mann–Whitney U test and Fisher’s exact test were used to compare continuous and categorical variables, respectively, between the groups. For all analyses, *P*-values < 0.05 were used to denote statistical significance. All statistical analyses were performed using SPSS version 25.0 (IBM Corporation, Armonk, NY).”

**Comment 11**

Line 77-79 and line 109-110: I could not fully understand the importance of the information about the preoperative interventions in these parts. Did you mean that you routinely performed preoperative coil embolization for PS until PS? What are pitfalls of the procedure?

**Reply11:** Thank you for your question. As mentioned in the manuscript, we reported the pitfall of the hybrid surgery for intralobar PS (Nakanishi K, et al. Surg Case Rep. 2021;7:192). In the case report, we experienced that the aberrant artery was challenging to dissect using an energy device because of the presence of an intravascular coil placed by preoperative intervention. According to the reviewer’s comment, we deleted the following sentences because these are not the essence of this study.

**Change in the text:**

Page 6, line 101: “Since 2021, preoperative interventions, such as coil embolization of aberrant arteries, were considered only in cases with a history of infection or when aberrant arteries of a certain size originating from the abdominal aorta were identified in preoperative computed tomography (CT), based on experience at our institution (4).”

Page 9, line 148: “However, we experienced pitfalls associated with this technique (4), and its use at our institution was limited after 2021.”

**Comment 12**

Line 84: how did you determine the exact lesion of PS?

**Reply12:** Based on the preoperative CT images, we measured the size of the mass and cystic changes, as described in Figure 1.

**Change in the text:** No change.

## **Reviewer H**

### **Comment 1**

The author states that preoperative interventions were considered for aberrant arteries of a certain size. What exactly is a certain size?

**Reply1:** Thank you for your question. Although hybrid surgery with preoperative coil embolization was performed for an aberrant artery of 3.5 mm (He B, et al. *Ann Vasc Surg.* 2020;69:447), the precise cutoff diameter of the aberrant artery for hybrid surgery remains unclear. We deleted the following sentence because it is not the essence of this study.

### **Change in the text:**

Page 6, line 101: “Since 2021, preoperative interventions, such as coil embolization of aberrant arteries, were considered only in cases with a history of infection or when aberrant arteries of a certain size originating from the abdominal aorta were identified in preoperative computed tomography (CT), based on experience at our institution (4).”

### **Comment 2**

The author state that there was less bleeding in the ICG group, but there is bias in the approach, fewer cases, and no significant difference. It cannot be said that there was less bleeding in the ICG group.

**Reply2:** Thank you for your comment. We totally agree with your opinion. This analysis and the results were immature; therefore, this report was submitted as a brief report. We deleted the following sentence in the Results section. Furthermore, we added the limitation of this study in the Discussion section as follows.

### **Change in the text:**

Page 10, line 170: “Median blood loss in patients with ICG was marginally lower than blood loss in patients who underwent surgery without ICG (5 mL vs. 46 mL;  $P = 0.098$ ).”

Page 15, line 253: “A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

### **Comment 3**

Why does the use of ICG result in shorter hospital stays? The author need to clearly explain why.

**Reply3:** We thank the reviewer for this opinion. According to the other reviewer's suggestion, we performed additional subgroup analysis to compare six patients who underwent segmentectomy using ICG with five patients who under segmentectomy without the use of ICG. The results were similar to those presented in Table 3. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (median 5 days vs. 10 days;  $P = 0.03$ ). However, a significant difference in the surgical approach was observed between the two groups, and we considered that this difference significantly contributed to the difference in the postoperative hospital stay. Moreover, the difference in the surgical approach may depend on the surgical era. According to our response to Comment 2 of Reviewer G, our analysis and results were immature; therefore, we reported this as a brief report. We added the forementioned results and comments in the Results and Discussion sections with Supplementary Table S1. Furthermore, we added the limitation of this study in the Discussion section as follows.

#### **Change in the text:**

Page 10, line 171: "Furthermore, we performed additional subgroup analysis to compare patients who underwent segmentectomy using ICG ( $n = 6$ ) with those who underwent segmentectomy without ICG ( $n = 5$ ) (Supplementary Table S1). The ICG group included significantly more video-assisted thoracic surgeries (83% vs. 0%;  $P = 0.015$ ) than the non-ICG group. The postoperative hospital stay was significantly shorter in the ICG group than in the non-ICG group (5 days vs. 10 days;  $P = 0.03$ )."

Page 13, line 222: "Additionally, we performed subgroup analysis to compare patients who underwent segmentectomy using ICG with those who underwent segmentectomy without the use of ICG (Supplementary Table S1). However, the results of the subgroup analysis were similar to those presented in Table 3. We considered that the difference in the surgical approach significantly contributed to the difference in postoperative hospital stays. Moreover, the difference in the surgical approach may majorly depend on the surgical era. Therefore, further investigation of more cases using ICG in the modern era is necessary."

Page 15, line 253: "A limitation of our study was the small number of patients, and our analysis might have been underpowered to identify differences in several values in comparisons. Furthermore, follow-up data were inadequate, including postoperative CT images because of benign disease. Thus, we must continue to accumulate and examine

patients who undergo surgery for intralobar PS using ICG to determine the efficacy of this technique.”

**Comment 4**

“Median operative times were not significantly different between the two groups (144 mL vs. 159 mL; P = 0.30)”.

Are the units of measure incorrect?

**Reply4:** Thank you very much for your viewpoint. The units of measure was incorrect; therefore, we have revised it.

**Change in the text:**

Page 10, line 169: “144 min vs. 159 min”