PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Epidemiology and Medical Service Use for Spontaneous Pneumothorax: A 12-year Study Using Nationwide Cohort Data in Korea
AUTHORS	Kim, Doori; Jung, boyoung; Jang, Bo-Hyoung; Chung, Seol Hee; Lee, Yoon Jae; Ha, In-Hyuk

VERSION 1 – REVIEW

REVIEWER	T Desmettre university of franche comte, France (EU)
REVIEW RETURNED	30-Jan-2019

GENERAL COMMENTS	Comments on the manuscript entitled "epidemiology and medical serice use for spontaneous pneumothorax: a 12-Year longitudinal study using nationwide cohort data" submitted to BMJ open General comments. In this large national study conducted in Korea, the authors provide data on the epidemiology of spontaneous pneumothorax and its management over a 12-year period, involving 4,658 patients. In the case of a study carried out in Korea, it is necessary to include this concept in the text, in order to make the title more informative As the authors rightly point out, due to the retrospective nature of the study, there are still many uncertainties regarding the data presented with regard to completeness, particularly with regard to undeclared treatments for patient insurance purposes. There are no elements in the discussion or this limit is more detailed, in order to try to assess what proportion of patients this may represent under the conditions of the Korean health system. Another very important limitation is the lack of information about the type of spontaneous pneumothorax, first episode or recurrence, which makes it impossible to evaluate the treatment strategy used in first-line treatment, particularly in the case of a first episode of spontaneous pneumothorax. The reference to a recently accepted study on the management in France of a large series of pneumothorax deserves to be added to the references; the results of the study presented here can be discussed in relation to this study. The authors describe a series of spontaneous pneumothorax, but at no time do they explain how they identified traumatic pneumothorax to exclude them from the analysis. Finally, the authors speak of a "longitudinal cohort" concerning the series presented, but this does not correspond to this definition.

	Comments by section Introduction P4L28/29 "influencing the work productivity of patients": prefer a formula specifying the medico-economic repercussions, as there are, in addition to the economic aspects, physical and psychological sequelae which can be considerable. P4L36/37: Since the study presented by the authors also covers the management performed, the lack of knowledge about the modalities of management and the treatments performed should be highlighted in the introduction. A reference to a recent study on this subject should be added (Kepka S et al, https://doi.org/10.1186/s12873-018-0213-2) and also discussed in relation to the results of the study presented. P4L42/43 : "by the affected patients" a modifier, "for patients admitted for spontaneous pneumothorax" P4L44/45 : "to gain" : to reformulate Methods P7L10/17: "we set the follow duration at 60 days and defined new SP onset as cases in which no medical service use was recorded in the previous 60 days": this definition applied by the authors is not consensual, there are indeed early readmissions for the same episode and recurrences, which do not meet this type of definition which is not the one usually accepted in the literature. P7L42/50 :"" due to a lack..... Of this study " it is a limit of the study that must appear in the discussion by in the methods part in my opinion P7L61/62: "surgical treatments and non surgical treatments": the authors must specify which types of gestures correspond to these definitions in the Methods section The authors do not explain how they identified traumatic pneumohtorax to exclude it from the analysis. Results A flow chart is required to present the overall results. Discussion The various remarks made previously must be taken up, implemented and discussed.

REVIEWER	Hiroaki Ogata Research Institute for Diseases of the Chest, Graduate School of Medical Sciences, Kyushu University, Japan
REVIEW RETURNED	27-Feb-2019

GENERAL COMMENTS	This is a well written paper, which reports the annual prevalence rate and hospitalization treatment rate of spontaneous pneumothorax (SP) in Korea aiming to assess the detailed and objective information about SP. The value of this work is high considering the evolution of trend in SP prevalence and other outcomes in Korea using the nationwide database. Here are some remarks: Major comment 1: Since the discussion concerning the annual prevalence rate of SP was highly descriptive, the authors should discuss more about the

	reason for its increasing trend; it may provide additional contribution to better understanding of SP. Major comment 2: It is better to define primary SP (PSP) and secondary SP (SSP) according to the CCI score, and to investigate the trends in annual prevalence rate and hospitalization treatment rate of PSP and SSP separately as a subgroup analysis. This analysis might help the interpretation of the increase in the annual prevalence rate of SP; it would be likely due to increase in the underlying lung diseases (e.g., COPD) if there was an increasing trend only in SSP, or it would seem to be associated with trends in risk factors for both PSP and SSP (e.g., inhalation of polluted air, smoking, etc.) if both subtypes of SP increased with time. According to the paragraph in page 21, lines 9-18, the authors made efforts to distinguish between PSP and SSP; I think it might be better to mention this point in the limitation part (page 22, lines 12-14). Major comment 3: It would be appreciated if the authors perform some statistical analyses regarding the trends in the annual prevalence rate and hospitalization treatment rate of SP (e.g., logistic or Poisson regression models). It might be even better to perform such analyses with adjustment for age and sex. Minor comment 1: It may be better to fully explain the abbreviation “NSC” in page 5, line 6 rather than page 5, line 11. Minor comment 2: SP follow-up duration was set as 60 days; it may be useful to show the number of SP cases which required any medical service for more than 60 days. Minor comment 3: Page 7, line 11: the abbreviation “HIRA” should be fully explained. Minor comment 4: Page 7, line 13: the abbreviation “NHIS” may be better to be used instead of “National Health Insurance Service”. Minor comment 5: Page 22, lines 19-21: the font size of the sentence “Lastly, as the diagnosis codes in claim data that was the basis for identifying the patients may not completely accurate [40], it is not sure that all patients with SP were listed under the corresponding diagnosis codes” seems to be smaller than other sentences. Minor comment 6: It could be worth evaluating the annual prevalence rate of SP which required some surgical treatment or not, separately; it might contribute to estimation of trend in SP prevalence by the level of severity of SP. Minor comment 7: Since the random sampling was performed using the NHI data in 2002, I think there was a possibility of misclassification of first-time SP cases; there were “first-time” SP patients who ever suffered from SP before 2002. If so, it should be considered as a limitation.

	Minor comment 8: Table 3: the number of COPD-related SP patients aged 15-34 years was similar to that among older populations, although COPD is likely to develop in aged population. It would be useful to explain this issue.

REVIEWER	Michael Goldacre Professor Emeritus of Public Health Nuffield Department of Population Health University of Oxford, UK
REVIEW RETURNED	19-Mar-2019

GENERAL COMMENTS	The authors should reconsider their use of the term 'prevalence rate' throughout the text and tables. As no doubt they know, 'prevalence' is, conventionally, a 'census measure' of the number of people who have the disease at a particular time and, 'when used without qualification, it usually refers to the situation at a specified point in time (point prevalence)' (Dictionary of Epidemiology, International Epidemiological Association, 5th edition). It is not really the right concept for an acute, short-lived condition like pneumothorax, and it is not in fact the measure that the authors show. There are problems, admittedly, in referring to their measure as 'incidence', because, as they discuss (e.g. last bullet-point of 'Strengths and limitations'), there are difficulties in knowing whether the events they describe are the 'first ever' events. However, what the authors show are reasonable proxies for incidence, and maybe this could be specified and the measures termed incidence with the limitations of the term, in this context, discussed. If the authors do not like the term 'incidence' (with qualifications specified), an alternative term could be medical 'service-contact' rates (which is actually what they show). I note that papers cited by the authors (eg their references 5-9) use the term 'incidence', despite its limitations. Also, the authors might like to reconsider their use of the term '12-year longitudinal nationwide cohort study' in the title of the paper and elsewhere. It is only a 'longitudinal cohort' study in the sense that they follow up the patients for a limited period. As the authors say, 'the approximate treatment period for SP was 11 days for outpatients and 17.7 days for hospitalized patients'. It is not a '12-year cohort study', as the title suggests, which might lead readers to assume that they will get data on the 12-year outcomes of patients with pneumothorax. 'A 12-year nationwide study' (or something similar) as a subtitle after their semi-colon is probably a sufficient descriptor. Nonetheless, the data shown are interesting and worthwhile. Some fairly minor points: The authors write that other epidemiological studies of pneumothorax tended to be based on small numbers (e.g. page 21, lines 4-10, which concludes that other cited studies were 'based on small sample sizes (82–273 subjects)'. But the publication by me and my colleagues (their reference 27) was based on over 170,000 cases. A stronger claim than size for originality of the authors' study is the fact that they identified cases treated wholly outside hospital as well as hospitalised cases. I think that I am right in saying that most of the published papers they cite did not do this; and this dimension is interesting.

	Abstract, Objectives: The objectives as specified – 'to analyse…in order to obtain information about…' is tautological and does not really inform the readers why they should be interested in the information sought. Reconsider, and be more specific? Methods. Patient and Public Involvement: The authors' text under this heading is really a description of the dataset, and not what I assume the journal expects on PPI. Comorbidities in SP patients (page 6): The authors specify that diseases were counted '…if they were diagnosed…within 1 year of the first date of SP onset'. Could they please specify that they mean in either the year before or in the year after SP onset (or both). Table 1, data on age, and associated text: one of the striking features of SP is its bimodal age distribution. The grouping of age in Table 1 is too broad to show this, although elsewhere the authors do draw attention to it. It would be good if the authors emphasised the bimodal nature of the age distribution (separately in males and females) as a characteristic of the epidemiology of SP. Table 2: this may be more a matter for the editors than the reviewer, but the layout of the table is clumsy and needs attention.

VERSION 1 – AUTHOR RESPONSE

RESPONSES TO REVIEWER #1's COMMENTS

T Desmettre
Institution and Country
university of franche comte, France (EU)

Dear Dr. T Desmettre

We thank the reviewer very much for the kind words about our paper. We are grateful for the time and energy expended in reviewing our manuscript. Your suggestions were very helpful in improving our paper. In the following sections, you will find our responses to each of your comments and suggestions

1. *In this large national study conducted in Korea, the authors provide data on the epidemiology of spontaneous pneumothorax and its management over a 12-year period, involving 4,658 patients. In the case of a study carried out in Korea, it is necessary to include this concept in the text, in order to make the title more informative*
   - **RESPONSE: Thank you for your insightful suggestion. The editor gave a similar suggestion. We have now added 'in Korean' to the title, Abstract, and other necessary sections in the text**

2. *As the authors rightly point out, due to the retrospective nature of the study, there are still many uncertainties regarding the data presented with regard to completeness, particularly with regard to undeclared treatments for patient insurance purposes. There are no elements in the discussion or this limit is more detailed, in order to try to assess what proportion of patients this may represent under the conditions of the Korean health system.*

- **RESPONSE: We agree with the reviewer's astute opinion. Due to the nature of the health insurance claims data, patients who are not covered by health insurance and data on uncovered treatments were missing. Therefore, the data analyzed in this study were not the total data in the real world. However, in Korea, 98% of the total population is covered by health insurance; it therefore seems to be the entire population data. Uncovered items or indirect medical costs are limitation of this study, and we have added those in the Discussion section (P23L30-P24L3).**

3. *Another very important limitation is the lack of information about the type of spontaneous pneumothorax, first episode or recurrence, which makes it impossible to evaluate the treatment strategy used in first-line treatment, particularly in the case of a first episode of spontaneous pneumothorax. The reference to a recently accepted study on the management in France of a large series of pneumothorax deserves to be added to the references; the results of the study presented here can be discussed in relation to this study*
   - **RESPONSE: We thank the reviewer for the valuable point and we agree with the reviewer. Because of the limitations of the data, it was very difficult to identify recurrence or to distinguish between PSP and SSP. Accordingly, it was also impossible to present each treatment method. We have added the above as well as the paper you mentioned in the Discussion section (How spontaneous pneumothorax is managed in emergency departments: a French multicenter descriptive study)(P23L21)**

4. *The authors describe a series of spontaneous pneumothorax, but at no time do they explain how they identified traumatic pneumothorax to exclude them from the analysis.*
   - **RESPONSE: We thank the reviewer for pointing this out. In KCD-7, spontaneous pneumothorax corresponds to J93 while traumatic pneumothorax corresponds to S27. Analysis was conducted only for those that were diagnosed with J93. We have specified the fact that we excluded S27 in the Methods section.(P5L18-L20)**

5. *Finally, the authors speak of a "longitudinal cohort" concerning the series presented, but this does not correspond to this definition.*
   - **RESPONSE: The reviewer's comment is with deep insight; we thank you. Reviewer 3 also made a similar comment. We have deleted 'longitudinal' from the title, abstract, and manuscripts. (leaving this as "a 12-year nationwide study) (P2L6)**

Comments by section

6. *Introduction*
   P4L28/29 "influencing the work productivity of patients": prefer a formula specifying the medico-economic repercussions, as there are, in addition to the economic aspects, physical and psychological sequelae which can be considerable.
   - **RESPONSE: This is a very valuable suggestion and we agree with you. Of course, although it would be nice to know the extent of socioeconomic losses due to pneumothorax, it was hard to know this through previous studies. We have added the consequences of pneumothorax to emphasize the significance of the condition in the introduction. (P4L13)**

7. *P4L36/37: Since the study presented by the authors also covers the management performed, the **lack of knowledge about the modalities of management and the treatments performed should be highlighted in the introduction**. A reference to a recent study on this subject should be added (Kepka S et al, https://doi.org/10.1186/s12873-018-0213-2) and also discussed in relation to the results of the study presented.*
   - **RESPONSE: Thank you for your kind opinion and for suggesting a good reference. We have added the reference. (P4L16**-L17**)**

8. *P4L42/43 : "by the affected patients" a modifier, "for patients admitted for spontaneous pneumothorax"*
   - **RESPONSE: Thank you for the valuable suggestion. However, we included outpatients as well as inpatients; thus, we used 'treated' instead of 'admitted'. Thank you for your understanding. (P4L21)**

9. *P4L44/45 : "to gain" : to reformulate*
   - **RESPONSE: Thank you for your revision. We have corrected it. (P4L24)**

10. *Methods*
    P7L10/17: "we set the follow duration at 60 days and defined new SP onset as cases in which no medical service use was recorded in the previous 60 days": this definition applied by the authors is not consensual, there are indeed early readmissions for the same episode and recurrences, which do not meet this type of definition which is not the one usually accepted in the literature.
    - **RESPONSE: This is a very important point and we totally agree with you. Defining the new SP onset was one of the biggest challenges of this analysis. We verified f/u consults due to SP through screening analysis and discussed enough with medical experts in Korea. Finally, we determined the operational definition of new onset of SP. Of course, we could not detect rehospitalizations with the same episodes and recurrence 60 days before the last treatment. We will continue our research to find ways to solve the problem that you pointed out. We added the above to the Discussion and Methods sections. (P7L14, P23L26-28)**

11. *P7L42/50 :"" due to a lack..... Of this study " it is a limit of the study that must appear in the discussion by in the methods part in my opinion*
    - **RESPONSE: Thank you for your good suggestion. We have corrected the Discussion and Methods sections as you said (P7L26~, P23L30~)**

12. *P7L61/62: "surgical treatments and non surgical treatments": the authors must specify which types of gestures correspond to these definitions in the Methods section*
    - **RESPONSE: Thank you for your opinion. We have added to the Methods section regarding the kinds of treatments in each category and the classification criteria. (P8L7~)**

13. *The authors do not explain how they identified traumatic pneumothorax to exclude it from the analysis.*

- **RESPONSE: Thank you for astute suggestion. The points were mentioned for Reviewer 1, comment 4 above (P5L18~).**

14. *Results*
    A flow chart is required to present the overall results.
    - **RESPONSE: This is an important suggestion and we agree that a flowchart would make it easier to understand the results. However, in this study, only J93 patients were considered out of the data of the entire population from the start, Thus, it was quite difficult for us to draw a flowchart. We are really sorry about that. Please have us excused.**

15. *Discussion*
    The various remarks made previously must be taken up, implemented and discussed.
    - **RESPONSE: We greatly appreciate the reviewer for the valid opinion. All of these comments are very valuable and much helpful to us. We have revised the Discussion as best possible to reflect each of the points indicated above.**

RESPONSES TO REVIEWER #2's COMMENTS

Hiroaki Ogata
Research Institute for Diseases of the Chest, Graduate School of Medical Sciences, Kyushu University, Japan

Dear Dr.Hiroaki Ogata

Thank you very much for your kind words about our paper. We are grateful for the time and energy you expended on our behalf. Your suggestions were very helpful in improving our paper. In the following sections, you will find our responses to each of your points and suggestions

1. *Major comment 1:*
   Since the discussion concerning the annual prevalence rate of SP was highly descriptive, the authors should discuss more about the reason for its increasing trend; it may provide additional contribution to better understanding of SP.
   - **RESPONSE: Thank you for your suggestion. We have added to the Discussion as you suggested. (P21L10~)**

2. *Major comment 2:*
   It is better to define primary SP (PSP) and secondary SP (SSP) according to the CCI score, and to investigate the trends in annual prevalence rate and hospitalization treatment rate of PSP and SSP separately as a subgroup analysis. This analysis might help the interpretation of the increase in the annual prevalence rate of SP; it would be likely due to increase in the underlying lung diseases (e.g., COPD) if there was an increasing trend only in SSP, or it would seem to be associated with trends in risk factors for both PSP and SSP (e.g., inhalation of polluted air, smoking, etc.) if both subtypes of SP increased with time. According to the paragraph in page 21, lines 9-18, the authors made efforts to distinguish between PSP and SSP; I think it might be better to mention this point in the limitation part (page 22, lines 12-14).

- **RESPONSE: Thank you for your pertinent opinion. We further analyzed the prevalence rate of SSP by year (Supplement fig. 2). SSP was operationally defined as SP patients with a diagnosis of underlying lung disease (COPD, pneumonia, interstitial lung disease, lung cancer, asthma, and lung abscess) within a year before the first date of SP onset. As a result, tere was no significant change in the prevalence rate of SSP by year, which meant that PSP was increased. We have added the above to the Methods, Results, and Discussion. (P6L4~, P11L21~, P21L14~)**

3. *Major comment 3:*
   It would be appreciated if the authors perform some statistical analyses regarding the trends in the annual prevalence rate and hospitalization treatment rate of SP (e.g., logistic or Poisson regression models). It might be even better to perform such analyses with adjustment for age and sex.
   - **RESPONSE: Thank you for your interesting suggestion. We further analyzed hospitalization treatment rate of SP using logistic regression model. According to the analysis, the hospitalization rate was affected by sex, age, and underlying lung disease. (Supplement Table 5, P9L3~, P12L3~,)**

4. *Minor comment 1:*
   It may be better to fully explain the abbreviation "NSC" in page 5, line 6 rather than page 5, line 11
   - **RESPONSE: Thank you for pointing this out. We have revised it.(P4L23, P5L3)**

5. *Minor comment 2:*
   SP follow-up duration was set as 60 days; it may be useful to show the number of SP cases which required any medical service for more than 60 days.
   - **RESPONSE: This is a very important point, thank you very much. Determining the follow-up duration was one of the biggest challenges in this study. We confirmed the time when the new onset of follow-up treatment occurred through screening analysis, and discussed with medical experts in Korea. Finally, we determined the f/u duration as 60 days. In other words, we could not tell from the data whether treatment that occurred more than 60 days was due to existing SP or its recurrence, so we sought for advice. For the above reasons, we could not determine the number of SP cases which required any medical service for more than 60 days. Please have us excused, and thank you for your understanding. We will continue to research on how to overcome this problem./span>**

6. *Minor comment 3:*
   Page 7, line 11: the abbreviation "HIRA" should be fully explained.
   - **RESPONSE: Thank you for pointing that out. We have corrected it. (P7L20)**

7. *Minor comment 4:*
   Page 7, line 13: the abbreviation "NHIS" may be better to be used instead of "National Health Insurance Service".
   - **RESPONSE: Thank you for pointing this out. We have corrected it. (P7L22)**

8. *Minor comment 5:*
   Page 22, lines 19-21: the font size of the sentence "Lastly, as the diagnosis codes in claim data that was the basis for identifying the patients may not completely accurate [40], it is not sure that all patients with SP were listed under the corresponding diagnosis codes" seems to be smaller than other sentences.
   - **RESPONSE: Thank you for pointing that out. We have corrected it.**

9. *Minor comment 6:*
   It could be worth evaluating the annual prevalence rate of SP which required some surgical treatment or not, separately; it might contribute to estimation of trend in SP prevalence by the level of severity of SP.
   - **RESPONSE: Thank you very much, that is an interesting suggestion. Based on your advice, we analyzed the annual surgery rate. (Supplement fig. 3). As a result of the analysis, we found a very weak trend, but given the data, we could not figure out the cause of the change. We have added related content to the Results and Discussion section. (P19L12~, P23L14~)**

10. *Minor comment 7:*
    Since the random sampling was performed using the NHI data in 2002, I think there was a possibility of misclassification of first-time SP cases; there were "first-time" SP patients who ever suffered from SP before 2002. If so, it should be considered as a limitation.
    - **RESPONSE: We agree with you and thank you for the valuable opinion. It would be nice to do data wash out, but we analyzed without washing out considering the data loss. We** apologize **for that. We have added the above to the Discussion section as suggested. (P24L6~)**

11. *Minor comment 8:*
    Table 3: the number of COPD-related SP patients aged 15-34 years was similar to that among older populations, although COPD is likely to develop in aged population. It would be useful to explain this issue.
    - **RESPONSE: Thank you for your suggestion. Looking closely at the table, though COPD-was related to SP numbers among those aged 15-34 years, and the older population had similar numbers, the ratio was much higher in the older people (57.7%) than in those aged 15-34 years (11.3%). This is because the number of young patients with SP was much higher than those of older people. If you do not mind, we would prefer not to add any special description to the manuscript related to this. We thank you for understanding with us.**
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RESPONSES TO REVIEWER #3's COMMENTS

Michael Goldacre

Professor Emeritus of Public Health
Nuffield Department of Population Health
University of Oxford, UK

Dear Dr.Michael Goldacre

We thank the reviewer very much for the kind words written about our paper. We are grateful for the time and energy expended while reviewing our manuscript. Your suggestions were very

helpful in improving our study. In the following sections, you will find our responses to each of your comments and suggestions

1. *The authors should reconsider their use of the term 'prevalence rate' throughout the text and tables. As no doubt they know, 'prevalence' is, conventionally, a 'census measure' of the number of people who have the disease at a particular time and, 'when used without qualification, it usually refers to the situation at a specified point in time (point prevalence)' (Dictionary of Epidemiology, International Epidemiological Association, 5th edition). It is not really the right concept for an acute, short-lived condition like pneumothorax, and it is not in fact the measure that the authors show. There are problems, admittedly, in referring to their measure as 'incidence', because, as they discuss (e.g. last bullet-point of 'Strengths and limitations'), there are difficulties in knowing whether the events they describe are the 'first ever' events. However, what the authors show are reasonable proxies for incidence, and maybe this could be specified and the measures termed incidence with the limitations of the term, in this context, discussed. If the authors do not like the term 'incidence' (with qualifications specified), an alternative term could be medical 'service-contact' rates (which is actually what they show). I note that papers cited by the authors (eg their references 5-9) use the term 'incidence', despite its limitations*
   - **<u>RESPONSE:</u> Thank you for your insightful comments. The choice of terms between 'prevalence' and 'incidence' was something we have been thinking about. Strictly speaking, 'prevalence' is not an appropriate term. However, we determined to use 'prevalence' because there were several previous studies that reported 'prevalence' (Primary and Secondary Spontaneous Pneumothorax: Prevalence, Clinical Features, and In-Hospital Mortality, Prevalence and risk factors of pneumothorax among patients admitted to a Pediatric Intensive Care Unit., etc.), and this study was based on the occurrence within the period of a year. We would appreciate it if you could understand with us about the inevitable use of 'prevalence'. We have added the above to the Discussion section (P21L27).**

2. *Also, the authors might like to reconsider their use of the term '12-year longitudinal nationwide cohort study' in the title of the paper and elsewhere. It is only a 'longitudinal cohort' study in the sense that they follow up the patients for a limited period. As the authors say, 'the approximate treatment period for SP was 11 days for outpatients and 17.7 days for hospitalized patients'. It is not a '12-year cohort study', as the title suggests, which might lead readers to assume that they will get data on the 12-year outcomes of patients with pneumothorax. 'A 12-year nationwide study' (or something similar) as a subtitle after their semi-colon is probably a sufficient descriptor. Nonetheless, the data shown are interesting and worthwhile.*
   - **<u>RESPONSE:</u> We thank you for your valuable suggestion. Reviewer 1 also gave the same suggestion. We have revised the title, abstract, and manuscript to delete 'longitudinal' throughout, as you suggested. We also appreciate your interest in this analysis. (P2L6)**

Some fairly minor points:

3. *The authors write that other epidemiological studies of pneumothorax tended to be based on small numbers (e.g. page 21, lines 4-10, which concludes that other cited studies were 'based on small sample sizes (82–273 subjects)'. But the publication by me and my colleagues (their reference 27) was based on over 170,000 cases. A stronger claim than size for originality of the authors' study is*

the fact that they identified cases treated wholly outside hospital as well as hospitalised cases. I think that I am right in saying that most of the published papers they cite did not do this; and this dimension is interesting.

- **RESPONSE: It is a great pity that we were unaware of the paper being referred to by the reviewer. We had concentrated on looking for prior studies before October 2018. However, we have carefully read the paper 'Trends in the Incidence and Recurrence of Inpatient-Treated Spontaneous Pneumothorax, 1968-2016', and it has greatly helped us to revise our paper. We thank the reviewer very much, and as you pointed out, we have made the necessary revision.(P22L11)**

4. *Abstract, Objectives: The objectives as specified – 'to analyse…in order to obtain information about…' is tautological and does not really inform the readers why they should be interested in the information sought. Reconsider, and be more specific?*
   - **RESPONSE: Thank you for your pertinent suggestion. We have revised the objective to be clearer. (P2L2~)**

5. *Methods. Patient and Public Involvement: The authors' text under this heading is really a description of the dataset, and not what I assume the journal expects on PPI.*
   - **RESPONSE: We thank you for pointing this out. We have added PPI in the Methods section. (P9L7~)**

6. *Comorbidities in SP patients (page 6): The authors specify that diseases were counted '…if they were diagnosed…within 1 year of the first date of SP onset'. Could they please specify that they mean in either the year before or in the year after SP onset (or both).*
   - **RESPONSE: Thank you for pointing this out and we apologize for being unclear. It meant the year before; and we have rewritten this clearly.(P6L24)**

7. *Table 1, data on age, and associated text: one of the striking features of SP is its bimodal age distribution. The grouping of age in Table 1 is too broad to show this, although elsewhere the authors do draw attention to it. It would be good if the authors emphasised the bimodal nature of the age distribution (separately in males and females) as a characteristic of the epidemiology of SP.*
   - **RESPONSE: Thank you for your valuable suggestion. More detailed data on the age (divided into 5-yearly groups) was graphically added as a Supplement Figure 1, showing the bimodal age distribution.**

8. *Table 2: this may be more a matter for the editors than the reviewer, but the layout of the table is clumsy and needs attention.*
   - **RESPONSE: We are indeed sorry for the inconvenience. We have slightly modified the format of the table to improve the readability.**

9. *The author (Doori Kim) in your main document is written as (Kim, Duri) in ScholarOne. Please ensure that the author has same registered name.*
   - **RESPONSE: Thank you very much. We have checked and corrected the name from 'Kim, Duri' to 'Doori Kim.**

VERSION 2 – REVIEW

REVIEWER	Desmettre France university of bourgogne franche comte
REVIEW RETURNED	07-Jul-2019

GENERAL COMMENTS	Comments about the manuscript "Epidemiology and Medical Services Utilization for Spontaneous Pneumothorax: A 12-Year Study Using National Cohort Data in Korea" We commend the authors for their answers to the questions of the reviewers and the modifications made to the new version submitted. My recommbndations : 1) In the section "results", I definitively think that an organizational flow chart that summarizes the selection of patients will be interesting. 2) At the very beginning of the discussion, a paragraph of a few lines summarizing the main results is missing, before discussing these results 3) In the discussion section, to be affirmative of the increase of the prevalence of SP is difficult ; this must be more nuanced. In the manuscript just as in the summary, according to the bias of the study 4) the conclusion imust be modified. The conclusion must support the results and put into perspective, which is not the case in this version

REVIEWER	Hiroaki Ogata Research Institute for Diseases of the Chest, Graduate School of Medical Sciences, Kyushu University, Japan
REVIEW RETURNED	15-Jun-2019

GENERAL COMMENTS	The authors responded adequately to all comments.

REVIEWER	Michael Goldacre University of Oxford, UK
REVIEW RETURNED	14-Jun-2019

GENERAL COMMENTS	The authors have adequately addressed the reviewers' comments.