PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (see an example) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Factors influencing length of hospital stay among patients resident in Blackpool admitted with COPD: a cross sectional study
AUTHORS	Agboado, Gabriel ; Peters, Jonathan; Donkin, Lynn

VERSION 1 - REVIEW

REVIEWER	Dr Arpana Verma, MBChB, MPH (distinc), PhD, FFPH Senior Lecturer and Honorary Consultant in Public Health (NHS Bury) Director, Manchester Urban Collaboration on Health School of Translational Medicine Room 2.523, Stopford Building University of Manchester Manchester M13 9PT
REVIEW RETURNED	21-Feb-2012

THE STUDY	I am unsure how generalisable this data is. The authors acknowledge in the limitations the problems with incomplete data. It is also only using one area which questions how generalisable the paper is . I am not a statistician but I'm not sure that the approach used is the most for investigating the research question. I feel that multi-level modelling may be needed to adequately analyse this type of data. I would like to refer this question to a stats expert.
REPORTING & ETHICS	The authors have not adequately answered the STROBE checklist in line with the discussion. I don't fully understand the comments the authors have made when they recognise the nature of the data source used. There is no mention of ethics - I suspect that this was not needed however should have been stated
GENERAL COMMENTS	I would welcome the opportunity to discuss with a senior statistician the relevance of the stats performed to make these conclusions. I feel that the STROBE checklist doesn't take into account the issues you have mentioned in your manuscript. I think the paper needs additional comment on how generalisable the study is. What if this is an artefact of the limitations you have presented and cannot be replicated elsewhere.

REVIEWER	Dr Michael Soljak Clinical Research Fellow Department of Primary Care & Public Health School of Public Health Imperial College London LONDON W6 8RP
REVIEW RETURNED	01-Mar-2012

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THE STUDY	Supplemental documents do not contain information that should be better reported in the manuscript.
GENERAL COMMENTS	This paper examines factors associated with hospital length of stay for COPD as a primary diagnosis in the NW of England. It is clearly written, well organised and the methods and analyses are appropriate. The use of survival analysis methods for length of stay is innovattive. There is an up to date literature review. My comments about the methods are that discharge destination should have been included as a predictor variable, as it is known to influence length of stay, and that it is unclear whether or not there were separate multivariable models for each of the categories of predictor variables, or a single multivariable model. This is important as it is only by controlling for population factors that the effects of patient or healthcare factors, or vice versa, can be properly assessed. The authors also state that they "did not use the likelihood ratio test (i.e. I presume to select or drop variables) for the main effects because it was our aim to describe any association between these variables and LOS". However variable selection is accepted as a next step in describing associations and improving model fit (by parsimony). Possibly further explanation i.e. minor revision is all that is required. The main criticism of the study is that it is purely descriptive and does not add significantly to the evidence base on length of stay in COPD. The authors quote several papers which have previously described associations with the variables used, in particular age, deprivation and comorbidities. I have left the decision about acceptance or rejection for this reason to the editor.

REVIEWER	BERNET KATO, IMPERIAL COLLEGE LONDON Competing Interests: NONE
REVIEW RETURNED	16-Apr-2012

THE STUDY	The authors' mention that the strength of this study lies in the method used – proportional hazard analyses. I would not report this as strength of the study as researchers are obliged to use the correct statistical analysis methods to analyse their data.
REPORTING & ETHICS	CONSORT (Consolidated Standards of reporting Trials) is not considered since the paper does not deal with a trial. However, no note on informed consent or ethical approval has been mentioned.
GENERAL COMMENTS	GENERAL COMMENTS The authors investigate the factors associated with length of stay in hospital for COPD related admissions. METHODS The authors mention that they used the Kaplan-Meier to estimate the LOS by the predictor variables of interest. The Kaplan-Meier is a method of estimating survival curves for different groups e.g. patients on treatment 1 vs treatment 2. The authors should clarify what the Kaplan-Meier method was used for. Furthermore, the results of the "Kaplan-Meier analyses" are not mentioned in the paper.

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	The authors mention that they adjusted for clustering at patient level. How was this done? RESULTS Admission outcomes 2226 of the admissions were discharged to usual places of residence, 7 were to local authority-run care homes, 3 were to local authority run hospices, 147 resulted in death and 15 were transferred to other NHS hospitals. This renders a total of 2398 admissions. What about the remaining 12 (2410 – 2398) admissions that are not accounted for? Length of stay and socio-demographic variables The authors present results of three analyses where they seek to identify the factors that influence length of stay (LOS) in hospital. The factors are 1. Socio-demographic variables. 2. Temporal factors and geographical factors. 3. Health and health service factors. Subsequently results of the analyses are presented in Tables 1, 2 and 3. In the tables unadjusted and adjusted HRs are reported. For the adjusted HRs it is not clear which variables were adjusted for. For instance in Table 1, did the model include only socio-economic variables or did it also include variables in 2 and 3 above? I suggest the authors include a footnote in Tables 1, 2 and 3 indicating the variables that are included in the multivariate analyses. Further, results for analyses of interaction terms for season*IMD and Age*distance are presented in Table 4. Were any main effects included in the model(s) used to obtain the interaction terms? If so, were the main effects significant? The authors should provide some information on this. The authors mention that the proportional hazards assumption did not hold for gender. I would include a graph of the Schoenfeld Residuals. The second paragraph on page 8 states that “there was a progressive reduction in LOS over the period of study from a median of 7 days in 2005/06 to a median of 6 in 2009/10”. It looks like a reduction really happened in the period 2005/06 to 2006/07. After that there are no major changes – this is evidenced by the adjusted HRs in Table 2. For instance, if you take the period 2006/07 as the reference then the adjusted HRs comparing 2007/08 to 2006/07, 2008/09 to 2006/07 or 2009/10 to 2006/07 would be pretty close to 1 suggesting that there is no evidence of a reduction over the period 2006/07 to 2009/10. In the discussion (last paragraph on page 15) and conclusion (last paragraph on page 16), the authors mention that they found significant association between LOS and season of admission and distance of place of residence in selected patient groups. However, the analyses that give rise to these conclusions are not mentioned or shown anywhere in the paper. The first sentence on page 2 states that the predictor variables considered in the study were selected because of their well-

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	established association with health outcomes. The authors should briefly mention these health outcomes and provide some references. In Box 1, the title “Temporal factors” should be replaced with “Temporal and Geographical factors” so that it fits in with the Results section. Under Outcomes (page 6), the sentence “In this study we tested the hypothesis that LOS among Blackpool COPD patients are influenced by the factors listed in Box 1” should be replaced with something like “we sought to identify the factors that influence the length of stay in hospital among Blackpool COPD patients”. The authors should round off the p-values to two or three significant digits wherever they appear in the text and tables. Further, p-values = 0.0000 should preferably be presented as “<0.001”. How were the confidence intervals for the mean and median LOS obtained? Edits: Second sentence in paragraph 4 of the introduction: the word ‘COPD’ is misspelt. Line 11 in the section titled statistical analyses: ‘the all’ should be replaced with ‘all the’. First paragraph on page 8: put a space between the numbers and “km”. Fourth paragraph on page 15: The word ‘COPD’ is misspelt. Last paragraph on page 15: Insert ‘by’ after the word ‘captured’.

VERSION 1 – AUTHOR RESPONSE

Responses to comments from Reviewer: Dr Arpana Verma,

There is no mention of ethics - I suspect that this was not needed however should have been stated. (Thanks for pointing this out. This was an oversight and it has now been included after FUNDING)

I would welcome the opportunity to discuss with a senior statistician the relevance of the stats performed to make these conclusions. (Survival analyses are suitable for analysing longitudinal data in situations where time to an event is the outcome under investigation. When Cox proportional hazard model is used it is appropriate to test for the proportional hazard assumption. If this is violated one of the valid methods for accounting for that is to use the stratified proportional hazard model. This is why we did what we did.)

I feel that the STROBE checklist doesn't take into account the issues you have mentioned in your manuscript. I think the paper needs additional comment on how generalisable the study is. What if this is an artefact of the limitations you have presented and cannot be replicated elsewhere. (We have included the limited generalisability of the study in the limitations. This reads “This study was on a patient population in a defined geographical area. This has implications for its generalisability as factors such as patient profile, seasonal influences and service configuration could influence outcomes”. The wording STROBE checklist has also been changed accordingly to reflect this change.)

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Reponses to comments from Reviewer: Dr Michael Soljak

This paper examines factors associated with hospital length of stay for COPD as a primary diagnosis in the NW of England. It is clearly written, well organised and the methods and analyses are appropriate. The use of survival analysis methods for length of stay is innovattive. There is an up to date literature review. (Thank you)
My comments about the methods are that discharge destination should have been included as a predictor variable, as it is known to influence length of stay. (We did not have information on the discharge destinations (i.e. where they would have been discharged to should their clinical management achieve the outcome of being fit enough to leave hospital) for patients whose data were censored and small numbers in categories other than usual place of residence precluded this. We acknowledged this as a limitation of the study).
It is unclear whether or not there were separate multivariable models for each of the categories of predictor variables, or a single multivariable model. This is important as it is only by controlling for population factors that the effects of patient or healthcare factors, or vice versa, can be properly assessed. (We used a single multivariate model including all the variables. This has been clarified in our response to the next reviewer's comments. We also stated in the 2nd paragraph of STATISTICAL analysis section "the final multivariate model which included all the remaining variables")

The authors also state that they "did not use the likelihood ratio test (i.e. I presume to select or drop variables) for the main effects because it was our aim to describe any association between these variables and LOS". However variable selection is accepted as a next step in describing associations and improving model fit (by parsimony). Possibly further explanation i.e. minor revision is all that is required. (Thanks. We revised the relevant sentence to read "We did not use the likelihood ratio test for the main effects to identify those that contribute significantly to the fit of the model because it was our aim to describe any association between these variables and LOS".)

The main criticism of the study is that it is purely descriptive and does not add significantly to the evidence base on length of stay in COPD. (This is an "observational study" using routinely collected data. It will in the main be "descriptive" as it is not an experimental study.)

The authors quote several papers which have previously described associations with the variables used, in particular age, deprivation and comorbidities. I have left the decision about acceptance or rejection for this reason to the editor. (We did mention what the study adds to existing knowledge after CONCLUSIONS. We agree with the suggestion that the decision about the acceptance is left to the editor)

Reponses to comments from Reviewer: BERNET KATO

The authors' mention that the strength of this study lies in the method used – proportional hazard analyses. I would not report this as strength of the study as researchers are obliged to use the correct statistical analysis methods to analyse their data. (There are other valid methods of doing this. We believe proportional hazard model provide a more robust framework compared with e.g. logistic regression based on categorisation of LOS, as it described the "risk" of an event occurring at any point in time over the observation period. We reckon the comments reflect a personal preference of the reviewer hence we left this unchanged).

No note on informed consent or ethical approval has been mentioned. (This was an oversight in the submitted draft. This has now been included under ethical approval (see after FUNDING)

The authors mention that they used the Kaplan-Meier to estimate the LOS by the predictor variables of interest. The Kaplan-Meier is a method of estimating survival curves for different groups e.g. patients on treatment 1 vs treatment 2. The authors should clarify what the Kaplan-Meier method was used for. Furthermore, the results of the "Kaplan-Meier analyses" are not mentioned in the paper. (Based on the KM survival estimates, summary statistics such as the mean and median survival periods can be calculated. Many statistical softwares provide these facilities which we used. The mean and median LOS were from the output from the analysis. We refer the reviewer to the help section of the software we used for further details)

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The authors mention that they adjusted for clustering at patient level. How was this done? (Adjustment for clustering was done to adjust for the standard errors for the HR. This helps in appropriately adjusting for the 95% CI and the associated p values. Stata provides this facility and for further details we refer the reviewer to the help section of the software we used).

RESULTS
Admission outcomes 2226 of the admissions were discharged to usual places of residence, 7 were to local authority-run care homes, 3 were to local authority run hospices, 147 resulted in death and 15 were transferred to other NHS hospitals. This renders a total of 2398 admissions. What about the remaining 12 (2410 – 2398) admissions that are not accounted for? (The errors have been corrected)

Length of stay and socio-demographic variables

The authors present results of three analyses where they seek to identify the factors that influence length of stay (LOS) in hospital. The factors are
1. Socio-demographic variables.
2. Temporal factors and geographical factors.
3. Health and health service factors.

Subsequently results of the analyses are presented in Tables 1, 2 and 3. In the tables unadjusted and adjusted HRs are reported. For the adjusted HRs it is not clear which variables were adjusted for. For instance in Table 1, did the model include only socio-economic variables or did it also include variables in 2 and 3 above?
I suggest the authors include a footnote in Tables 1, 2 and 3 indicating the variables that are included in the multivariate analyses. (Suggested amendments have been done)

Further, results for analyses of interaction terms for season*IMD and Age*distance are presented in Table 4. Were any main effects included in the model(s) used to obtain the interaction terms? If so, were the main effects significant? The authors should provide some information on this. (More information have been provided and states “Only the results for the variable involved in the interaction terms were displayed because the results for the other main variables were the same as in the final model without the interaction terms” in the paragraph preceding the RESULTS section)

The authors mention that the proportional hazards assumption did not hold for gender. I would include a graph of the Schoenfeld Residuals. (We reported the p value which we believed adequately summarises what the graph will depict hence did not consider it necessary to include the graph.)

The second paragraph on page 8 states that “there was a progressive reduction in LOS over the period of study from a median of 7 days in 2005/06 to a median of 6 in 2009/10”. It looks like a reduction really happened in the period 2005/06 to 2006/07. After that there are no major changes – this is evidenced by the adjusted HRs in Table 2. For instance, if you take the period 2006/07 as the reference then the adjusted HRs comparing 2007/08 to 2006/07, 2008/09 to 2006/07 or 2009/10 to 2006/07 would be pretty close to 1 suggesting that there is no evidence of a reduction over the period 2006/07 to 2009/10. (The paragraph has been amended and ended as “however this increase was not sustained over the rest of the period”)

In the discussion (last paragraph on page 15) and conclusion (last paragraph on page 16), the authors mention that they found significant association between LOS and season of admission and distance of place of residence in selected patient groups. However, the analyses that give rise to these conclusions are not mentioned or shown anywhere in the paper. (The selected patient group referred to were those included in the interaction terms. We realised this statement may be unclear so we have replaced “groups” with “characteristics”. Please refer to the relevant section for the amendments in red fonts)

The first sentence on page 2 states that the predictor variables considered in the study were selected because of their well- established association with health outcomes. The authors should briefly mention these health outcomes and provide some references. (Thanks for pointing this out. The necessary amendment has been done)

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In Box 1, the title “Temporal factors” should be replaced with “Temporal and Geographical factors” so that it fits in with the Results section. (Thanks. This has been amended as suggested)

Under Outcomes (page 6), the sentence “In this study we tested the hypothesis that LOS among Blackpool COPD patients are influenced by the factors listed in Box 1” should be replaced with something like “we sought to identify the factors that influence the length of stay in hospital among Blackpool COPD patients”. (We accepted the text suggested here with gratitude)

The authors should round off the p-values to two or three significant digits wherever they appear in the text and tables. Further, p-values = 0.0000 should preferably be presented as “<0.001”. (We have retained the 4 decimal places as it is consistent with other articles published by BMJ Open. We have changed 0.0000 to <0.0001 (i.e. 4 decimal places) to maintain consistency throughout this paper)

How were the confidence intervals for the mean and median LOS obtained? (These are provided by Stata. Other statistical softwares do report these as well. Standard statistical texts on the subject provide ample explanation of the various methodological issues involved and are beyond the scope of this study.)

Edits:
Second sentence in paragraph 4 of the introduction: the word ‘COPD’ is misspelt. (Thanks. These have been amended)

Line 11 in the section titled statistical analyses: ‘the all’ should be replaced with ‘all the’. (Thanks. This has been amended)

First paragraph on page 8: put a space between the numbers and “km”. (Thanks. This has been amended)
Fourth paragraph on page 15: The word ‘COPD’ is misspelt. (Correction made)
Last paragraph on page 15: Insert ‘by’ after the word ‘captured’. (Thanks. The correction has been made)

VERSION 2 – REVIEW

REVIEWER	Arpana Verma Dr Arpana Verma, MBChB, MPH (distinc), PhD, FFPH Senior Lecturer and Honorary Consultant in Public Health (NHS Bury) Director, Manchester Urban Collaboration on Health School of Translational Medicine Room 2.523, Stopford Building University of Manchester Manchester M13 9PT
REVIEW RETURNED	04-Jun-2012

THE STUDY	I am in agreement with the other reviewers and would like to request that the authors fully answer the previous set of comments.
GENERAL COMMENTS	Many thanks to the authors for the revisions made and some attempt at addressing the concerns from myself and the other reviewers. I have attached a list of comments that still require further

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	clarification. Responses to comments from Reviewer: Dr Arpana Verma, There is no mention of ethics - I suspect that this was not needed however should have been stated. (Thanks for pointing this out. This was an oversight and it has now been included after FUNDING) I would welcome the opportunity to discuss with a senior statistician the relevance of the stats performed to make these conclusions. (Survival analyses are suitable for analysing longitudinal data in situations where time to an event is the outcome under investigation. When Cox proportional hazard model is used it is appropriate to test for the proportional hazard assumption. If this is violated one of the valid methods for accounting for that is to use the stratified proportional hazard model. This is why we did what we did.) I UNDERSTAND THE USE OF CPH MODEL FOR THE ANALYSIS OF LONGITUDINAL DATA BUT THE COMPARISON OF THE YEARS SURELY REQUIRES MORE COMPLEX ANALYSIS I.E. MULTI-LEVEL MODELLING AS TIME TO THE EVENT IS NOT ONLY USED AS THE OUTCOME VARIABLE BUT ALSO THE YEARS ARE COMPARED TO EACH OTHER AND SEASONAL DIFFERENCES ARE (SEE LATER). I feel that the STROBE checklist doesn't take into account the issues you have mentioned in your manuscript. I think the paper needs additional comment on how generalisable the study is. What if this is an artefact of the limitations you have presented and cannot be replicated elsewhere. (We have included the limited generalisability of the study in the limitations. This reads "This study was on a patient population in a defined geographical area. This has implications for its generalisability as factors such as patient profile, seasonal influences and service configuration could influence outcomes". The wording STROBE checklist has also been changed accordingly to reflect this change.) THE OTHER MAJOR LIMITATION IS THAT THE OTHER POSSIBLE CONFOUNDERS WERE NOT IDENTIFIED OR COLLECTED. THE INTERNAL VALIDITY OF THIS STUDY IS LOW AND HENCE THERE'S A NEED FOR THE AUTHORS TO CONSIDER IF THE CONCLUSIONS AND "WHAT THIS PAPER ADDS" ARE VALID Reponses to comments from Reviewer: Dr Michael Soljak This paper examines factors associated with hospital length of stay for COPD as a primary diagnosis in the NW of England. It is clearly written, well organised and the methods and analyses are appropriate. The use of survival analysis methods for length of stay is innovattive. There is an up to date literature review. (Thank you)

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	My comments about the methods are that discharge destination should have been included as a predictor variable, as it is known to influence length of stay. (We did not have information on the discharge destinations (i.e. where they would have been discharged to should their clinical management achieve the outcome of being fit enough to leave hospital) for patients whose data were censored and small numbers in categories other than usual place of residence precluded this. We acknowledged this as a limitation of the study). It is unclear whether or not there were separate multivariable models for each of the categories of predictor variables, or a single multivariable model. This is important as it is only by controlling for population factors that the effects of patient or healthcare factors, or vice versa, can be properly assessed. (We used a single multivariate model including all the variables. This has been clarified in our response to the next reviewer's comments. We also stated in the 2nd paragraph of STATISTICAL analysis section "the final multivariate model which included all the remaining variables") I AM NOT SURE THAT THE REVIEWER'S COMMENTS HAVE BEEN ADEQUATELY ADDRESSED The authors also state that they "did not use the likelihood ratio test (i.e. I presume to select or drop variables) for the main effects because it was our aim to describe any association between these variables and LOS". However variable selection is accepted as a next step in describing associations and improving model fit (by parsimony). Possibly further explanation i.e. minor revision is all that is required. (Thanks. We revised the relevant sentence to read "We did not use the likelihood ratio test for the main effects to identify those that contribute significantly to the fit of the model because it was our aim to describe any association between these variables and LOS".) I WOULD BE INTERESTED TO SEE IF THE REVIEWER FELT THIS IS A VALID STATEMENT The main criticism of the study is that it is purely descriptive and does not add significantly to the evidence base on length of stay in COPD. (This is an "observational study" using routinely collected data. It will in the main be "descriptive" as it is not an experimental study.) THIS IS DEFINITELY NOT AN OBSERVATIONAL STUDY AND IS PURELY DESCRIPTIVE AS THE REVIEWER STATES. I AM UNCLEAR ON WHAT GROUNDS THE AUTHORS WARRANT THIS AS AN OBSERVATIONAL STUDY. The authors quote several papers which have previously described

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	associations with the variables used, in particular age, deprivation and comorbidities. I have left the decision about acceptance or rejection for this reason to the editor. (We did mention what the study adds to existing knowledge after CONCLUSIONS. We agree with the suggestion that the decision about the acceptance is left to the editor) I DO NOT FEEL THAT THE METHODOLOGICAL FLAWS WARRANT THE STATEMENTS MADE IN THE WHAT THIS STUDY ADDS (SEE ABOVE). Reponses to comments from Reviewer: BERNET KATO The authors' mention that the strength of this study lies in the method used – proportional hazard analyses. I would not report this as strength of the study as researchers are obliged to use the correct statistical analysis methods to analyse their data. (There are other valid methods of doing this. We believe proportional hazard model provide a more robust framework compared with e.g. logistic regression based on categorisation of LOS, as it described the "risk" of an event occurring at any point in time over the observation period. We reckon the comments reflect a personal preference of the reviewer hence we left this unchanged). I COMPLETELY DISAGREE WITH THE AUTHORS AND WOULD RECOMMEND THAT THE ANALYSES ARE REVIEWED. No note on informed consent or ethical approval has been mentioned. (This was an oversight in the submitted draft. This has now been included under ethical approval (see after FUNDING) The authors mention that they used the Kaplan-Meier to estimate the LOS by the predictor variables of interest. The Kaplan-Meier is a method of estimating survival curves for different groups e.g. patients on treatment 1 vs treatment 2. The authors should clarify what the Kaplan-Meier method was used for. Furthermore, the results of the "Kaplan-Meier analyses" are not mentioned in the paper. (Based on the KM survival estimates, summary statistics such as the mean and median survival periods can be calculated. Many statistical softwares provide these facilities which we used. The mean and median LOS were from the output from the analysis. We refer the reviewer to the help section of the software we used for further details) THIS IS A VERY DISAPPOINTING RESPONSE FROM THE AUTHORS AND MAY BE A REFLECTION OF THEIR LIMITED EXPERIENCE, I WOULD NOT REFER A REVIEWER TO A HELP SECTION OF STATS PACKAGE, ESPECIALLY TO THIS IMPORTANT POINT. The authors mention that they adjusted for clustering at patient level. How was this done? (Adjustment for clustering was done to adjust

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	for the standard errors for the HR. This helps in appropriately adjusting for the 95% CI and the associated p values. Stata provides this facility and for further details we refer the reviewer to the help section of the software we used). AS ABOVE RESULTS Admission outcomes 2226 of the admissions were discharged to usual places of residence, 7 were to local authority-run care homes, 3 were to local authority run hospices, 147 resulted in death and 15 were transferred to other NHS hospitals. This renders a total of 2398 admissions. What about the remaining 12 (2410 – 2398) admissions that are not accounted for? (The errors have been corrected) Length of stay and socio-demographic variables The authors present results of three analyses where they seek to identify the factors that influence length of stay (LOS) in hospital. The factors are 1. Socio-demographic variables. 2. Temporal factors and geographical factors. 3. Health and health service factors. Subsequently results of the analyses are presented in Tables 1, 2 and 3. In the tables unadjusted and adjusted HRs are reported. For the adjusted HRs it is not clear which variables were adjusted for. For instance in Table 1, did the model include only socio-economic variables or did it also include variables in 2 and 3 above? I suggest the authors include a footnote in Tables 1, 2 and 3 indicating the variables that are included in the multivariate analyses. (Suggested amendments have been done) Further, results for analyses of interaction terms for season*IMD and Age*distance are presented in Table 4. Were any main effects included in the model(s) used to obtain the interaction terms? If so, were the main effects significant? The authors should provide some information on this. (More information have been provided and states “Only the results for the variable involved in the interaction terms were displayed because the results for the other main variables were the same as in the final model without the interaction terms” in the paragraph preceding the RESULTS section) The authors mention that the proportional hazards assumption did not hold for gender. I would include a graph of the Schoenfeld Residuals. (We reported the p value which we believed adequately summarises what the graph will depict hence did not consider it necessary to include the graph.) I DISAGREE AND THE AUTHORS SHOULD ADEQUATELY ADDRESS THIS POINT

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	The second paragraph on page 8 states that "there was a progressive reduction in LOS over the period of study from a median of 7 days in 2005/06 to a median of 6 in 2009/10". It looks like a reduction really happened in the period 2005/06 to 2006/07. After that there are no major changes – this is evidenced by the adjusted HRs in Table 2. For instance, if you take the period 2006/07 as the reference then the adjusted HRs comparing 2007/08 to 2006/07, 2008/09 to 2006/07 or 2009/10 to 2006/07 would be pretty close to 1 suggesting that there is no evidence of a reduction over the period 2006/07 to 2009/10. (The paragraph has been amended and ended as "however this increase was not sustained over the rest of the period") AS POINT 2 ABOVE In the discussion (last paragraph on page 15) and conclusion (last paragraph on page 16), the authors mention that they found significant association between LOS and season of admission and distance of place of residence in selected patient groups. However, the analyses that give rise to these conclusions are not mentioned or shown anywhere in the paper. (The selected patient group referred to were those included in the interaction terms. We realised this statement may be unclear so we have replaced "groups" with "characteristics". Please refer to the relevant section for the amendments in red fonts) The first sentence on page 2 states that the predictor variables considered in the study were selected because of their well-established association with health outcomes. The authors should briefly mention these health outcomes and provide some references. (Thanks for pointing this out. The necessary amendment has been done) In Box 1, the title "Temporal factors" should be replaced with "Temporal and Geographical factors" so that it fits in with the Results section. (Thanks. This has been amended as suggested) Under Outcomes (page 6), the sentence "In this study we tested the hypothesis that LOS among Blackpool COPD patients are influenced by the factors listed in Box 1" should be replaced with something like "we sought to identify the factors that influence the length of stay in hospital among Blackpool COPD patients". (We accepted the text suggested here with gratitude)

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	The authors should round off the p-values to two or three significant digits wherever they appear in the text and tables. Further, p-values = 0.0000 should preferably be presented as “<0.001”. (We have retained the 4 decimal places as it is consistent with other articles published by BMJ Open. We have changed 0.0000 to <0.0001 (i.e. 4 decimal places) to maintain consistency throughout this paper) How were the confidence intervals for the mean and median LOS obtained? (These are provided by Stata. Other statistical softwares do report these as well. Standard statistical texts on the subject provide ample explanation of the various methodological issues involved and are beyond the scope of this study.) THIS IS AN UNACCEPTABLE RESPONSE TO SIMPLE STATISTICAL PRINCIPLES. AUTHORS SHOULD KNOW HOW TO CALCULATE CIs Edits: Second sentence in paragraph 4 of the introduction: the word ‘COPD’ is misspelt. (Thanks. These have been amended) Line 11 in the section titled statistical analyses: ‘the all’ should be replaced with ‘all the’. (Thanks. This has been amended) First paragraph on page 8: put a space between the numbers and “km”. (Thanks. This has been amended) Fourth paragraph on page 15: The word ‘COPD’ is misspelt. (Correction made) Last paragraph on page 15: Insert ‘by’ after the word ‘captured’. (Thanks. The correction has been made)

REVIEWER	Michael Soljak Clinical Research Fellow School of Public Health Imperial College London No conflicting interests.
REVIEW RETURNED	26-Jun-2012

THE STUDY	The authors have responded thoroughly to all the statistical issues raised in the previous review. With regard to the question: "are the patients representative?", in my previous review I stated that the authors quote several papers which have previously described associations with the variables used, in particular age, deprivation and comorbidities, and that I had left the decision about acceptance or rejection for this reason to the editor. The paper reports on patients from a relatively small area and a single provider, so its generalisability therefore remains uncertain. On the other hand, some reassurance is given by the fact that the findings are similar to those in previous studies. The major new finding is that seasonality is also a risk factor in one part of the UK, as previously documented elsewhere. The contribution to existing

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	knowledge is therefore quite limited. However I am not in a position to judge what priority this report should have compared to others under consideration, so have indicated that the report is publishable with regard to technical criteria.

REVIEWER	DR BERNET KATO NATIONAL HEART AND LUNG INSTITUTE IMPERIAL COLLEGE LONDON UNITED KINGDOM I declare that I have no competing interests
REVIEW RETURNED	22-Jun-2012

- The reviewer completed the checklist but made no further comments**.**

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