

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	National trends in emergency readmission rates: A longitudinal analysis of administrative data for England between 2006 and 2016.
AUTHORS	Friebel, Rocco; Hauck, Katharina; Aylin, Paul; Steventon, Adam

VERSION 1 – REVIEW

REVIEWER	Dr. Valerie Moran, Research Fellow London School of Hygiene and Tropical Medicine, London, UK
REVIEW RETURNED	23-Nov-2017

GENERAL COMMENTS	<p>Strengths and limitations of this study: I don't think the fourth bullet point is a finding of this study.</p> <p>Introduction:</p> <p>Page 4, Line 24: It would be helpful to list some of the limitations, rather than the reader having to go to the references to find them.</p> <p>Methods:</p> <p>Page 6, Line 36: it's unclear what the two samples are – is it elective and emergency? Need to make this clearer.</p> <p>Page 6, Line 36-38: Please provide the number of observations excluded separately for each group/variable i.e. under 18s, maternity etc.</p> <p>Page 6, Lines 36 – 40: Did you also exclude patients with a diagnosis of cancer or chemotherapy for cancer as per the indicator specification?</p> <p>Page 6, Line 39: why did you exclude patients who died? These are included in the numerator in the national indicator specification.</p> <p>Page 6, Line 47: Reference 28 should be the most recent version i.e. 2017 rather than 2014.</p> <p>Page 7, Lines 22-25: the reference justifying the selection of conditions is somewhat dated – could these causes have changed since then or is there a more recent reference? More importantly, since you are using HES data why not analyse the utilisation data yourself to ascertain the sub-groups?</p> <p>Page 7, Line 55: why did you adjust for length of stay? What is it</p>
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	<p>capturing that you haven't already controlled for? It could reflect some aspects of quality of care that you don't want to control for. My understanding of risk-adjustment variables is that they should be unrelated to the treatment and measured prior to or at the onset of treatment. It would be useful to replicate the analysis without adjusting for length of stay to see if it makes a difference to the results.</p> <p>Results: General Comment: It would be good to present the findings of the sub-group analyses in a chart similar to Figure 1. I think a visual representation would highlight the extent of differences across sub-groups as this is the main contribution of the paper.</p> <p>Page 12, Lines 18-22: Need to include a reference to Figure 1.</p> <p>Page 12, Lines 22-25: This is not in Table 1.</p>
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REVIEWER	Bernard Silke St James' Hospital and Trinity College, Dublin
REVIEW RETURNED	30-Nov-2017

GENERAL COMMENTS	<p>The authors are experts in their field and the data is very large and impressive. Clearly the data is important and the paper should be published.</p> <p>That is not to say that the paper cannot be criticised or should be published in its present form.</p> <p>The notion that early readmissions are unequivocally a measure of quality is not universally accepted. There is a large body of literature that would seriously argue that this is not the case.</p> <p>I am not a supporter of this argument that early readmission is necessarily a measure of quality. According to the authors our hospital would be performing sub optimally. Between 2002 and 2006 our early readmission rate was 9.0% with a high and low of 8.2 and 9.7%. And a SD of 0.29. The 30-day mortality (in contact to the complete lack of systematic trend over that period) fell from 7.0% to 4.7%. The literature appears to agree that the mortality outcomes and the early readmission rates are not correlated. Our variation in the early readmission rates was of the order of 14%. The problem with attribution of readmissions as a quality indicator as fact rather than merely an opinion is that it is used by healthcare planners as a weapon. It is a short step to pejorative language such as poor performers and suboptimal outcomes. Much of the variations between hospital (perhaps 50 - 60%) may be attributed to deprivation status. It is not clear that the statistical methods adequately adjust or allow for the load factor between hospitals, when adjusting for deprivation.</p> <p>The authors are clearly preminent and experts in statistics. However, from my perspective as a clinician they do not present the data in a way that I could support.</p> <p>Overall I would expect that improved healthcare outcomes (as</p>
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	<p>evidenced by the early readmission rate) would be consistent over many areas and show a strong trend. The readmission rates went up between 2006 and 2011/12 and then fell. Overall reductions were unimpressive (in metrics rather than statistical significance) and for some reported groups seemed to show larger increases than decreases. Yet these tiny changes are described as encouraging healthcare improvements in healthcare quality to patients across England. I despair that such eminent professionals could spin so much on these data that show absolutely no consistent trend over time.</p> <p>It would be nice to see the variation in the readmission rates within each hospital. And to see how the intrinsic variation in these rates compared with the magnitude of the improved outcomes. For example rather than considering the average value of each hospital, consider the lowest year readmission rate of the worst performer and the highest year readmission rate of the best performer and their overlap. I would take each hospital then and calculate a loading factor relating to the number of small areas in the top two quintiles and then look at the total admissions year by year, against diagnoses by category, with the loading factor and considering the intrinsic variation (the early readmission rate) within each institution. But then I would as Lord Kelvin said be seeking understanding rather than confirmation bias.</p> <p>Clearly in the authors views some trusts are not performing adequately and must be encouraged to make efforts to mend their ways. Despite the intrinsic deficiencies of their data and its limitations.</p> <p>Early readmissions with us have the following metrics re predictability: Acuity 1.50 (95% CI: 1.45, 1.55) - lab score Charlson 1.20 (95% CI: 1.15, 1.25) Disabling Disease 1.08 (95% CI: 1.04, 1.10) Deprivation 1.18 (95% CI: 1.13, 1.23)</p> <p>So the authors are missing the acuity component and not determining chronic disabling disease. The acuity (laboratory score) accounting for 50% of the readmission variance will be unexplained as these data typically are not collected.</p> <p>In conclusion, of course the data is wonderful and I would like to see it mirrored in the critical application of the authors critique of the data. I would be in no doubt that had I access to their data, and the opportunity to play with it, I could put a completely different spin on it. Hopefully much more realistic view. But then I am a dinosaur.</p>
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VERSION 1 – AUTHOR RESPONSE

Dear Mrs Bedi,

We would like to thank you for re-considering our manuscript titled "National trends in emergency readmission rates: A longitudinal analysis of administrative data for England between 2006 and 2016" for publication in BMJ Open.

We believe that this study addresses an important aspect of the delivery of health care services, particularly because policy makers across several health care systems have placed great emphasis on reducing readmission rates as a way to improve quality of care. While we acknowledge the clear limitations that are present when using readmission rates as an outcome measure, the enhanced policy focus on reducing readmission rates in the English NHS following the 2010 white paper, Equity and Excellence: Liberating the NHS, clearly warrant a descriptive analysis, as presented in this paper. Unlike other studies, we could examine a large administrative health data source, investigating trends of emergency readmission rates and their changes in variation across 150 providers over ten years. Moreover, we were able to look into trends and variation across nine clinical subgroups.

We are grateful that you entered our manuscript into the peer-review process and found the peer-reviewer comments very constructive and encouraging. We have made major changes to both the introduction and discussion sections to address the comments, and provide one-by-one reviewer responses below. As requested, we have refined the inclusion criteria of our study sample to align closer with national guidelines. Therefore, we have now excluded patients admitted with a diagnosis of cancer, or chemotherapy. Following the exclusion of these patients, we have re-estimated our model and updated the results section. Moreover, the revised manuscript offers a more balanced presentation on the use of readmission rates as a quality measure; specifically we refined the section on limitations associated with relying on readmission rates as an outcome measure. We hope that this will provide your readership with a clearer understanding about the possible interpretation of observed changes in trends of readmission rates and their variation that occurred in England. Furthermore, we now provide a more balanced interpretation of our findings in the discussion section.

Best wishes,
The authors

Editorial Requirements:

(1) Please revise the Strengths and Limitations section (after the abstract) to focus on the methodological strengths and limitations of your study rather than summarizing the results.

Our response:

We have modified the strength and limitations section to address this comment. Specifically, we have added/substantially amended three bullet points that explain the limitations of our paper, and the limitations of using readmission rates as a measure for health care quality. We also highlighted that this study investigates trends of readmission rates and does not evaluate specific interventions that were aimed at reducing readmission rates across the observation period. The new strengths and limitations section reads as follows:

- The use of a large administrative health data source allowed capturing all patients entering the English National Health Service between 2006 and 2016.
- This study extended the scope of the previous literature, by examining changes in readmission trends and variation for all patients, and for nine clinical subgroups.
- Our analysis employed the systematic component of variation, which provides an estimation of the unobservable part of the variation that is due to hospital characteristics.
- The risk-adjusted, 30-day readmission rate and the systematic component of variation assume that all patient-level predictors of a readmission are controlled for by the information entered into the logistic regression model.

• There may be other dimensions of quality of care that we were not able to measure through readmission rates.

(2) Thank you for confirming that the study uses pseudomised and unidentifiable patient records. Please include a statement to this effect in the main text of the manuscript.

Our response:

We have now added the requested information into the manuscript. Page 6 states: "We obtained pseudonomised and unidentifiable patient health care records from the administrative Hospital Episode Statistics (HES) database."

Reviewer #1 - Dr Valerie Moran:

(1) Strengths and limitations of this study: I don't think the fourth bullet point is a finding of this study

Our response:

Thank you for your comment. We have now modified the strengths and limitations section to make it align better with its purpose. Instead of summarising the findings of the paper, we have now listed strengths related to the use of HES data, and limitations related to the use of readmission rates as a quality metric, and related to our methodology. Please, refer to editorial requirement #(1) for the updated strengths and limitations section.

(2) Page 4, Line 24: It would be helpful to list some of the limitations, rather than the reader having to go to the references to find them.

Our response:

We now list three key limitations to the use of readmission rates as a quality metric. Specifically, we mention a) difficulties in distinguishing avoidable readmission rates from those unavoidable through actions of the health care services; b) possibility of omitted variable bias in the risk-adjustment of readmission rates; and c) their competing relationship with other outcome measures (i.e. mortality rates). Please, find changes made to the manuscript on page 4.

(3) Page 6, Line 36: it's unclear what the two samples are – is it elective and emergency? Need to make this clearer.

Our response:

To address this comment, on page 6, the manuscript now states: "We also excluded the following elective and emergency admissions from the study sample". We hope by spelling this out, it became now clearer for the reader that we refer to elective and emergency admissions.

(4) Page 6, Line 36-38: Please provide the number of observations excluded separately for each group/variable i.e. under 18s, maternity etc.

Our response:

We incorporated the requested additions in the manuscript. Please, find on page 6 the following listed exclusions:

- Below 18 years of age = 17,860,079;
- Without complete records of variables required for risk-adjustment = 11,173,561;
- Maternity cases = 12,085,711;
- Admission related to cancer or chemotherapy = 13,985,696;
- Indexed admission that was not survived by the patient = 1,296,703.

(5) Page 6, Lines 36 – 40: Did you also exclude patients with a diagnosis of cancer or chemotherapy for cancer as per the indicator specification?

Our response:

Thank you for this comment. We have re-run our analysis, excluding any admissions with a diagnosis of cancer, or chemotherapy. The identification of patients followed the diagnoses codes listed by NHS Digital, and could be found here [1]. We also list the number of excluded observations on page 6. As pointed out correctly by the reviewer, our study therefore now aligns closely with the official indicator specification for England.

(6) Page 6, Line 39: why did you exclude patients who died? These are included in the numerator in the national indicator specification.

Our response:

Thank you for this comment. The focus of our study was on patients that survived the indexed hospitalisation, as we were interested to see whether the rate of those patients who survived the initial care episode changed. Specifically, we assumed that the observed readmission rate was related to the quality of care provided to patients along the care pathway. In order to pick up quality of transitional care services, or during the post-discharge period, it is essential that the patients were discharged alive. One confounder of our study is of course the lack of information on patient mortality outside of hospital. However, we were not able to capture this within the remit of our available HES data.

(7) Page 6, Line 47: Reference 28 should be the most recent version i.e. 2017 rather than 2014.

Our response:

We have updated reference 28 to the most recent version, i.e. 2017 - [1].

(8) Page 7, Lines 22-25: the reference justifying the selection of conditions is somewhat dated – could these causes have changed since then or is there a more recent reference? More importantly, since you are using HES data why not analyse the utilisation data yourself to ascertain the sub-groups?

Our response:

Thank you for this comment. We have now updated the previously stated reference. Specifically, we refer to official utilisation statistics from 2015/16 published by NHS Digital. The link to the excel spread sheet can be found here - [2]. It appears that the selected conditions for this study impose a significant burden on the NHS. The authors therefore agreed to stick to the initially selected list of acute and chronic conditions.

(9) Page 7, Line 55: why did you adjust for length of stay? What is it capturing that you haven't already controlled for? It could reflect some aspects of quality of care that you don't want to control for. My understanding of risk-adjustment variables is that they should be unrelated to the treatment and measured prior to or at the onset of treatment. It would be useful to replicate the analysis without adjusting for length of stay to see if it makes a difference to the results.

Our response:

Thank you for this comment. Determining variables used in the risk-adjustment of the outcome is often difficult and mostly limited to information available in the utilised database. By entering patient-level information, we are able to account for differences in patient case-mix that naturally affects the outcome, but is under no control of the discharging hospital. As highlighted by reviewer #2, one of the key limitations of a study like ours, which is based on administrative health data, is its limited availability of information on patient complexity, or specifically the severity of the disease. Acuity or severity has been shown to be highly predictive of a readmission.[3] By using the Charlson index, we are trying to capture some of the patient complexity, yet the measure is not perfect. We decided to enter length of stay into the risk-adjustment model, as we believed it would act as a proxy for patient complexity. Thus, the longer a patient remains in hospital, the more severe his condition is to be assumed. Moreover, the more time the patient remains in hospital, the higher the chances of incurring an adverse event, which in turn will impact on the likelihood of being readmitted following discharge.

We have now entered the reasoning for including length of stay into the risk-adjustment model on page 8. The revised manuscript states: "Length of stay was entered into the risk-adjustment process, as every extra day spent in hospital was found to be associated with an increased risk of incurring an adverse health event,[4] possibly affecting the patients' likelihood of recovery, but it might also indicate disease severity in the absence of any other adequate measures recorded within the HES database."

To further address to the reviewer comment, we have re-estimated our risk-adjusted readmission rates with and without length of stay as a predictor variable. The risk-adjusted rates only differ marginally and followed the same trend. Please find a table outlining both rates below:

(10) General Comment: It would be good to present the findings of the sub-group analyses in a chart similar to Figure 1. I think a visual representation would highlight the extent of differences across sub-groups as this is the main contribution of the paper.

Our response:

Thank you for this comment. We fully agree with the reviewer and created a new visual representation - figure 2, subdivided into a panel of three to present each clinical subgroup (acute, chronic, and surgical). We hope that this will strengthen the message of our paper and also provide a better overview of changes to the readership of this journal.

(11) Page 12, Lines 18-22: Need to include a reference to Figure 1.

Our response:

We inserted a reference to Figure 1 on page 12.

(12) Page 12, Lines 22-25: This is not in Table 1.

Our response:

Thank you for spotting this. We deleted the reference to table 1 from page 12 and refer to it once at an earlier point in the paragraph.

Reviewer #2 – Dr Bernard Silke:

(1) The authors are experts in their field and the data is very large and impressive. Clearly the data is important and the paper should be published. That is not to say that the paper cannot be criticised or should be published in its present form.

Our response:

Thank you. We will address your comments in the following.

(2) The notion that early readmissions are unequivocally a measure of quality is not universally accepted. There is a large body of literature that would seriously argue that this is not the case. I am not a supporter of this argument that early readmission is necessarily a measure of quality. According to the authors our hospital would be performing sub optimally. Between 2002 and 2006 our early readmission rate was 9.0% with a high and low of 8.2 and 9.7%. And a SD of 0.29. The 30-day mortality (in contact to the complete lack of systematic trend over that period) fell from 7.0% to 4.7%. The literature appears to agree that the mortality outcomes and the early readmission rates are not correlated. Our variation in the early readmission rates was of the order of 14%. The problem with attribution of readmissions as a quality indicator as fact rather than merely an opinion is that it is used by healthcare planners as a weapon. It is a short step to pejorative language such as poor performers and suboptimal outcomes. Much of the variations between hospital (perhaps 50 - 60%) may be attributed to deprivation status. It is not clear that the statistical methods adequately adjust or allow for the load factor between hospitals, when adjusting for deprivation.

Our response:

While we agree with your comment about the limitations related to using readmission rates as an outcome measure, we feel that our paper does not endorse the use of readmission rates as measures of quality. In fact, our main conclusion is that there is a significant degree of unexplained variation in readmission rates, supporting the statistics you mention above and the need for careful interpretation. Moreover, because the NHS has been focussing increasingly at reducing readmission rates, justifiably or not, there is a need for information about what has happened to them, longitudinally. Lastly, there are now papers which do suggest that emergency readmission rates do reflect on certain aspects of health care quality. We have made several changes to the manuscript to avoid language such as poor performers and suboptimal outcomes, since we agree with you that providers should be judged on more than just one metric. We have made 3 substantial changes to the manuscript to address your comments, they are as follows:

1. The strengths and limitations section now clearly states the key limitations related to readmission rates, which are:

- Both employed metrics assume that all person-level predictors of a readmission are controlled for by the information entered into the logistic regression model.
- There may be other dimensions of quality of care that we were not able to measure through readmission rates.

2. On page 4, in the introduction section, we spelled out three key limitations to the use of readmission rates as an outcome measure, followed by links to literature that found correlations with quality of care along the clinical pathway.

“Emergency hospital readmission rates are widely used for measuring health system performance.[8–10] They have important and well-known limitations,[11] which include the difficulty in distinguishing readmissions avoidable through actions of health care providers from those caused by other factors such as the patient complexity, a sensitivity to omitted variable bias in risk-adjustment models, a link with competing outcome measures of quality (i.e. mortality rates, or length of stay), and their link to factors outside the control of hospitals (e.g. primary care, or social isolation). Nevertheless, there is now mounting evidence that they are correlated with quality of care provided to patients along the clinical pathway. This includes quality of care at the initial hospital stay,[12] transitional care services[13–15] and post-discharge support.[16,17]”

3. On page 17, in the discussion section, we outline the importance for examining other outcome measures before being able to make clear judgements about changes in quality of care. The revised manuscript now states:

“The validity of emergency readmission rates as a measure for quality of care had been questioned before, mainly due to their sensitivity to changes in patient case-mix, random variation, and the poor correlation with other indicators of hospital quality.[50] It is therefore important to bear in mind that readmission rates may not be the best metric for investigating quality of care, with research into a set of widely used and accepted quality indicators required to provide a more comprehensive picture of changes occurred in health care systems and over time. While the limitations of readmission rates as a metric might be a particularly relevant concern for direct provider comparisons, such as in the case of imposing financial penalties for hospitals with high readmission rates and associated fears about unintended consequences,[51] in this study we aimed to assess overall trends in readmission rates for all trusts and across ten years. This approach helped to deal with random variation and presented longitudinal changes in readmission rates in the English NHS.”

(3) Overall I would expect that improved healthcare outcomes (as evidenced by the early readmission rate) would be consistent over many areas and show a strong trend. The readmission rates went up between 2006 and 2011/12 and then fell. Overall reductions were unimpressive (in metrics rather than statistical significance) and for some reported groups seemed to show larger increases than decreases. Yet these tiny changes are described as encouraging healthcare improvements in healthcare quality to patients across England. I despair that such eminent professionals could spin so much on these data that show absolutely no consistent trend over time.

Our response:

Thank you for this comment. As explained in our reply to Dr Moran, we have excluded cancer patients from our study sample and re-run the analysis. This has led to some changes in the results. We still observe a small increase in average risk-adjusted, 30-day readmission rates from 6.56% in 2006/07 to 6.76% in 2012/13, followed by a small decrease to 6.64% in 2015/16. When expressed as percentages, changes in readmission rates appear very small. However, when calculating changes in terms of number of patients based on the admissions per financial year, it becomes apparent that even small changes in rates translate in to thousands of admissions. This is explained by the unusually large sample size used for our analysis. , To clarify this, we have added an additional sentence into the results section on page 12:

“While percentage changes in risk-adjusted, 30-day emergency readmission rates appear insubstantial, when calculating the total number of patients readmitted per year from the number of indexed admissions per year, the small decrease in readmissions between 2012/13 and 2015/16 translated into approximately 7000 fewer readmissions per year.”

We also provided a more critical interpretation of changes in the discussion section on page 17:

“While our findings present statistically significant differences in readmission rates across financial years, the relative magnitude of change was small, with their clinical meaningfulness depending on the distribution of their incremental changes across trusts.”

To address the reviewers concerns, regarding the interpretation of findings as too positive, we have revised the conclusion section on page 19. Small corrections were made throughout the manuscript to provide a more balanced interpretation.

“Small initial rises in emergency readmission rates after discharge from any indexed admission was followed by stable, or even slightly decreasing emergency readmission rates after 2012/13. We also found a decrease in variation from 2006/07 to 2015/16. These changes in readmission rates fall into a period of an enhanced focus on quality improvement in the English NHS, thereby suggesting possible impacts of local-level and national-level efforts to stabilise, or even contain rises emergency readmission rates. However, changes in both metrics were only modest and they varied widely by clinical area, which might have several possible causes. For example, while reductions in readmissions for chronic conditions may indicate changes in quality provided outside the hospital (i.e. in primary care settings), increases in readmissions for acute conditions such as pneumonia patients might be linked to factors in quality not captured through readmission rates, such as improvements in patient survival at the indexed admission. Lastly, and importantly, changes in readmission rates may be related to changes in other factors that we could not adjust for in our analysis.”

Finally, we have now added an additional figure (figure 2) to illustrate trends in readmission rates across patient subgroups, which will help visualise that certain clinical subgroups are in fact following similar patterns in readmission rates over time, while others don't.

The observed trend in readmission rates could potentially be explained by the national-level and local-level efforts to reduce readmission rates, although our paper is not able to evaluate those policies, as we clearly state in the limitations. This includes several policies such as financial penalties for hospitals with excess readmission rates, but also funding transfers from secondary to social care services in an attempt to provide better quality care outside the hospital environment, post 2011. Many of these policies focussed on particular patient groups, and it is therefore unreasonable to assume that changes in trends have to remain constant across a relatively lengthy ten-year period and across patient subgroup.

In summary, we are confident that our study presents, analyses and interprets data in a way that is not in any way biased in its interpretation. The methodology used in this paper is fully described in the methods section, including its limitations, and it is consistently applied across patient groups and thus, replicable.

(4) It would be nice to see the variation in the readmission rates within each hospital. And to see how the intrinsic variation in these rates compared with the magnitude of the improved outcomes. For example rather than considering the average value of each hospital, consider the lowest year readmission rate of the worst performer and the highest year readmission rate of the best performer and their overlap. I would take each hospital then and calculate a loading factor relating to the number

of small areas in the top two quintiles and then look at the total admissions year by year, against diagnoses by category, with the loading factor and considering the intrinsic variation (the early readmission rate) within each institution. But then I would as Lord Kelvin said be seeking understanding rather than confirmation bias.

Our response:

Thank you for this suggestion, this is a great idea. However, it would be a substantial change to our research question, which is to examine pooled trust-level changes in readmission over time. We therefore feel this is beyond the scope of the present study. However, in future work we may well take up your idea and investigate variation in readmission further, which would present an opportunity to apply your suggested methodology.

(5) Clearly in the authors views some trusts are not performing adequately and must be encouraged to make efforts to mend there ways. Despite the intrinsic deficiencies of their data and its limitations.

Early readmissions with us have the following metrics re predictability:

Acuity 1.50 (95% CI: 1.45, 1.55) - lab score

Charlson 1.20 (95% CI: 1.15, 1.25)

Disabling Disease 1.08 (95% CI: 1.04, 1.10)

Deprivation 1.18 (95% CI: 1.13, 1.23)

So the authors are missing the acuity component and not determining chronic disabling disease. The acuity (laboratory score) accounting for 50% of the readmission variance will be unexplained as these data typically are not collected.

Our response:

Thank you for this comment. Our risk-adjustment models are unfortunately confined to data that is collected and made available in administrative data sets. As highlighted by the reviewer, one key predictor of a readmission is disease severity, or acuity. Unfortunately, this is not recorded within HES and this has been a problem commonly faced by observational studies using this data. In our study, we tried to address this concern by constructing the Charlson comorbidity index. While not perfect, it allows accounting for differences in patient complexity at least to some degree. Moreover, we also adjusted for length of stay, which we consider another a proxy for severity. In the revised manuscript we now state in greater detail than before that our study might suffer from omitted variable bias in the risk-adjustment of readmission rates. For example:

Page 18 – “Another concern relates to omitting variable bias in the risk-adjustment for emergency readmission rates, such as by the lack of information on clinical severity (i.e. acuity determined through laboratory test results) that was found to be highly predictive of a readmission.[52] Our study may therefore dilute the true predicted likelihood (i.e. upward or downward depending on the severity of disease) of a patient having to return to hospital. We were not able to address this limitation within our dataset, but we used the Charlson index to capture some of the patients’ clinical complexity[53] and further accounted for improvements in recording practices by including interaction terms of the Charlson index in each financial years into our risk-adjustment model.”

Page 18 – “We constructed the SCV, a measure that represented potentially ‘avoidable’ variation that can be attributed to differences in quality of care, provided our controls for patient characteristics that are not under the influence of the health system within the prediction model. Similar to the risk-adjusted readmission rates, the interpretation of the SCV follows the assumption that all ‘unavoidable’ variation in readmissions was sufficiently addressed by the information that was entered into the

prediction model. However, it is possible that other factors explained the variation in emergency readmission rates. In particular, the subgroup analysis showed rises in emergency readmission rates for many of the selected acute conditions. These changes might be explained by reductions in patient mortality, triggered through technological advancements, which have been found to inversely correlate with emergency readmission rates for patient with hip fracture.[54] In fact, increases in readmission rates may reflect positively on the care provided to patients in the NHS. Our findings are also susceptible to time varying confounders, such as the establishment of Hyper Acute Stroke Units in London and Greater Manchester in 2010,[55,56] leading to a step change in quality provided to stroke patients across different parts of the country.“

Page 19 – “Populating risk-adjustment models with information other than those currently available from secondary care data sets would allow for more precise estimates of risk-adjusted, emergency readmission rates.“

However, we do not state that some trusts are not performing adequately in the manuscript, and hope that the revised version provides a balanced interpretation of the findings. The key message of this paper was to examine changes in readmissions over time, without being able to explain the causal links behind the observed changes.

(6) In conclusion, of course the data is wonderful and I would like to see it mirrored in the critical application of the authors critique of the data. I would be in no doubt that had I access to their data, and the opportunity to play with it, I could put a completely different spin on it. Hopefully much more realistic view. But then I am a dinosaur.

Our response:

Thank you for providing such a detailed and thoughtful peer-review of our article. We believe that your comments contributed greatly to strengthening the paper, and we hope that the changes made to the manuscript provide a more balanced interpretation of the results, a more ‘realistic’ view on what can be measured through readmission rates, and a critical discussion what other factors could explain our findings. Our analysis can easily be replicated based on the described inclusion and exclusion criteria applied to the data, and the detailed description of the methods. Overall, we believe that our paper is an important contribution to the discussion on readmission rates, because it provides robust evidence on changes of readmission rates over time.

References

- 1 Digital N. NHS Outcomes Framework. 2017. <https://indicators.hscic.gov.uk/webview/>
- 2 NHS Digital. Hospital Admitted Patient Care Activity: 2015-16. 2016. doi:ISBN 978-1-78386-862-9
- 3 Zhou H, Della PR, Roberts P, et al. Utility of models to predict 28-day or 30-day unplanned hospital readmissions: an updated systematic review. *BMJ Open* 2016;6:e011060. doi:10.1136/bmjopen-2016-011060
- 4 Hauck K, Zhao X. How Dangerous is a Day in Hospital? *Med Care* 2011;49:1068–75. doi:10.1097/MLR.0b013e31822efb09

VERSION 2 – REVIEW

REVIEWER	Bernard Silke St James' Hospital and Trinity College, Dublin
REVIEW RETURNED	09-Jan-2018

GENERAL COMMENTS	The paper is now much more balanced and reflective than the initial draft. Some of the bias has been removed and the position adopted is a reasonable reflection of the data strengths and limitations.
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REVIEWER	Dr. Valerie Moran London School of Hygiene and Tropical Medicine, U.K.
REVIEW RETURNED	23-Jan-2018

GENERAL COMMENTS	Thanks to the authors for taking my comments on board and revising the article accordingly.
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