Author's response to reviews


Authors:

   Peter J.E. Holt (peteholt@btinternet.com)
   Sidartha Sinha (peteholt@btinternet.com)
   Baris A Ozdemir (peteholt@btinternet.com)
   Alan Karthikesalingam (peteholt@btinternet.com)
   Jan D Poloniecki (peteholt@btinternet.com)
   Matt M Thompson (peteholt@btinternet.com)

Version: 4 Date: 28 April 2014

Author's response to reviews: see over
Author’s response to reviews

Title: Variations and inter-relationship in outcome from emergency admissions in England. A retrospective analysis of Hospital Episode Statistics from 2005-2010

Authors:

Peter J.E. Holt pholt@sgul.ac.uk
Sidartha Sinha sid261@hotmail.com
Baris A Ozdemir drbaozdemir@gmail.com
Alan Karthikesalingam alankarthi@gmail.com
Jan D Poloniecki sgp300@sgul.ac.uk
Matt M Thompson Matt.Thompson@stgeorges.nhs.uk

Version: 2   Date: 23rd January 2014

Author’s response to reviews: see over
Dear Biomed Central Editorial Team,

Please find attached the responses to your editorial office comments.

MS: 1882070134119445

1. Title page

Please include a title page at the front of your manuscript file. It should contain, at minimum, the names, institutions, countries and email addresses of all authors, and the full postal address of the submitting author.

This has been updated to include the information requested.

2. Name of ethics committee

Please update your ethics statement to include the name of the ethics committee that state that this study does not require ethical approval.

This has been done as requested. (WanREC)

3. Author's Contribution

For manuscripts with more than one author, all BMC Series journals require an Authors' Contributions section to be placed after the Competing Interests section.

An 'author' is generally considered to be someone who has made substantive intellectual contributions to a published study. To qualify as an author one should 1) have made substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) have been involved in drafting the manuscript or revising it critically for important intellectual content; and 3) have given final approval of the version to be published. Each author should have participated sufficiently in the work to take public responsibility for appropriate portions of the content. Acquisition of funding, collection of data, or general supervision of the research group, alone, does not justify authorship.

We suggest the following format (please use initials to refer to each
author's contribution): AB carried out the molecular genetic studies, participated in the sequence alignment and drafted the manuscript. JY carried out the immunoassays. MT participated in the sequence alignment. ES participated in the design of the study and performed the statistical analysis. FG conceived of the study, and participated in its design and coordination and helped to draft the manuscript. All authors read and approved the final manuscript.

Contributors who do not meet the criteria for authorship should be listed in an acknowledgements section. Examples of those who might be acknowledged include a person who provided purely technical help, writing assistance, or a department chair who provided only general support.

An author’s contribution section has been added as requested.

Author’s Contribution:
PJEH: conception, design, analysis, drafting and critical revision
SS: conception, design, analysis, drafting and critical revision
BAO: design and drafting
AK: drafting and critical revision
JDP: Design, Analysis, Drafting and critical revision
MMT: Drafting, critical revision and guarantor

4. Please include your funding into the Acknowledgment Section. Place the Acknowledgement section after the Author’s Contribution.

The funding statement has been moved into an Acknowledgement section.

5. Please move the Competing Interest section after "Conclusion Section".

The competing interest statement has been moved to after the conclusion section as requested.
Author’s response to reviews

Title: Variations and inter-relationship in outcome from emergency admissions in England: a retrospective analysis of Hospital Episode Statistics from 2005-2010

Authors:

Peter J.E. Holt  pholt@sgul.ac.uk
Sidhartha Sinha sid261@hotmail.com
Baris A Ozdemir drbaozdemir@gmail.com
Alan Karthikesalingam alankarthi@gmail.com
Jan D Poloniecki sggp300@sgul.ac.uk
Matt M Thompson Matt.Thompson@stgeorges.nhs.uk

Version: 3  Date: 20th April 2014

Author’s response to reviews: see over
Reviewer's report
Title: Variations and inter-relationship in outcome from emergency admissions in England. A retrospective analysis of Hospital Episode Statistics from 2005-2010

Version: 3

Date: 3 March 2014

Reviewer: Rupert Pearse

Reviewer's report:
Very interesting paper and one which will have an important impact because it demonstrates not only important variation but the pattern of that variation across diagnoses. The clinical relevance and topicality is undoubted and not being an expert in the detailed methods, I have few comments. I assume that a health services researcher with experience of using HES data will also review.

Minor Essential Revisions
1. The authors make some reasonable points about the accuracy of HES data. However, many readers from an international audience may not be familiar with the data source and its strengths and limitations. I think the description of the possible flaws in HES analyses could go a little further. I agree that coding is better but it still has some way to go! Some codes are better than others primarily because certain diagnoses/procedures may still fit under more than one code. I don’t see that any change to the methods or results is needed but a minor revision of the discussion would be helpful.

We have expanded the methods section describing HES data for readers unfamiliar with the database and we have added some more detail describing the limitations of the data source and risk-adjustment methodology in the strengths and limitations section.

2. Some thought could be given to the headline findings. In one sentence, what is most important about these data? The current conclusion is a little wordy and doesn't quite bring this out.

We have simplified the conclusions section of the abstract. The key message is that risk-adjusted outcomes vary considerably across England and cross traditional speciality boundaries. This supports the notion that global (generic) institutional infra-structure and processes of care influence outcomes.

Discretionary Revisions
Funnel plots really help to make the argument. Given this is an online open access journal, the authors could provide funnel plots on more of the diagnoses (not all). This will help others to disseminate the work in talks etc.

We have added 2 more figures depicting funnel plots for 1 year mortality and 28-day emergency readmission.
**Reviewer's report**

**Title:** Variations and inter-relationship in outcome from emergency admissions in England. A retrospective analysis of Hospital Episode Statistics from 2005-2010

**Version:** 3

**Date:** 11 March 2014

**Reviewer:** Ian Blunt

**Reviewer's report:**

This is an interesting and extensive piece of work, with a generally robust design. While it replicates work done by the CQC and Dr Foster (and possibly others), these organisations do not (to my knowledge) publish their assessments of variations in mortality. The challenge for the authors will be to frame the implications on their results in terms of the potential and mechanisms for quality improvement.

**Major Compulsory Revisions**

1. The authors need to justify their choice to analyse trusts in quintiles of aggregate mortality rate, instead of reporting on the direct association between aggregate and procedure-specific mortality (possibly censoring for extreme outliers). It would be possible to analyse non-stratified data while retaining quintiles for presentational convenience.

   The choice of quintile analysis was based on published methodology (Dimick et al, Sinha et al). Previous work has studied the direct association between aggregate and procedure-specific mortality and found only weak to modest correlation co-efficients (Dimick et al). As such, we did not feel that repeating these analyses would add to the body of work presented in the manuscript given the amount of data already amassed. Nonetheless, if the editors wish these analyses to be completed, we would be happy to perform these.

2. Whatever method is used to compare condition-specific to aggregate, the aggregate used in each test should not include the contribution from the procedure being tested (eg. hernia mortality should be tested against an aggregate of all mortality rates except those from hernia). While I doubt it will make a difference to the results, it is the formally correct way to approach the analysis.

   This is indeed how we handled the analyses and this has been made explicitly clear in the methods section.
3. The authors should be much more circumspect on reasons for variation, rather than assuming anything not removed by standardisation must be a quality issue. I agree quality of services will have a lot to do with the observed variations, but what other factors might also explain it?

We have acknowledged these concerns in the limitations section. Briefly: it is possible that there are unmeasured confounding factors despite the risk-adjustment methodology such as inter-provider coding variations (which may in fact be a systematic source of error, i.e. bias) or geographical variations in provider infra-structure (such as primary care and social services) not included as regression covariates.

4. It may have been dropped for reasons of length, but I felt that the lack of comment on any association between in-hospital mortality rates and 1 year mortality rates was a major omission. Please try to squeeze something in, even if it is just a sentence.

We appreciate the reviewer’s point. The analysis of inter-relationships between different mortality outcomes (e.g. in-hospital mortality vs 1-year mortality or mortality vs emergency readmission) was not performed as part of this body of work. This is the focus of current research within our unit.
We would add – however – that other researchers have demonstrated such associations using other administrative datasets and we have now added a reference to these studies in the discussion.

5. Likewise, it feels important to consider if there is any association between mortality and readmission rates. Other studies have suggested that hospitals with low mortality rates have higher readmissions (as a consequence of keeping the patients alive when they are sicker).

We acknowledge the reviewer’s point but we would add that this area remains the subject of on-going research with others finding no association between mortality and readmissions for certain high-risk diagnoses (e.g. Krumholz et al, JAMA, 2013). As we have not performed these analyses (see response to comment 4 above), we feel it is outside the scope of the current manuscript to introduce discussion of this otherwise important area of on-going research.

6. Lastly (as mentioned above) the authors should frame the discussion in terms of not just the fact that variation exists, but the potential reasons for the variation and the actions that may be taken to reduce variation (standardised procedures, monitoring, CQC mortality outliers programme etc).
We have added to the discussion section to expand on the potential reasons for variation.

Minor Essential Revisions
7. You should report the total number of first emergency admissions in HES for the period (ie. the pool from which your 2.5 million were drawn).

We have added this data in the form of a reference in the methods section. The reference is a report from the Nuffield Trust by Blunt et al.

8. Please add a little more detail on the method of aggregating mortality rates rather than just a reference.

We have expanded this section of the methods to elaborate on the methodology.

9. Top of page 10, missing “in” in sentence “also seen surgical procedures”.

We have corrected this typographical error.

10. Page 13 “until complete and accurate disease staged data…” – what about the national audits and registries?

We have added in a sentence specifically discussing national clinical registries and the limitations thereof due to non-compulsory submission of data (with appropriate references).

Discretionary Revisions
11. I was curious about whether strengthening the standardisation for co-morbidities (based on data available in HES) would reduce the observed variation. The Charlson score is doing an awful lot of work – would more detailed morbidity measures perform better?

We had previously conducted post-hoc sensitivity analyses excluding Charlson from the regression modelling and noted no differences in the results. This was not included in the first draft of the manuscript to maintain conciseness but we have added a statement to this effect in this revision. The Charlson score has been used many times in contemporary work and has been validated in risk-modelling against clinical databases (also now referenced in the text). As such we do not feel that (nor do we know of any) administrative data derived co-morbidity measures which would be preferable.

12. There are additional factors that might be worth standardising for in your readmission measure. Both distance from home to hospital and a
13. Some of the paragraphs on pages 9 and 10 present numbers from the tables without adding much. It would be useful to add further detail about why you chose to highlight those particular results. What are they telling you? Why are they more interesting than the other results?

The results highlighted were chosen to illustrate – respectively – conditions with particularly high variability in outcomes (e.g. AMI), examples of disparate specialties with variable outcomes (i.e. both surgical and medical) and also conditions which might otherwise be considered “low risk” (such as appendicectomy) if assessed purely by mortality outcomes. The inclusion of all quintile figures as additional supplementary material affords the reader access to all results which will allow them to draw their own further conclusions from the work.

14. I was also left wondering the extent to which there might be associations between the mortality rates for pairs of conditions. For example might the conditions treated under cardiology specialties show similar patterns? Alternatively, might it reveal inconsistencies in coding between trusts?

This was a concurrent aspect of the current body of work carried out by our unit. Pair-wise correlations were assessed using novel statistical methodology including simulation of randomly assigned outcomes. The work demonstrated that there were numerous moderate to strong (r>0.5) positive correlations between pairs of conditions (including between seemingly disparate conditions) – far more beyond those expected by chance alone. There was also a dearth of significant negative correlations which also supports the hypothesis that there are underlying global structures and processes of care underlying clinical outcomes.

Unfortunately including this aspect of the analysis (and the underlying novel methodology) would create a manuscript of excessive length and complexity. If the reviewer or Editors wish to view the results of the pair-wise correlations then we would be happy to supply these (either as appended material for review or as an invited paper). Of note, we would point out that others have carried out similar work (albeit using slightly different methodology) using other administrative datasets and have demonstrated relatively low coefficients (r<0.3) for the majority of pair-
wise correlations except for some surgical conditions (Chassin et al, Health Services Research, 1989).
Reviewer's report

Title: Variations and inter-relationship in outcome from emergency admissions in England. A retrospective analysis of Hospital Episode Statistics from 2005-2010

Version: 3

Date: 18 March 2014

Reviewer: Anthony Laverty

Reviewer's report:

I enjoyed reading this paper, and overall I think it is sound. I have a few points. Really these are all discretionary, but I do think they should improve the paper and make it more accessible to the general reader:

• I think it would be helpful in the abstract to mention that you have used the 20 most common reasons for admission, as the question most people will ask when you list various conditions is “how were they chosen”.

We have elaborated on how these conditions were chosen and included a further eTable (eTable 2) which provides the crude data upon which the choices of emergency groups were made and also adjusted the abstract section accordingly.

• The intro gives the impression that the rationale for this study is in the context of how well a rating could be designed, but the abstract gives the impression it is more about how to improve care.

We would propose that the two ideals are linked – and we have expanded the discussion section to bring the ideas together. Thus “transparent reporting of results with clear elaboration of statistical methodology used to risk-adjust data and identify outliers and peer-review of outcomes in an environment seeking to improve quality of care rather than to ostracize individuals or units is essential”.

• The methods seem sound to me, a few points here-

  o I don’t think I have heard it called the RCS Charlson score (?) I think the general reader would certainly benefit from description of what Charlson represents and how it is derived.

We have expanded the methods and limitations section to describe how this validated modification of the original Charlson co-morbidity score is derived (and provided the appropriate reference).
As it wasn’t explicit I was a little unclear about whether the analysis was at the hospital (site) level, or trust level. This should be clarified.

Analysis was at trust level and we have made this explicitly clear in the methods section. Given that the concept of a “trust” is a unique feature to the NHS, we have used the term “hospital” synonymously with the term “trust” throughout the manuscript in order to cater to a global readership.

I also was not clear how you made the division into medical vs surgical procedures, and I think there are a few ways you could do this. This should also be clarified.

Medical conditions were defined by ICD-10 codes whilst surgical procedures were defined by OPCS-4 codes. This has been made explicitly clear in the revised manuscript.

- I think language such as “High-risk diagnoses such as sepsis” should be avoided in the results (unless this is a defined category of high-risk – I don’t think it is).

The term “high-risk” is meant to denote those with a high risk of death (or equivalent poor outcome). Nonetheless – especially as the basket of 20 conditions could all be considered “high-risk” (particularly given how they were selected) – we take the reviewers point and we have removed this text.

- You refer in the discussion to your use of superspells, but you don’t say this in the methods. Again, I think you should just clarify this for readers not very familiar with HES.

We have added to the methods section explaining this terminology along with suitable references.

- I found the discussion of developing a rating interesting, but as with much discussion of ratings, there isn’t any mention of why we would do this. You say it could be supported as there is evidence of variation, but do we expect ratings to change this? Perhaps the question here is do we expect ratings to improve care: through some vague notion of patient choice, or by simply pushing providers to improve.

We have elaborated on this in the revised discussion. Prevailing opinion is that patients do not typically behave in a consumerist fashion when choosing healthcare providers. The impetus is therefore quality
improvement through good governance, transparent reporting and peer-review of outcomes and an environment which seeks to foster change rather than to lay blame or stigmatise outlier units.

There is discussion of the trade-off between a whole hospital rating and using more detailed data, but if the idea is to rely on patient choice, perhaps this would confuse patients more.

Please see our response to the comment above.

- You say that using “a group of commonly encountered medical and surgical pathologies and ensured a plausible link between mortality and quality by including patients with conditions amenable to salvage” and also that this means that there is a relationship between process and outcome. I am not quite convinced about this – just because the conditions are common it does not mean that they are “amenable to salvage” I think it is a strength that you have included common conditions because they make up a large number of admissions, and this gives the study good power to detect differences, and in practical terms affect a lot of people.

We have added to the discussion section elaborating on what we mean by the term “amenable to salvage”. In summary, the groups chosen would tend not include cases of terminal illness or palliative care where a poor outcome may be inevitable despite high-quality care. Appropriate references have also been provided. We do – however – also agree with the reviewer that a major strength of the study is the large numbers of patients in each of the groups.

I think the discussion of process vs. outcome is more complex than you mention here, but you have used what is probably the best data around at the moment and used some important outcomes.

We thank the reviewer for their supportive comment. We have additionally expanded on the issue of structure and process in the discussion.

- Also, I think a big strength is the linkage to out of hospital mortality, but you don’t seem to have mentioned this here (!)

We have now added a sentence mentioning this to the discussion.

- In table 1 I would remove the capital letters at the top of each column, and add SDs after those means and %. Also, for Charlson, the final category (I presume) is 3+ rather than just 3? I would also mark which of your deprivation categories is the most/least deprived.
We have added SEs for patient-level variables to Table 1. We would point out that the results in Table 1 are averaged over patients and not trusts. The SD of a dichotomous variable for a single patient (such as gender or death) is not a useful concept. On the other hand, the SE suggests what the precision of the proportion is since +/- 2SE provides an approximate 95% confidence interval. The very small SEs indicate that – due to the large sample sizes – the values are very precise.

The methods section now includes a more detailed description of the Charlson scoring and social deprivation quintiles such that the answers to the reviewer’s questions can now be found there. In summary however, the highest score that can be achieved in the RCS Charlson score is 3 (in effect this is the same as “3+” because a patient with 4 flagged co-morbidities would score 3 as would a patient with (say) 6 flagged co-morbidities). Social deprivation quintile 5 represents the least deprived. We have elected to retain the abbreviations as these provide a link to understanding the abbreviations used in the supplementary eTables and eFigures. If, however, the reviewer or Editors feel particularly strongly about this point then we would be willing to delete them.

• I think tables 3 to 5 could be made clearer by replacing the +/- in favour of parathenses. Also, I think the general reader would find SDs more informative than SE.

We have made this change to parentheses.

We have elected to retain SEs rather than convert to SDs. In addition to the points made for the preceding comment (regarding SDs and SEs for Table 1), we wish to preserve homogeneity in the presentation of the results with a contemporary paper which we have published and which is referenced in the current manuscript (Sinha et al, Inter-relationship of procedural mortality rates in vascular surgery in England, Circulation Cardiovascular Outcomes and Quality, 2014).

Furthermore we would add that not only can the SD be derived from SE (by dividing by the square root of the sample size), the SE (rather than the SD) allows approximation of a 95% CI and the p-values derived from the logistic regression are the key results relevant to the quintile analyses. As such we feel that the request to change all of the SEs to SDs – which would necessitate re-drawing of all of the figures and tables – is excessive given the gains it would produce.
We have clearly indicated that the number being quoted is the SE (rather than the SD or the 95% CI) and as such we hope that this is sufficiently clear for readers not to misinterpret.

• Finally, I am not sure if these 3 very similar tables would be better if some of them were moved to the appendix. Perhaps one of your eFigures would be better in the main paper and the numbers behind them in the appendix. I appreciate that this may come down to space requirements etc however.

We thank the reviewer for their comment. At present, Tables 3-5 are all included as eFigures given space constraints. We have retained the current layout of tables and figures (albeit with 2 additional figures in the main manuscript) as we feel this is the most uniform and space-conscious way of presenting the data. If, however, the reviewer or the Editors feel particularly strongly about this decision then we would be willing to make this change.