PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Risk factors for admission at three, urban Emergency Departments in England: a cross-sectional analysis of attendances over one month
AUTHORS	Ismail, Sharif; Pope, Ian; Bloom, Ben; Catalao, Raquel; Green, Emilie; Longbottom, Rebecca; Jansen, Gwyneth; McCoy, David; Harris, Tim

VERSION 1 - REVIEW

REVIEWER	SUZANNE MASON UNIVERSITY OF SHEFFIELD UK
REVIEW RETURNED	03-Mar-2016

GENERAL COMMENTS	This study included 3 EDs from London and analysed patient level data from one month in 2013 along with some data on organisational factors that they felt led to differences in emergency admission rates. The authors explored patient related factors along with organisational factors in a binary logistic modelling process. I have a number of comments / concerns over the paper addressed below in no particular order: 1. The use of 3 closely situated EDs from inner London will limit the generalisability of the findings from this study - this would appear to have been a convenience sample rather than derived for any particular purpose. 2. As the authors state I was surprised that a short period of time was selected - December 2013, which is not only very short, but notoriously one of the months when EDs tend to perform the worst 3. It would have helped to outline in table 1 the number of annual attendances at each of the EDs. 4. Table 2 gives no measure of patient acuity - whilst I accpet the difficulties in coding causing problems in diagnosis etc - is there a way of extracting triage scoring / NEWS or MEWS scores from records in order to identify the levels of acuity patients presented with? 5. Workforce - I was surprised that the authors only included consultant working hours - they did not present a hypothesis / rationale for doing this and I wondered what it was? Did the consultants in each ED operate a system such as senior doctor triage or senior sign off that would make their presence notably different in the ED? If not, why Did they only include this data? It is widely known that other staffing factors have an influence on patient flow and outcomes. The lack association with consultant availability may reflect that other staffing factors were important in this. 6. Site 2 seems particularly anomalous with very high admission rates - is this something to do with the demographic of patients it serves - is this a standard type I ED?

	I was concerned that the measures identified and used in the regression would have been inter-correlated. I was not sure what process the authors followed in relation to identifying variable for use in the regression analysis - they appear to have included all of them rather than looked at where there were high levels of correlation and perhaps considered excluding some prior to the regression - e.g. Moving in the last 10 mins before 4 h and LWBT and ED bed occupancy rates? 7. Some results from a qualitative analysis referred to and not referenced were mentioned in the discussion - I felt this was inappropriate given that these results had not been presented before in the paper and there was no peer reviewed reference to give for this analysis 8. Overall this was an interesting paper and analysis. However I was left wondering what the authors had found - the patient related factors of deprivation, ethnicity etc have all been described before and do not represent new information. The organisational factors were difficult to interpret and did not appear to explain the differences observed.

REVIEWER	Win Sen Kuan Emergency Medicine Department, National University Hospital, Singapore
REVIEW RETURNED	28-Mar-2016

GENERAL COMMENTS	Many thanks for the opportunity to review your work that investigated possible factors associated with unscheduled admissions following presentation to 3 inner London EDs; looking at 2 different models adjusting for patient demographic and arrival mode (Model 1), and staffing and workload of the ED in addition to Model 1 (Model 2). The manuscript is generally well-written and easy to follow. The background information provided in the Introduction is comprehensive, insightful and critically identifies the gap in the literature. However, the reason for selection of the month of December 2013 for analysis is not apparent. A statement on institutional review board review for waiver of informed consent should be provided. What was the rationale of grouping the patients into the 3 age groups? Was there any training done for the case abstractors and counter-checking of the data collected? Table 2 provides useful summarised information of the variables collected. For Table 3, please consider if the variables listed were normally distributed. If they had skewed distribution, perhaps median (IQR) would be a better summary measure. Would the authors be able to furnish more information on the inpatient characteristics of the hospitals e.g. number of beds, ICUs etc. to give a better understanding of each Site?

Could the authors explain the differences in age and ambulance arrivals for Site 2 compared with the rest? Was the population that Site 2 catered to different from the others?

The recognition of the lack of diagnosis is important in this study that highlights areas for improvement, but unfortunately is also a great disadvantage to this study, not being able to infer this critical information to the outcome. This would make Figure 1 less useful and I would suggest its removal from the manuscript.

I would like to clarify with the authors on the information in "Time of arrival" (Table 4). Were there really much larger numbers who arrived on weekends (2 days) compared to weekdays (5 days)?

Abbreviations and acronyms should be elaborated in the footnote of the tables e.g. IMD score, ACSC, LAS, LWBT.

Did the authors consider whether there were any differences between surgical and non-surgical unscheduled admissions?

Most of the limitations in this study were well-discussed.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1
1. The use of 3 closely situated EDs from inner London will limit the generalisability of the findings from this study - this would appear to have been a convenience sample rather than derived for any particular purpose.
The reviewer is correct to identify this as a potential limitation in terms of generalisability and we have added comments to this effect in the discussion (page 14). However, the use of three sites in close geographic proximity and serving similar populations enables the study to overcome - to some extent - the difficulties engendered by not being able to include diagnosis data.
2. As the authors state I was surprised that a short period of time was selected - December 2013, which is not only very short, but notoriously one of the months when EDs tend to perform the worst
We thank the reviewer for this comment. Additional comments have been included in the method (page 5) and the discussion (page 15) to clarify that this was a convenience sample, and to acknowledge the volatility effects that one might expect when analysing attendances in December. Unfortunately because of the considerable amount of labour involved in extracting and cleaning data and the short amount of time available for this work, it was not possible to include further months – although this was part of the project plan initially. In the discussion we recommend that future studies incorporate data covering a longer time period.
3. It would have helped to outline in table 1 the number of annual attendances at each of the EDs.
Data on total ED attendances at the three sites for the period October 2012-September 2013 have now been included in table 1 to give an indication of load at the three sites in the immediate run-up to the study period.
4. Table 2 gives no measure of patient acuity - whilst I accpet the difficulties in coding causing problems in diagnosis etc - is there a way of extracting triage scoring / NEWS or MEWS scores from records in order to identify the levels of acuity patients presented with?
We thank the reviewer for this comment – and recognise the limitations imposed on this study by the absence of diagnosis and acuity data. It was unfortunately not possible to extract triage scores without accessing individual patient records - which was impractical in the time period available for this study, as approaching 20,000 attendances were eligible for inclusion for December 2013 alone. However, we acknowledge this limitation and have included a comment to highlight it as a shortfall in the method section (page 5), as well as in the strengths and limitations section of the discussion.

5. Workforce - I was surprised that the authors only included consultant working hours - they did not present a hypothesis / rationale for doing this and I wondered what it was? Did the consultants in each ED operate a system such as senior doctor triage or senior sign off that would make their presence notably different in the ED? If not, why Did they only include this data? It is widely known that other staffing factors have an influence on patient flow and outcomes. The lack association with consultant availability may reflect that other staffing factors were important in this.

We are grateful to the reviewer for highlighting this – an important issue. We gathered information on junior doctor staffing in these departments during the study period but the complexity of rotas and in particular variations in the number of junior doctors on the floor at any given time point made it impossible to code this into a variable that could manageably be included in the analysis. For consultant staffing this was more straightforward, and we opted to include a simple, binary variable for presence or absence of at least one consultant since there was a clear distinction between the three sites here (site 1 is 24-hour consultant staffed; the other two are not).

Notwithstanding the limitations of an analysis of workforce looking at consultant presence alone, we felt this finding merited inclusion, because of the statistical significance of the result and in highlighting the need for further analysis of staffing factors in future work. We have included a comment to this effect in the discussion.

6. Site 2 seems particularly anomalous with very high admission rates - is this something to do with the demographic of patients it serves - is this a standard type I ED?

Site 2 is a standard type 1 ED. All urgent case presentations at the three sites were excluded from the dataset prior to analysis.

The reviewer is correct to point out an anomalously high rate for site 2 – which we are unable to fully explain. In response to reviewer comments on a related analysis we have disaggregated elderly (65-84) from "oldest old" (85 and over) patients for this revised paper, and can now demonstrate a clear difference in proportions of presentations by age group especially for site 2 – with a much greater proportion in the "oldest old" category compared with sites 1 and 3. However, these and other demographic factors were controlled for in the regression analyses.

The project of which this analysis formed part included an in-depth qualitative analysis (which we have submitted to BMJ Open as a separate but accompanying piece) which identified departmental culture differences (particularly at site 2) as potentially important in explaining variations in observed admission rates across the three sites – and we feel these factors may merit further exploration as drivers of differing admission behaviour.

7. I was concerned that the measures identified and used in the regression would have been inter-correlated. I was not sure what process the authors followed in relation to identifying variable for use in the regression analysis - they appear to have included all of them rather than looked at where there were high levels of correlation and perhaps considered excluding some prior to the regression - e.g. Moving in the last 10 mins before 4 h and LWBT and ED bed occupancy rates?

We thank the reviewer for highlighting this. We have revisited the analysis for these variables both in descriptive terms and for model 2 and made some adjustments. First, we ran collinearity diagnostics on the workload variables in SPSS, and – despite using stringent tolerance and Variance Inflation Factor thresholds (0.3 and 3 respectively most studies in the literature use thresholds of 0.1 and 10 respectively) – were unable to find significant evidence of multicollinearity between them. Correlation between moving in the last 10 minutes before the 4 hour target, and the other variables would not have been expected because the former is a categorical variable with just two categories.

However, as we note in the introduction (page 4), evidence on the impact of various workload factors on admission risk elsewhere in the literature is conflicting, although higher inpatient bed occupancy may reduce the probability of admission. So although we initially included all variables, a backward elimination approach was then used to include only those variables with significant effects – as now listed in a revised version of table 5.

8. Some results from a qualitative analysis referred to and not referenced were mentioned in the discussion - I felt this was inappropriate given that these results had not been presented before in the

paper and there was no peer reviewed reference to give for this analysis

We accept this criticism. However, the qualitative analysis is also under review with BMJ Open (the two manuscripts were submitted as linked pieces) and having queried the appropriateness of this approach with the journal editors have been advised that we should cross-reference the manuscripts. To try to meet the reviewer's concerns, the qualitative analysis is referenced in the introduction (reference no 22) and we have now amended the relevant section of the discussion to explicitly reference this study.

Reviewer 2

1 The background information provided in the Introduction is comprehensive, insightful and critically identifies the gap in the literature. However, the reason for selection of the month of December 2013 for analysis is not apparent.

See response to point 2 from reviewer 1 above.

2. A statement on institutional review board review for waiver of informed consent should be provided. This has now been included on page 16.

3. What was the rationale of grouping the patients into the 3 age groups?

We thank the reviewer for highlighting this point: the rationale for our approach is now outlined further on page 5. Our decision-making was guided partly by model parsimony (to keep the number of categories within each categorical variable to a minimum), but principally by clinical factors – notably the increased risk of emergency admission among older patients. In response to reviewer comments on a related manuscript (from the same project), we have now disaggregate the age bands further to include an additional, age 85+ group to capture risks relating to the "oldest old" among admissions. The rationale for doing so is that admission risk in this group is known to be particularly high.

4. Was there any training done for the case abstractors and counter-checking of the data collected?

Training was provided by the second author (the lead for this project) to those involved in the data extraction. Reference to this has now been made in the method section (page 5).

5. For Table 3, please consider if the variables listed were normally distributed. If they had skewed distribution, perhaps median (IQR) would be a better summary measure.

We thank the reviewer for this comment. In a dataset of this size, simple tests of normality are difficult to interpret. Skewness and kurtosis values for all of these variables were in the band -1 to +1 with the exception of "Left Without Being Seen" which had high values for both. Formal testing (Kolmogorov-Smirnoff tests and Q-Q plots for the variables) indicated that all variables were non-normally distributed, but these results should be interpreted with caution.

On this basis LWBS intensity was certainly non-normal but there is uncertainty about the other variables, especially ED bed occupancy. For consistency, the figure in table 3 have been adjusted to report median and IQR figures as suggested by the reviewer since at least one was definitely non-normal.

For the purposes of the binary logistic regression analysis, however, non-normality of predictor variables should not affect model fitting.

6. Would the authors be able to furnish more information on the inpatient characteristics of the hospitals e.g. number of beds, ICUs etc. to give a better understanding of each Site?

Further site details have been provided in table 1 - giving the total number of inpatient (acute) and ICU/HDU beds at each site.

7. Could the authors explain the differences in age and ambulance arrivals for Site 2 compared with the rest? Was the population that Site 2 catered to different from the others?

Please see response to comment 6 from reviewer 1 above. This is partly the result of local differences in population structure, and in particular the slightly older population (with a higher proportion of people of white ethnicity) served by site 2 when compared with the other two sites (9% of presenting patients were in the 85 and over age category by comparison with 2% and 3% at sites 1 and 3 respectively). We have commented on these broad differences on page 8 to link to the increased proportion of attendances by ambulance at this site compared with the other two.

8. The recognition of the lack of diagnosis is important in this study that highlights areas for

improvement, but unfortunately is also a great disadvantage to this study, not being able to infer this critical information to the outcome. This would make Figure 1 less useful and I would suggest its removal from the manuscript.

We thank the reviewer for this comment – the figure and accompanying text have now been removed.

9. I would like to clarify with the authors on the information in "Time of arrival" (Table 4). Were there really much larger numbers who arrived on weekends (2 days) compared to weekdays (5 days)?

Apologies – this was in error (the rows were the wrong way round). This has now been corrected in the draft.

10. Abbreviations and acronyms should be elaborated in the footnote of the tables e.g. IMD score, ACSC, LAS, LWBT.

This has now been amended in the legends for each table in the draft.

11. Did the authors consider whether there were any differences between surgical and non-surgical unscheduled admissions?

We did not explicitly consider this as part of this analysis although we acknowledge that this would have provided an additional avenue to pursue with the data. We feel that a dedicated analysis looking at distinctions between surgical and non-surgical admissions is probably beyond the scope of this paper but will investigate whether we can pursue this as a separate analysis.

VERSION 2 – REVIEW

REVIEWER	Win Sen Kuan Emergency Medicine Department, National University Hospital, Singapore
REVIEW RETURNED	08-Jun-2016

GENERAL COMMENTS	Thank you for the revision of the manuscript. The authors have sufficiently addressed my previous comments. I have no further comments to add.