

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Associations between Work Satisfaction, Engagement, and 7-day Patient Mortality; a Cross-Sectional Survey
AUTHORS	Brubakk, Kirsten; Svendsen, Martin; Hofoss, Dag; Hansen, Tonya; Barach, paul; Tjomsland, Ole

VERSION 1 – REVIEW

REVIEWER	Chris Sherlaw-Johnson Nuffield Trust, UK
REVIEW RETURNED	04-Jun-2019

GENERAL COMMENTS	<p>This is a useful study analysing relationships between staff survey responses relating to their work environment and outcome. However, there are a few points I think the authors need to consider:</p> <ol style="list-style-type: none">1. The structure of the paper is imbalanced with a very short Results section. I would normally expect reporting of response rates as well as the descriptive statistics described in the “clinical data” subsection to appear in the Results rather than Methods.2. The authors explain why they have chosen 7-day mortality as their outcome measure. There are limitations in using this since many deaths would be unavoidable, particularly early deaths from AMI and stroke. Also, it means this analysis probably only works when dealing with higher mortality conditions. Can they suggest other measures that would be useful, particularly with lower risk patients?3. The authors recognise in the Discussion that they cannot rule out the possibility that the correlations are non-causal. I think this is very important. However, when explaining the results in the abstract they mention the impact of one standard deviation shifts in scores on mortality. This is only relevant if the effect is causal (and linear – which they don’t test). Some of the negative findings might be presented in the abstract instead. Also, in the results, they refer to superior management engagement as a “predictor” of mortality, which again assumes causality.4. Can the authors present survey response rates by occupation?5. It would be useful to have more explanation as to how the mortality rates were created including how the three conditions were combined and adjusted for.6. In Table 5 could they explain the headings “nurse adjusted” etc. and the term “diagnosis specific”.7. Also, in the results, the directions of the relationships between environmental factors and mortality are not clear. I assume a statistical reader can work it out from the tables, but I think these need to be clearer to the more general reader.8. There are some problems with language. One of the most notable is the use of the word “insidious” in the description of
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	Objective 1 in the Results, which I doubt is what they mean. I would suggest they remove that word and not replace it as whatever they put there could appear as a value judgement.
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REVIEWER	Peter Griffiths University of Southampton
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REVIEW RETURNED	27-Jun-2019
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GENERAL COMMENTS	<p>This is a potentially interesting study that could add something to what we know about the correlates of patient safety. However I find the methods and some aspects of the reporting rather unclear which currently makes it impossible to judge the validity of the study. The statements of objectives for the study could also do with revision to ensure clear alignment throughout.</p> <p>Specific comments / suggestions:</p> <p>Can you clarify how you arrived at a score for each of the named factors? Can you provide more clarity on the ward-specific mortality rate. Did all wards only have a single group of patients? If not how were the different mortality rates combined? What about patients who spent time on more than one ward? At what point and how did you risk adjust? In the final regression? It is not clear at all. You appear to calculate a relative mortality rate for the wards based on the diagnostic group. How are the risk adjustment factors incorporated? Why not calculate a standardized mortality ratio based on observed vs expected from a regression model including diagnosis and comorbidities etc.?</p> <p>Analysis – please be explicit about all the variables included in the backwards regression models. How were variables selected for elimination? What variables were included in the final models? What regression procedure did you use? What is the unit of analysis – ward? If so how were the patient risk adjustment factors you identified included?</p> <p>It would be useful to report some more descriptive data about your sample to help understand the nature of the data.</p> <ul style="list-style-type: none"> • How many staff responses per ward (mean and range)? • How did you link middle managers and doctors to wards? • How many patients per ward (mean and range)? <p>More minor points P4 “In other words, organizational culture may be a fundamental determinant of patient outcomes” – Patients age and condition are potentially fundamental determinants, the treatment they receive may influence variation.... Organisational culture is not a fundamental determinant...</p> <p>Objective: “Examine the impact of professional roles and the insidious influence of the organizational environment on seven-day patient outcomes”</p> <p>This is not really all that clear and it is certainly unclear in why the influence is described as ‘insidious’. I suggest revising. All</p>
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	<p>statements of objectives should align – this differs from that in the abstract.</p> <p>Methods – you talk about work environment and patient safety culture, which seems to imply you see these as distinct constructs. My reading would be that patient safety culture is part of the psychosocial work environment which is measured by just two items on your questionnaire. I would question whether what these items measure is really safety culture – “I would feel safe if I was a patient here” seems more an evaluative statement of outcome. As a matter of clarity, it might be better to talk about work environment as the overarching construct then the specific aspects of this that you study – including those items that you call “safety culture”.</p> <p>In your results, you report results under two objectives. The second had not been previously mentioned.</p>
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REVIEWER	Walter Sermeus KU Leuven, Belgium
REVIEW RETURNED	03-Jul-2019

GENERAL COMMENTS	<p>Interesting manuscript but still many questions</p> <ol style="list-style-type: none"> 1. The claim that this study is the first study in linking Norwegian hospital staff survey data with patient mortality data is not true. There was the RN4CAST-study (reference 40) with Tvedt et al reporting these relationships already in 2014. The Aiken et al publication in the Lancet (2014) on RN4CAST results was also including Norwegian data 2. On p.5 I the aim of the study is listed. I would expect here the two research questions, that are listed further in the manuscript. 3. Setting and study design might be more developed. There are two data sources: the hospital staff survey and the patient mortality data. <ol style="list-style-type: none"> a. For the hospital staff survey, the data are from 3 years (2010-2012). It is unclear how these data are collected and which data are precisely available. They are described later, but all info should be together. I presume that the data are available on the provider level and that data are available about the hospital they are working. But are also the ward data available? b. The mortality data are less well described. Who is collecting these type of data? Is it the Norwegian Institute of Public Health? What data are precisely available? The risk adjustment is described later, but should be described here. c. How the data were made available for the study? How are the two databases linked. The authors say that they are linking on ward level? How did they do it? What data were available? I don't see how they did it. d. So databases, available data and linkages should be described precisely in this section 4. Next section describes the selection of patient groups. Why these patients groups were selected? How were they identified? Later (p7) these codes were described, but we see here a mix of diagnostic codes and procedure codes. Some other criteria are used such as age (e.g. >age 65?, only emergency admissions?). All criteria for selection of patient groups should be listed here in detail. 5. The selection of hospitals and wards should be better documented. I only read in the abstract that there are 20 Hospitals. Selection should be described. There are 56 wards. I
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	<p>don't know exactly what to understand about wards. Hip fracture patients are admitted on orthopaedic wards. But not all patients on orthopaedic wards are hip fracture patients. Did the staff (doctors, nurses) worked on these dedicated wards. How do we know from the available data? We have 20 hospitals and 3 patients groups: $20 \times 3 = 60$ but only 56 wards. What does it mean? Does it mean that some hospitals don't have one of the patient groups e.g. no stroke unit, no STEMI unit? Please explain.</p> <p>6. The questionnaire is well described.</p> <p>7. The clinical data on p7 is a mix of methods and results and should just describe the different variables, how there are coded</p> <p>8. Statistical analysis should be further elaborated. The problem is that mortality is known on the patient level. The staff data are on the organizational level (meaning staff data aggregated on the level of hospital*patient group). It is confirmed by the authors on p9. This requires a multilevel analysis approach on which organizational level data are related to patient level data. I don't see in the analysis plan this kind of approach. I don't know how they have addressed the relationships. A second difficulty is that doctors, nurses, middle management evaluate the working environment of a ward (what I still assume as hospital*patient group). The same working environment might be perceived differently by the various professional groups. What does this say over the working environment? And how to address this in the equations? Just to give an example: the questionnaire doesn't address interprofessional collaboration, but in reality they work in teams and individual perceptions should be viewed in this respect.</p> <p>9. On page 8 the 2 research questions are formulated. Both questions are very similar. It would be useful to have a first descriptive question in describing work environment in the 20 hospitals and 3 patient groups. The second question could then focus on the relationship between organizational characteristics and patient mortality. The results are not well described. In table 5, it is very unclear what means "nurse unadjusted", "physician unadjusted", "managers unadjusted".</p> <p>10. Too many questions to even read the discussion and conclusion</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

1.1 The structure of the paper is imbalanced with a very short Results section. I would normally expect reporting of response rates as well as the descriptive statistics described in the "clinical data" subsection to appear in the Results rather than Methods.

Answer: Based on the very relevant feedback we have revised the structure accordingly with moving data to the "Results" section and increased its length accordingly.

1.2 The authors explain why they have chosen 7-day mortality as their outcome measure. There are limitations in using this since many deaths would be unavoidable, particularly early deaths from AMI and stroke. In addition, it means this analysis probably only works when dealing with higher mortality conditions. Can they suggest other measures that would be useful, particularly with lower risk patients?

Answer

Although the overall hospital mortality is low (approximately 2%), data show that 70-80% of these are older multi-morbid patients dying after emergency admission, 2% are patients admitted with “low risk” conditions, whereas 20% are patients admitted with AMI, stroke and hip-fracture – conditions with high mortality. According to our data approximately 20-, 30- and 60% die after discharge within 30 days following hospital admission for AMI, stroke and hip-fracture respectively. Furthermore, it has been demonstrated that the moderate correlation between in-hospital and 30 days mortality do not apply for patients admitted with stroke and hip fracture. Mortality post discharge might be caused by factors that may not be impacted by the hospital work-environment monitored in the present study – accordingly patients admitted with these conditions and 7 days mortality were chosen in the present study.

Indeed, one of the limitations of our study is that the severity of the illness is not available in the Norwegian Patient Administrative system; this means that we do not know which patients die of their diagnosis or because of sub-optimal care. In this study, lower risk patients were not included. However, measures such as 30-day mortality, number of adverse events, patient complaint rates or Global Trigger Tool might be useful parameters in this context.

1.3 The authors recognise in the Discussion that they cannot rule out the possibility that the correlations are non-causal. I think this is very important. However, when explaining the results in the abstract they mention the impact of one standard deviation shifts in scores on mortality. This is only relevant if the effect is causal (and linear – which they don't test). Some of the negative findings might be presented in the abstract instead. Also, in the results, they refer to superior management engagement as a “predictor” of mortality, which again assumes causality.

Answer: We have removed the suggested parts implying causal and linear effects as we agree that this might be misleading.

1.4 Can the authors present survey response rates by occupation?

Answer: We have the exact number of surveys sent to eligible staff. However, stating their profession is voluntarily and 78% chose to do so. This makes it difficult to produce the response rate per profession at the ward level.

1.5 It would be useful to have more explanation as to how the mortality rates were created including how the three conditions were combined and adjusted for.

Answer: The mortality rates were provided by the Norwegian Knowledge Centre for the Health Services. These data are published annually as part of the Norwegian Quality Indicator System as survival probabilities. A complete description on the creation and adjusting of the data is available in the paper “30-day survival Probabilities as a Quality Indicator for Norwegian Hospitals: Data Management and Analysis” by Sahar Hassani et al. PLoS ONE 10(9): e0136547. Doi:10.1371/journal. In addition we have expanded and clarified on the calculation, combination and adjustment of the mortality rate in the method section.

1.6 In Table 5 could they explain the headings “nurse adjusted” etc. and the term “diagnosis specific”.

Answer: Thank you for making us aware of this, we agree that this might be confusing. This is changed in the current version of the paper and the headers and table titles and subtitles are updated.

1.7 Also, in the results, the directions of the relationships between environmental factors and mortality are not clear. I assume a statistical reader can work it out from the tables, but I think these need to be clearer to the more general reader.

Answer: The header and title have been updated to clarify that we have focused on the association between environmental factors and mortality by linear regression with mortality as outcome.

1.8. There are some problems with language. One of the most notable is the use of the word “insidious” in the description of Objective 1 in the Results, which I doubt is what they mean. I would suggest they remove that word and not replace it as whatever they put there could appear as a value judgement.

Answer: We agree and have removed the word “insidious” which did not describe our intention.

Reviewer: 2

2.1 Can you clarify how you arrived at a score for each of the named factors

Answer: We have included another sentence in the method section clarifying this: “The score on each dimension were calculated as the mean of the score on each item included in the dimension.”

2.2 Can you provide more clarity on the ward-specific mortality rate. Did all wards only have a single group of patients?

Answer: We have rephrased and adjusted the paper to make this clearer. The work environment of the nurses employed at the wards having the responsibility for each of the three patient groups, physicians responsible for the patient groups and the managers responsible for managing the wards at each hospital were connected to the mortality of the specific patient groups.

2.3 If not how were the different mortality rates combined?

Answer: We have added info on this to make it clearer. Mortality rates were not combined but a relative (to the patient group) mortality rate was calculated for each patient group at each hospital.

2.4 What about patients who spent time on more than one ward?

Answer: The patient data provided by NOKC are adjusted for time spent at ward and hospital. (see ref 30 in the manuscript)t. Furthermore, it has been clarified in the method section.

2.5 At what point and how did you risk adjust?

Answer: Please see previous answer.

2.6 In the final regression? It is not clear at all.

Answer: Please see previous answer.

2.7 You appear to calculate a relative mortality rate for the wards based on the diagnostic group. How are the risk adjustment factors incorporated? Why not calculate a standardized mortality ratio based on observed vs expected from a regression model including diagnosis and comorbidities etc.?

Answer: See previous answers. In addition, we clearly see that the phenomena could be studied in a number of ways, but our data were available risk adjusted at a hospital ward level and not as individual patient records. The design is a result of that fact, which we understand we did not manage

to make clear enough in the first version. We think this input have made the paper better and hope the readers will have better understanding of our analysis after our adjustments.

2.8 Analysis – please be explicit about all the variables included in the backwards regression models. How were variables selected for elimination? What variables were included in the final models? What regression procedure did you use? What is the unit of analysis – ward? If so how were the patient risk adjustment factors you identified included?

Answer: Thank you for the input, we agree that this was not clear enough and have included the following in the statistic analysis section: “The associations between the profession-specific work environments and adjusted patient mortality rates at a specific ward level were analyzed using a linear regression model that was adjusted for diagnosis and the annual number of treatments.” and “A backward conditional regression analysis was performed by including all the significant work environmental factors from the initial separate analysis (Nurses: Patient centered, Respect, Motivation, Engagement, Commitment, Role expectations, Workload, Autonomy, Role conflicts, Sick leave, Leadership, Patient safety climate; Physicians: None; Managers: Quality, Motivation, Engagement, Commitment, Personal development, Empowerment, Social climate, Workload, Role conflicts). The level for removal of variables was set to $P > 0.05$.”

2.9 It would be useful to report some more descriptive data about your sample to help understand the nature of the data.

- How many staff responses per ward (mean and range)?
- How did you link middle managers and doctors to wards?
- How many patients per ward (mean and range)?

Answer: To get a better understanding of the data we have elaborated on how we linked managers and physicians to the wards and have included descriptive of responses and patients at the wards. Description of method also added to statistical analysis. “Descriptive data on number of patients treated and survey responses were given as median and range due to not being normal distributed. Normality were tested by Kolmogorov–Smirnov test.”, “The median number of treatments for the 56 wards that participated in this study, (patients within the diagnosis codes included) were 214, ranging from 36 to 1242. The median number of work environment survey responses per ward included in the analysis was 87 (range 26-296) for nurses, 32 (range 5-157) for physicians, and 15 (range 5-47) for managers.” and “Whereas nurses typically worked on one hospital ward and responses were allocated to the specific ward, physicians and middle managers typically were covering several wards or units. The assessment of their work environment is not the physician or manager perceptions of the patient ward that are being measured, but the explicit perceptions about the work environments where physicians and middle managers work (entire clinical departments) supporting these patient wards.”

2.10 P4 “In other words, organizational culture may be a fundamental determinant of patient outcomes” – Patients age and condition are potentially fundamental determinants, the treatment they receive may influence variation.... Organisational culture is not a fundamental determinant.

Answer: We do not fully agree that organizational culture is not a fundamental determinant. As we see it, both patients and their characteristics and characteristic of the environment where treatment and care is given can be determinants (of outcome).

Objective: “Examine the impact of professional roles and the insidious influence of the organizational environment on seven-day patient outcomes”

This is not really all that clear and it is certainly unclear in why the influence is described as 'insidious'. I suggest revising. All statements of objectives should align – this differs from that in the abstract.

Answer: Thank you for your comment. We have revised our objective and deleted the word "insidious".

2.11 Methods – you talk about work environment and patient safety culture, which seems to imply you see these as distinct constructs. My reading would be that patient safety culture is part of the psychosocial work environment which is measured by just two items on your questionnaire. I would question whether what these items measure is really safety culture – "I would feel safe if I was a patient here" seems more an evaluative statement of outcome. As a matter of clarity, it might be better to talk about work environment as the overarching construct then the specific aspects of this that you study – including those items that you call "safety culture".

Answer: We agree that it is a challenge to measure patient safety culture with only two items in the staff survey. It was the choice of the South Eastern Norway Regional Health Authority to include the chosen items on culture and not the complete validated Safety Attitudes Questionnaire (SAQ). However, we do not agree that patient safety culture is only part of the psychosocial work environment. Work environment is also affected by resources made available to the ward, such as budget and staffing. In our opinion, patient safety culture is more than only aspects of the psychosocial work environment. It could be defined as the product of individual and group values, attitudes and values that staff share in relation to risk in an organization and thus in our opinion more than part of the overarching construct.

2.12 In your results, you report results under two objectives. The second had not been previously mentioned.

Answer: Thank you for pointing this out. This is now changed.

Reviewer: 3

3.1 The claim that this study is the first study in linking Norwegian hospital staff survey data with patient mortality data is not true. There was the RN4CAST-study (reference 40) with Tvedt et al reporting these relationships already in 2014. The Aiken et al publication in the Lancet (2014) on RN4CAST results was also including Norwegian data

Answer: Thank you for preventing us from making wrongs claims. This was not intended. However, the mentioned study used HSMR similar mortality data and staff survey data on hospital level As far as we know, this study is the first to combine profession specific Norwegian survey data with diagnose-specific patient mortality data at the ward level. We have made sure to cite both of these important references relating to Norwegian data.

3.2 On p.5 I the aim of the study is listed. I would expect here the two research questions, that are listed further in the manuscript.

Answer: We fully agree and have changed this according to your comment.

3.3 Setting and study design might be more developed. There are two data sources: the hospital staff survey and the patient mortality data.

Answer: We agree with this key comment and have provided detailed descriptions of the two datasets used in this study to produce clarity on who provided the data and how they were made available to us.

3.4 For the hospital staff survey, the data are from 3 years (2010-2012). It is unclear how these data are collected and which data are precisely available. They are described later, but all info should be together. I presume that the data are available on the provider level and that data are available about the hospital they are working. But are also the ward data available?

Answer: We agree and have put all of the data together for clarity. The hospital staff survey data are collected electronically in each participating hospital. All staff with more than three-month employment are invited to participate in the survey. The staff responses relate to their perception of the work environment on the ward/unit where they are employed.

Collected data is processed if more than five employees in the unit respond to ensure anonymity. The mean unit score for each item and factor is published in a report, which is made public. As part of their work agreement, managers are expected to discuss the report with their ward staff for continuous quality improvement.

3.5 The mortality data are less well described. Who is collecting these type of data? Is it the Norwegian Institute of Public Health? What data are precisely available? The risk adjustment is described later, but should be described here.

Answer: We agree and have moved the risk adjustment explanation accordingly.

The mortality data are collected from the Norwegian Patient Administrative System by the Norwegian Patient Register. Encrypted data are made available for the Norwegian Knowledge Centre for the Health Services. These data are used to estimate survival probabilities. Data are presented for three diagnosis; AMI, stroke and hip-fracture and a hospital-wide indicator. These data are made public as part of the Norwegian quality improvement initiative. The Norwegian Knowledge Centre was merged with the Norwegian Institute of Public Health which has taken over the responsibility for publishing the annual hospital mortality reports.

3.6 How the data were made available for the study? How are the two databases linked. The authors say that they are linking on ward level? How did they do it? What data were available? I don't see how they did it. So databases, available data and linkages should be described precisely in this section

Next section describes the selection of patient groups. Why these patients groups were selected? How were they identified? Later (p7) these codes were described, but we see here a mix of diagnostic codes and procedure codes. Some other criteria are used such as age (e.g. >age 65?, only emergency admissions?). All criteria for selection of patient groups should be listed here in detail.

Answer: Thank you for very helpful comments to make this clearer in the paper. We have changed the methods section to clarify that the two sets of data used in this study are public available. However, in order to do the analysis, we obtained consent from all participating hospital trust to combine their anonymous ward score on work environment with data on mortality. The data file was provided by the Norwegian Knowledge Centre for Health Services. We have listed all relevant criteria for selection of patient groups.

3.7 The selection of hospitals and wards should be better documented. I only read in the abstract that there are 20 Hospitals. Selection should be described. There are 56 wards. I don't know exactly what to understand about wards. Hip fracture patients are admitted on orthopaedic wards. But not all patients on orthopaedic wards are hip fracture patients. Did the staff (doctors, nurses) worked on

these dedicated wards. How do we know from the available data? We have 20 hospitals and 3 patients groups: $20 \times 3 = 60$ but only 56 wards. What does it mean? Does it mean that some hospitals don't have one of the patient groups e.g. no stroke unit, no STEMI unit? Please explain.

Answer: All public hospitals in the South Eastern Health Region that provided acute care are included, $n=20$, but only 17 hospitals provide care to all three of our included diagnosis. This results in 56 wards.

We chose the ward level as our level of observation and that is where the microsystem of care is centered. We identified the wards where patients with the included diagnosis were most likely was treated. The mortality rate for these patients is known and not combined with the mortality rate of other patients, as they were not part of our study.

We also had the work environment score from the staff working or providing services to these specific wards. The nurses work on the wards with the included patients with these three diagnoses. Physicians and managers provide services to several wards, but it was the perception of their work environment that was measured (the physician unit/manager unit).

The clinical data on p7 is a mix of methods and results and should just describe the different variables, how there are coded

Answer: This has now been changed. Please see answer 1.1.

3.8 Statistical analysis should be further elaborated. The problem is that mortality is known on the patient level. The staff data are on the organizational level (meaning staff data aggregated on the level of hospital*patient group). It is confirmed by the authors on p9. This requires a multilevel analysis approach on which organizational level data are related to patient level data. I don't see in the analysis plan this kind of approach. I don't know how they have addressed the relationships.

A second difficulty is that doctors, nurses, middle management evaluate the working environment of a ward (what I still assume as hospital*patient group). The same working environment might be perceived differently by the various professional groups. What does this say over the working environment? And how to address this in the equations? Just to give an example: the questionnaire doesn't address interprofessional collaboration, but in reality they work in teams and individual perceptions should be viewed in this respect.

Answer: Thank you for bringing these issues up. We have elaborated further on statistical analysis and we agree that some of the methodology was difficult to follow in our first draft.

We have made several adjustments of the paper as also stated to some of the other reviewers and think that it has improved the readability and clarity of the methods used. The main additions is to make clearer that both the environment and mortality data is available at ward level and not as individual patient or employee data. We believe that will remove most of your concerns regarding the challenges regarding multilevel analysis. We have also eluded on how the different professions' work environments are defined. We acknowledge that there could be many possible approaches to this kind of analysis, but our data were available at a unit level and not individually, which provided some boundaries for what approach we could choose.

We did not study the interprofessional collaboration specifically, but are aware that profession work together. However, our data indicates that they perceive their work environment differently and we wanted to study how different factors for different professions might be associated with patient mortality. The Factor Social Climate addresses teamwork and collaboration.

3.9 On page 8 the 2 research questions are formulated. Both questions are very similar. It would be useful to have a first descriptive question in describing work environment in the 20 hospitals and 3 patient groups. The second question could then focus on the relationship between organizational characteristics and patient mortality. The results are not well described. In table 5, it is very unclear what means “nurse unadjusted”, “physician unadjusted”, “managers unadjusted”.

Answer: Thank you for your comment. This has been addressed by updating the research question and added a hypothesis. We have updated the header and title in table 5 to clarify that we look at the association between environmental factors and mortality by linear regression with mortality as outcome with the adjustments done denoted in the footnotes.

VERSION 2 – REVIEW

REVIEWER	Chris Sherlaw-Johnson Nuffield Trust, UK
REVIEW RETURNED	11-Sep-2019

GENERAL COMMENTS	The authors have adequately addressed my earlier comments
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REVIEWER	Peter Griffiths University of Southampton
REVIEW RETURNED	29-Aug-2019

GENERAL COMMENTS	<p>The manuscript is much improved and it is now much easier to understand what has been done.</p> <p>I do have a few residual points for the authors/editors to consider.</p> <p>i) You have declined to make my revision in relation to your characterization: "organizational culture may be a fundamental determinant of patient outcomes". I have considered your response. There are a number of ways of looking at this. I do appreciate that you don't have to agree with me - but I still don't agree with you and I think what you say is, at best, misleading.</p> <p>One perspective is that the precise meaning of the phrase is very much open to interpretation and our interpretations differ. To me, the phrase, in particular, the term 'fundamental' implies that the observed outcomes are in large part determined by the phenomenon you refer to. This is simply not the case. Organizational culture makes a small (but important) contribution to the variation in patient outcomes between organizations. Your data actually demonstrate this. You clearly interpret it differently. While we might agree to differ on this interpretation, it raises the consideration that the lack of specific meaning renders the phrase essentially meaningless yet it is apt to mislead. This is significant because you offer it as a clarification.</p> <p>The other way of looking at it is that one or the other of us is correct. If so, I think I am right and you are wrong to characterize organizational culture in this manner, at least in so far as it relates to the determination of outcomes.</p>
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	<p>ii) The additional description of the backward regression procedure is useful. I am not a statistician and have no specific objection but I do wonder if the selection of variables for inclusion in the final model based on significance in the univariable models is the best approach. However, it is now clearly described and I have no problem for it to stand - but ideally, a statistician should take a look.</p> <p>iii) I am not fully convinced by your comments about the work environment and patient safety culture - perhaps the bigger issue that this raises is that you don't clearly define either to support the distinction of these two as entirely independent. I find a slight irony that your revised (and much clearer) aims and objectives make no mention of patient safety culture despite your assertion that it is distinct!</p> <p>"Our hypothesis was that there is an association between the work environment and patient mortality, and that this association is profession-specific for nurses, physicians and middle managers. The objective of this study was to examine the associations between profession-specific work"</p> <p>Either way I think you could do with further clarifying the background and aims and objectives to ensure that your definitions of the work environment and safety culture are made clearer and, if you do see safety culture as an independent factor as opposed to being part of the psychosocial work environment you need to specify it in your aims and objectives and define that as well.</p> <p>Revisions in relation to this point are probably not strictly necessary but I think it will make your paper stronger because it will be more convincing.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 2

Reviewer Name: Peter Griffiths

Institution and Country: University of Southampton

Please state any competing interests or state 'None declared': None

The manuscript is much improved and it is now much easier to understand what has been done.

I do have a few residual points for the authors/editors to consider.

i) You have declined to make my revision in relation to your characterization: "organizational culture may be a fundamental determinant of patient

outcomes". I have considered your response. There are a number of ways of looking at this. I do appreciate that you don't have to agree with me - but I still don't agree with you and I think what you say is, at best, misleading.

One perspective is that the precise meaning of the phrase is very much open to interpretation and our interpretations differ. To me, the phrase, in particular, the term 'fundamental' implies that the observed

outcomes are in large part determined by the phenomenon you refer to. This is simply not the case. Organizational culture makes a small (but important) contribution to the variation in patient outcomes between organizations. Your data actually demonstrate this. You clearly interpret it differently. While we might agree to differ on this interpretation, it raises the consideration that the lack of specific meaning renders the phrase essentially meaningless yet it is apt to mislead. This is significant because you offer it as a clarification.

The other way of looking at it is that one or the other of us is correct. If so, I think I am right and you are wrong to characterize organizational culture in this manner, at least in so far as it relates to the determination of outcomes.

Answer: We have given your feedback a lot of thought to identify where we might disagree. You are right in challenging us about the magnitude of cultural influence of patient outcomes. After more consideration and discussion, we agree that the use of “fundamental” in this setting could easily be perceived as overemphasizing the contribution. Our statement was triggered by the Francis and subsequent Berwick reports of the Mid Staffordshire NHS scandal. We interpreted these findings that culture was fundamental to the outcome of patient care. We accept that it was incorrect to claim it as a universal statement.

II) The additional description of the backward regression procedure is useful. I am not a statistician and have no specific objection but I do wonder if the selection of variables for inclusion in the final model based on significance in the univariable models is the best approach. However, it is now clearly described and I have no problem for it to stand - but ideally, a statistician should take a look.

Answer: Thank you for the feedback. This is a well-known and used statistical method, and one of the co-authors is a statistician. Despite that, we agree that the model has some limitations. One aspect is the importance of only including variables in the bivariate analysis when you have a theoretical framework or hypothesis supporting how it is associated with the outcome, and to evaluate the results with a theory and hypothesis in mind. It is an ongoing discussion (and has been for many years) if the selection of variables used in a “final” or “best” model should be:

- 1) Purely based on the theoretical framework, prior knowledge and the hypothesis.
- 2) Based on a pure statistical approach (pure number crunching).
- 3) Based on a mixed approach where you apply statistical methods to select models and validate the results by prior knowledge about the phenomena.

We have landed on a version of option 3) above where we have:

- Selected 19 variables we hypothesize could influence mortality
- Investigated which of them are bivariately associated with the outcome
- Used a statistical method based on bivariate significance for input in the model
- Done an evaluation of how reasonable the selection is compared to what we already know of the phenomena; and,
- Used a backward regression model and made an evaluation of how reasonable the results are.

In addition it can be noted that other approaches, (manually trying to build models based on what we believe would be the best models based on theory did not outperformed the models in the current paper.

iii) I am not fully convinced by your comments about the work environment and patient safety culture - perhaps the bigger issue that this raises is that you don't clearly define either to support the distinction of these two as entirely independent. I find a slight irony that your revised (and much clearer) aims and objectives make no mention of patient safety culture despite your assertion that it is distinct!

"Our hypothesis was that there is an association between the work environment and patient mortality, and that this association is profession-specific for nurses, physicians and middle managers. The objective of this study was to examine the associations between profession-specific work"

Either way I think you could do with further clarifying the background and aims and objectives to ensure that your definitions of the work environment and safety culture are made clearer and, if you do see safety culture as an independent factor as opposed to being part of the psychosocial work environment you need to specify it in your aims and objectives and define that as well.

Revisions in relation to this point are probably not strictly necessary but I think it will make your paper stronger because it will be more convincing.

Answer: Thank you for pointing out the inconsistency in our aims and the claims we make about patient safety culture as a determinant of patient outcomes. On page 6, we now offer a crisp definition of work environment and organizational culture, which we believe clarifies the aims of this paper.

The initial aim of the work environment survey in Norway was to measure staff perception of the physical and psychosocial conditions under which they work. The focus was solely on staff. However, with increasing focus on patient safety, measuring staff perception on patient safety culture seemed relevant.

The questions "I would feel safe if I was a patient here" and "Adverse events are appropriately handled here" were included to the survey. Our notion was that both the environment and culture are parts of the overarching work system as described by Carayon et al. in the SEIPS model. We did not take it upon us to discuss whether culture is a part of the psychosocial environment or not. This leads to a confusion that was not intended.

While Carayon et al separate organization, including teamwork, culture, social interaction and management from the physical environment; layout, noise, and lightning, in our survey we included both aspects and it can be argued that culture thus is an element of the work environment.

VERSION 3 – REVIEW

REVIEWER	Peter Griffiths University of Southampton, UK
REVIEW RETURNED	01-Nov-2019
GENERAL COMMENTS	Thank you for your constructive engagement with my final comments. This is an interesting paper