PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Epidemic of fractures during a period of snow and ice; has anything changed 33 years on?
AUTHORS	Al-Azzani, Waheeb Alawi Kassim; Mak, Danial Zulqarnain; Hodgson, Paul; Williams, Rhodri

VERSION 1 - REVIEW

REVIEWER	Lina Gyllencreutz Department of nursing, Umeå University, Sweden
REVIEW RETURNED	21-Dec-2015

GENERAL COMMENTS	It is an interesting topic and approach that the authors present. The authors addresses a problem that was relevant 30 years ago and is still relevant. Major problems from my point of view: The background does not give a complete picture of the topic today. I believe that the area is not fully explored. There are current literature lacking. I wish, for example, more information on the differences of fractures between men, women and age as it is part of the comparison result. To what extent have preventive measures been taken during the 30 years that have passed? To who have the preventive measures been directed; for pedestrians, car drivers, bicyclists? The purpose of the study is interesting i.e., to see what has changed 33 years on. Unfortunately, I think that the result does not fully meet the objective. The method is short and informative. I would like the primary and secondary outcome measures to be more clearly described in the method. It might improve the purpose also to become clearer. The injury incident site is not described. It makes me wonder where the injury incident occurred and what kind of activity that preceded the injury incident. Was it when the person was walking, driving a car, practicing sports (downhill skiing/snowboarding), etc. I would like to know how the authors proceeded to include only those incidents that were due to ice and snow? There are no information about the procedure or any injury database? If an injury database was used, what about the database validity? Are there losses that you may have missed? The result is clearly written. However, it is difficult to interpret the table and figures. In Table 1, the total number of fractures is missing. Moreover, the number of fractures on snow and ice days in the table 1 (n=120) does not match with the total number of fractures on snow and ice day in the text (n = 124). In the paragraph that begins with

	“There was a sharp increase”, you write “There was also an increase in number of fractures of the leg (4.3 times more common) and ankle and foot (1.7 times more common). This is not consistent with Table 1, when it is stated that the RR for leg is 4.9 and ankle and foot 1.9? Figure 3 is difficult to interpret. What is the percentage increase in risk of fractures? Discussion is short. The paragraph that begins with “Thirty three years ago”, can advantageously be placed in the background (except for the last sentence that begins with "this study"). It is written that "the striking finding of our study is that not much has changed in terms of fracture incidence in snow and ice conditions 33 years on." In figure 2, the percentage of increasing risk of fractures according to age is shown. The risk is lower today versus 33 years ago for the youngest and the oldest, which could mean that prevention efforts that have been taken might have paid off for these age groups. I lack a creative discussion of the efforts made in the area and how the results can be linked to them and current research.

REVIEWER	David Hamilton University of Edinburgh, UK
REVIEW RETURNED	31-Dec-2015

GENERAL COMMENTS	This is a short paper recreating a study from 30 years ago which evaluated the extent to which snow and ice influenced fracture rates. I think that as a point of interest this papers is essentially publishable, but the authors should consider the following points. 1. The study recreated the methodology previously used - and as such can probably be considered appropriate to answer their research question - however it must be clearly noted in the that this methodology is far from ideal. A single 4 day period of snow and ice contrasted to 2 control periods of similar length is hardly a comprehensive analysis of how climate influences fracture rates. From an epidemiological standpoint this is really pretty poor, and its hard to draw anything from such modest data. Were it not that this recreated a previous study it would be rejected. As such very clear methodological disclaimers should be added to the abstract and narrative sections. The discussion stops abruptly having reported the CIs and there is no consideration as to why the rates should be similar to 30 years ago. This is a missed opportunity to discuss the implications of weather on orthopaedic services, and in particular why there is no apparent change form the previous study despite the advent of fracture prevention strategies and bisphosphonates. The abstract needs to include the data from the current findings - p values and CI as opposed to a description of the old study The narrative - such as it is - is perhaps overly chatty for a research paper, this should be tightened up and greater focus placed upon the new data and risk ratios.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name
Lina Gyllencreutz

Institution and Country
Department of nursing, Umeå University, Sweden

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

Please leave your comments for the authors below
It is an interesting topic and approach that the authors present. The authors addresses a problem that was relevant 30 years ago and is still relevant.
Major problems from my point of view:
The background does not give a complete picture of the topic today. I believe that the area is not fully explored. There are current literature lacking. I wish, for example, more information on the differences of fractures between men, women and age as it is part of the comparison result. To what extent have preventive measures been taken during the 30 years that have passed? To who have the preventive measures been directed; for pedestrians, car drivers, bicyclists? (The aim of this paper was to replicate a seminal study 33 years ago. We therefore had to work within the confines of the original study design and data. Unfortunately the information was not available to us to comment on)

The purpose of the study is interesting i.e., to see what has changed 33 years on. Unfortunately, I think that the result does not fully meet the objective. (We have attempted to faithfully adhere to the original study design and present up to date data)

The method is short and informative. I would like the primary and secondary outcome measures to be more clearly described in the method. It might improve the purpose also to become clearer. (We have attempted to modify the methodology and clarify the primary and secondary outcome measure as advised.)

The injury incident site is not described. It makes me wonder where the injury incident occurred and what kind of activity that preceded the injury incident. Was it when the person was walking, driving a car, practicing sports (downhill skiing/snowboarding), etc. (Unfortunately, the mechanism of injury was unrecorded in the original study and therefore similar data 33 years later was presented to replicate the index paper. Whilst we are in agreement that the mechanism of injury is interesting, for simplicity and symmetry with the original paper, this data is not presented.)

I would like to know how the authors proceeded to include only those incidents that were due to ice and snow? (We compared the frequency of fracture in snow and ice conditions to control conditions. We concluded there was a statistically significant rise in fracture frequency in snow and ice conditions compared to control days. However, we did not look into any causality relationship and therefore we do not claim that all fractures were due to snow and ice)

There are no information about the procedure or any injury database? If an injury database was used, what about the database validity? Are there losses that you may have missed? (Thank you. We agree and amendments have been made to the methodology.)

The result is clearly written. However, it is difficult to interpret the table and figures. In Table 1, the total number of fractures is missing. Moreover, the number of fractures on snow and ice days in the table 1 (n=120) does not match with the total number of fractures on snow and ice day in the text (n = 124). (Thank you. We have made the amendment to explain that there were 4 facial fractures in our

study. We originally omitted this because Ralis did not include facial fractures in his original study and therefore we did not include those in the comparative table)

In the paragraph that begins with “There was a sharp increase”, you write “There was also an increase in number of fractures of the leg (4.3 times more common) and ankle and foot (1.7 times more common). This is not consistent with Table 1, when it is stated that the RR for leg is 4.9 and ankle and foot 1.9? (Thanks for highlighting this. This has been rectified)

Figure 3 is difficult to interpret. What is the percentage increase in risk of fractures? (Thank you for this. We agree and have clarified and amended figure 3)

Discussion is short. The paragraph that begins with “Thirty three years ago”, can advantageously be placed in the background (except for the last sentence that begins with "this study"). (Thank you for this. We agree and have amended accordingly)

It is written that "the striking finding of our study is that not much has changed in terms of fracture incidence in snow and ice conditions 33 years on." In figure 2, the percentage of increasing risk of fractures according to age is shown. The risk is lower today versus 33 years ago for the youngest and the oldest, which could mean that prevention efforts that have been taken might have paid off for these age groups. (Thank you for this. We agree and have clarified and amended accordingly)

I lack a creative discussion of the efforts made in the area and how the results can be linked to them and current research. (Thank you for this. We agree and have elaborated more on the discussion)

Reviewer: 2

Reviewer Name
David Hamilton

Institution and Country
University of Edinburgh, UK

Please state any competing interests or state ‘None declared’:
None declared

Please leave your comments for the authors below
This is a short paper recreating a study from 30 years ago which evaluated the extent to which snow and ice influenced fracture rates.

I think that as a point of interest this papers is essentially publishable, but the authors should consider the following points.

1. The study recreated the methodology previously used - and as such can probably be considered appropriate to answer their research question - however it must be clearly noted in the that this methodology is far from ideal. A single 4 day period of snow and ice contrasted to 2 control periods of similar length is hardly a comprehensive analysis of how climate influences fracture rates. From an epidemiological standpoint this is really pretty poor, and its hard to draw anything from such modest data. Were it not that this recreated a previous study it would be rejected. As such very clear methodological disclaimers should be added to the abstract and narrative sections. (Thank you very much for raising this very good point. We have amended the abstract and discussion accordingly)

The discussion stops abruptly having reported the CIs and there is no consideration as to why the rates should be similar to 30 years ago. This is a missed opportunity to discuss the implications of weather on orthopaedic services, and in particular why there is no apparent change form the previous

study despite the advent of fracture prevention strategies and bisphosphonates. (Interesting point. Thank you, we have elaborated more on the discussion)

The abstract needs to include the data from the current findings - p values and CI as opposed to a description of the old study. (Again extremely valid and we have amended accordingly. Thanks)

The narrative - such as it is - is perhaps overly chatty for a research paper, this should be tightened up and greater focus placed upon the new data and risk ratios. (Thank you for this. We agree and have amended accordingly)

VERSION 2 – REVIEW

REVIEWER	David Hamilton University of Edinburgh
REVIEW RETURNED	09-Mar-2016

GENERAL COMMENTS	I find this paper quite hard to comment on. The manuscript is well written on a relevant topic, repeating an historical study. The authors have revised in line with the reviews, however concerns remain as to the methodology. The stated 'need to work within the confines of the previous study' (that they are replicating) holds to a point, but I do retain concerns that were this an original research paper it would be rejected for claims made from such insufficient data. I believe this to be borderline acceptable in the context of the previous paper, but I would still like to see more discussion as to what the appropriate data collection and analysis would be from an epidemiological perspective - and explain why the data is insufficient. however i leave this decision to the editor.

VERSION 2 – AUTHOR RESPONSE

Were we not approaching this study bound by faithful replication of the original study methodology, with a need to compare and contrast historic data with current data, a more robust epidemiological method would have been applied. This would have included a longitudinal prospective population based cohort study of the people of Cardiff, where a large population is defined and followed up prospectively for a number of years. Relative Risks, along with Confidence intervals, could then have been calculated to estimate the contribution of snow and ice conditions to the frequency of fracture due to slipping and those due to other mechanisms. Subsequent stratification by age, sex, anatomical site of injury and mechanism could also have been performed.