BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (http://bmjopen.bmj.com/site/about/resources/checklist.pdf) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

**ARTICLE DETAILS**

<table>
<thead>
<tr>
<th>TITLE (PROVISIONAL)</th>
<th>The risk of sudden cardiac death in relation to season-specific cold spells: a case-crossover study in Finland</th>
</tr>
</thead>
<tbody>
<tr>
<td>AUTHORS</td>
<td>Ryti, Niilo; Mäkikyrö, Elina; Antikainen, Harri; Hookana, Eeva; Junttila, Juhani; Ikäheimo, Tiina; Kortelainen, Marja-Leena; Huikuri, Heikki; Jaakkola, Jouni.</td>
</tr>
</tbody>
</table>

**VERSION 1 – REVIEW**

<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Joseph Galvin</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>Mater University Hospital, Dublin, Ireland</td>
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<tr>
<td>REVIEW RETURNED</td>
<td>07-May-2017</td>
</tr>
<tr>
<td>GENERAL COMMENTS</td>
<td>This is an interesting, well-designed study looking at the effect of well characterised temperature drops on sudden cardiac death rates in the Finnish province of Oulu. The most interesting findings were the effect of cold spell duration with the increase in SCD not occurring unless the cold spell last 3 days, the dominant autumn (and winter) effect and the increased risk with ischaemic heart disease (55% increased risk). I think the paper is well written and have little to add except; 1. Limitations should emphasise a little more the impossibility of accurately determining temperature for a given individual because of house to house variability of wind factor / access to sunlight and individual to individual variability in the amount of clothing being worn and the possibility that cold weather may drive one person outside (to shovel the snow) and another inside because it's too cold. In addition the following minor corrections should be made; Page 11 line 3 should read; ‘Cold spells &gt; 3 days were associated with a significantly increased risk of SCD’ as all other durations of cold spell did not have a significant effect.</td>
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<tr>
<td></td>
<td>Page 14 line 2 Would re-phrase ‘generalizability’ as ‘is beneficial in terms of being able to generalise results’</td>
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<td></td>
<td>Page 14 Line 35 Would re-phrase as ‘should identify potential targets’</td>
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<td></td>
<td>Page 9 line 25 ‘Entified for the analyses’ should read ‘identified for the analyses’ Table 2 Should this Table read Age 35-64 rather than 0-64 as subjects less than 35 were reportedly excluded?</td>
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<tr>
<td>REVIEWER</td>
<td>Shih-Ann Chen</td>
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<tr>
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<td>Taipei Veterans General Hospital, Taipei, Taiwan</td>
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<tr>
<td>REVIEW RETURNED</td>
<td>17-May-2017</td>
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</tbody>
</table>

**GENERAL COMMENTS**

The authors demonstrated the association of cold spells with sudden cardiac death via 3614 autopsy-verified cases in Finland from 1998 to 2011. There are several concerns to be addressed.

1. Meteorological correlation with sudden cardiac death has been studied and presented before. Is there any additional information in this study other than the well-known phenomenon that cold weather is associated with higher risk of SCD?
2. In countries at high latitude like Finland, heaters and protective measures are quite common, which would possibly mitigate the effect of temperature change on risk of SCD. There is possibility that there are factors undetected by authors that influenced the risk.
3. According to Table 2, spring has lower mean temperature, larger ranges of temperature changes, and more cold spells than autumn. Authors also claimed that cold spells were associated with increased risk of SCD. So, why did seasonal analysis show that autumn had higher association of cold spells and SCD than spring? The seasonal differences seemed inconsistent with distribution of cold spells in terms of autumn and spring.
4. The percentage in parentheses is quite confusing in Table 1. For example, readers might interpret the second column as 80.25% of men died from ischemic etiology and 77.66% of men were non-ischemic. It should be polished for better understanding.
5. I would like to recommend a figure to illustrate temperature distribution in Table 2, so that readers can easily understand the temperature changes in different seasons.
6. Table 3 is also confusing. What constitutes the control group in this analysis? Authors should address this clearly.
7. Why did authors intend to present Figure 1 in this way that groups in Y-axis seem repetitive? I would recommend authors to adjust the component in Y-axis or add more discussion about Figure 1.

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<table>
<thead>
<tr>
<th>REVIEWER</th>
<th>Simon Stewart</th>
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<tr>
<td></td>
<td>Mary MacKillop Institute for Health Research</td>
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<td></td>
<td>Australian Catholic University</td>
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<td></td>
<td>Melbourne, Australia</td>
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<tr>
<td>REVIEW RETURNED</td>
<td>20-Jun-2017</td>
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</tbody>
</table>

**GENERAL COMMENTS**

It is very unusual (at least for me as a regular reviewer of manuscripts) to generate little or no comments for the authors to consider, but in this case, the authors have done a very good job in describing the purpose of their study (building on their previous research/findings) and used appropriate and robust methodology to investigate the relationship between SCD and cold spells in Finland.

I only have two small suggestions (one self-serving and not critical):

1. Perhaps comment on how the lifestyle (e.g. alcohol consumption and winter activities) and cold adaptations (behavioural and environmental) in Finland might influence the interpretation of these data in other countries.
2. We recently published a review of the overall phenomenon of Seasonality in CVD in Nature Reviews Cardiology - you may wish to reference this paper (your findings and data interpretation, including observed limitations around pollution and perhaps around infectious exposure, being consistent with the wider literature).

**VERSION 1 – AUTHOR RESPONSE**

Reviewer: 1
Joseph Galvin
Mater University Hospital, Dublin, Ireland

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

Please leave your comments for the authors below

**Comment:**
This is an interesting, well-designed study looking at the effect of well characterised temperature drops on sudden cardiac death rates in the Finnish province of Oulu. The most interesting findings were the effect of cold spell duration with the increase in SCD not occurring unless the cold spell last 3 days, the dominant autumn (and winter) effect and the increased risk with ischaemic heart disease (55% increased risk).
I think the paper is well written and have little to add except; 1. Limitations should emphasise a little more the impossibility of accurately determining temperature for a given individual because of house to house variability of wind factor / access to sunlight and individual to individual variability in the amount of clothing being worn and the possibility that cold weather may drive one person outside (to shovel the snow) and another inside because it's too cold

**RESPONSE:**
We have considered this carefully and modified the manuscript as requested (page 15-16): “Although we estimated the outdoor temperatures for each home coordinate for the week preceding the death of the resident, the behavioral activity patterns for the week preceding death were not available. Therefore, it was not possible to estimate how much time the cases spent outdoors, and what kind of thermal patterns they were exposed to. This is a common limitation in studies without personal measuring devices and activity pattern diaries. It also remains an important question how changes in weather manifest as changes in different indoor environments, and whether some of these changes mediate the harmful effects of weather. Variations between individuals in their attitudes toward cold weather, use of protective clothing, physical activity, comorbidity, substance abuse, and use of medications are likely to exist in our data and in the general population. Combinations of these and other individual factors might influence personal risk profiles in ways that are difficult to predict from an epidemiological stance, and although our results can be generalized over a population, they can't be directly translated to individual risk. It is reasonable to assume that the association between cold spells and risk of SCD is modified by a variety of factors still unknown”
In addition the following minor corrections should be made;

Comment:
Page 11 line 3 should read;
‘Cold spells > 3 days were associated with a significantly increased risk of SCD’ as all other durations of cold spell did not have a significant effect

RESPONSE:
We have considered this carefully. Cold spell has been defined on page 7: “Cold spell was defined as a period of ≥3 consecutive days with daily minimum temperature below the 5th percentile of the individual frequency distribution.” It is an informed decision to call periods with ≥3 consecutive cold days as cold spells, and to call other amounts of consecutive cold days as combinations of single cold days. This can be traced back to existing literature on cold spells,[6] where part of the original cold spell-hypothesis is that the health effect of a prolonged duration (f.ex. 3 days) of cold weather can be greater than the health effect of equally cold weather occurring on equal number (f.ex. 3 days) of days occurring in a non-consecutive formation. We believe elaborating this methodological-historical detail is not meaningful for the current manuscript, but we would like to apply the terminology as it is to appreciate this context. In order to avoid confusion, we have modified the Methods-section (page 7): “We used the same frequency distributions and thresholds to identify single cold days and any combination of single cold days in different definitions of unusually cold weather used in this study (e.g. 2 consecutive days”). Following this, we have modified the legend of Figure 1 (page 23): “y-axis presents the number of consecutive days with daily minimum temperature below threshold occurring during the week preceding death. Each effect estimate is derived by a stratified analysis according to the number of days, an integer being the exact number of consecutive days, and “≥” indicating the minimum number of consecutive days (e.g. at least n consecutive days).” The use of the term “cold spell” is now systematic throughout the manuscript. We hope this satisfies the reviewer.

Comment:
Page 14 line 2
Would re-phrase ‘generalizability’ as ‘is beneficial in terms of being able to generalise results’

RESPONSE:
We have considered this carefully and modified the manuscript as requested (pages 14-15): “Generally speaking, frequency distribution-based exposure assessment is beneficial in terms of being able to generalise results, making it easier to compare estimates obtained from different geographical regions.”

Comment:
Page 14 Line 35
Would re-phrase as ‘should identify potential targets’

RESPONSE: We have considered this carefully and modified the manuscript as requested (page 16): “We believe more studies on the associations between cold weather and SCD should identify potential targets for intervention, be it pharmacological modification, diagnosis and treatment of underlying cardiovascular diseases, or behavioral patterns during cold weather.”
Comment:
Page 9 line 25
'Entified for the analyses' should read 'identified for the analyses'

RESPONSE: We have modified the manuscript as requested (page 9): “A total of 3974 location specific cold spells were identified for the analyses.”

Comment:
Table 2
Should this Table read Age 35-64 rather than 0-64 as subjects less than 35 were reportedly excluded?

RESPONSE: The reviewer is referring to Table 1. Age 0-64 (or 0 to 65+) is correct for the main analyses and for the stratified analyses according to season and the number of cold days. Age 35-64 (or 35 to 65+) is correct for the subgroup analyses according to age, sex, and ischaemic versus non-ischaemic aetiology of the sudden cardiac death.

We have considered this carefully and made several adjustments to the manuscript to improve clarity: First, we have included both age groups 0-34 and 35-64 to Table 1.

Then, we have clarified the difference between stratified analyses and subgroup analyses in the Methods-section (page 7-8): “We conducted a priori stratified analyses according to an increasing number of cold days preceding death and according to the season of death. Calendar time was used to define the 4 seasons (autumn Sept-Nov, winter Dec-Feb, spring Mar-May, summer, Jun-Aug). We conducted a priori subgroup analyses using gender, age, and ischaemic versus non-ischaemic aetiology of the death confirmed by the autopsy finding as BY-variables in conditional logistic regression. Cases aged under 35 were excluded from the subgroup analyses a priori because of the few numbers (n=40) and expected heterogeneity.” (Note: A typo of the excluded number of cases has been corrected).

Then, we have made small modifications throughout the manuscript to systematically and explicitly use the terms “stratified analysis” and “subgroup analysis”. In the Results-section (page 11): “In the analyses stratified by season, distinct seasonal differences in the association between cold spells and SCD were observed”; In the Results-section (page 11): “We conducted several subgroup analyses for the purpose of elaborating effect modification of the relation between cold spells and SCD (Table 4)”; Title of Table 3 (page 11): “Table 3. The relation between the occurrence of cold spells and the risk of SCD based on stratified analyses by season”; Legend of Figure 1 (page 23): “...Each effect estimate is derived by a stratified analysis according to the number of days, an integer being the exact number of consecutive days, and “≥” indicating the minimum number of consecutive days (e.g. at least n consecutive days).”

Finally, to make sure that these different types of results stand out, we have relocated all numerical effect estimates of the subgroup analyses from the main text in the Results-section to a new Table 4 in the Results-section (page 11): “Table 4. The relation between the occurrence of cold spells and the risk of SCD by various subgroups of cases over 34 years of age”. Please note that the new table does not include any new information: all estimates were previously given in the main text of the Methods-section. Please note that no estimates are missing, they have just been relocated to a table in the same Methods-section. Please note that while we have removed the numerical effect estimates from the sentences, the sentences themselves remain as they were, with 2 minor exceptions: 1) We have changed “those aged under 65” to “those aged 34-64” for clarity, and; 2) We have added “...confirmed by Q-statistic 3.85, p 0.05 (Table 4)”, which links the main text and the table and helps in the interpretation of the results.

We thank the reviewer for expert advice. We believe that the manuscript has been improved during revision.
Reviewer: 2
Shih-Ann Chen
Taipei Veterans General Hospital, Taipei, Taiwan
Please state any competing interests or state 'None declared': none declared

Please leave your comments for the authors below
The authors demonstrated the association of cold spells with sudden cardiac death via 3614 autopsy-verified cases in Finland from 1998 to 2011. There are several concerns to be addressed.

Comment:
1. Meteorological correlation with sudden cardiac death has been studied and presented before. Is there any additional information in this study other than the well-known phenomenon that cold weather is associated with higher risk of SCD?

RESPONSE:
We have considered this carefully and modified the manuscript to address this comment and the editorial request #1. We have changed the subsection “Limitations” to “Strengths and limitations”, with added text (page 15): “There is evidence on the benefits of confirming the diagnosis and mode of sudden death by medico-legal autopsy in order to provide unbiased information on the incidence of SCD.[2, 3, 18] It is difficult to distinguish between cardiac and non-cardiac cause of sudden death without an autopsy, since many conditions that evolve rapidly, such as aortic dissection, massive pulmonary embolism, or stroke, can lead to sudden collapse and death. We conducted our analyses using the world’s largest autopsy-verified dataset of SCD. We took advantage of the individual-level nature of the data by forming and assessing individual frequency distributions of temperatures at each home coordinate to define cold weather for the cases.”
We have also relocated the section discussing our innovative exposure assessment under “Strengths and limitations” (page 14-15): “Improvements were introduced to the definition of cold spell based on the findings of our recent systematic review and meta-analysis...”
In addition to these and other methodological novelties, the novel findings include: 1) estimation of the relation between cold spells and the risk of SCD and showing a dose-response function, i.e. the effects of the duration of the cold period on the risk; 2) identification of ischaemic SCD as the primary end point of the effect of cold spells; and 3) demonstration of a seasonal variation in the strength of association between the occurrence of seasonally defined cold spells and the risk of SCD.

Comment:
2. In countries at high latitude like Finland, heaters and protective measures are quite common, which would possibly mitigate the effect of temperature change on risk of SCD. There is possibility that there are factors undetected by authors that influenced the risk.

RESPONSE: We have considered this carefully and modified the Strengths and limitations-section of the manuscript accordingly (page 15): “Although we estimated the outdoor temperatures for each home coordinate for the week preceding the death of the resident, the behavioral activity patterns for the week preceding death were not available. Therefore, it was not possible to estimate how much time the cases spent outdoors, and what kind of thermal patterns they were exposed to. This is a common limitation in studies without personal measuring devices and activity pattern diaries. It also remains an important question how changes in weather manifest as changes in different indoor environments, and whether some of these changes mediate the harmful effects of weather. Variations between individuals in their attitudes toward cold weather, use of protective clothing, physical activity, comorbidity, substance abuse, and use of medications are likely to exist in our data and in the general population. Combinations of these and other individual factors might influence personal risk profiles in ways that are difficult to predict from an epidemiological stance, and although our results can be generalized over a population, they can’t be directly translated to individual risk.
It is reasonable to assume that the association between cold spells and risk of SCD is modified by a variety of factors still unknown. 

We agree with Reviewer that the context where cold spells increase the risk of SCD and other health outcomes is not well known. We have explored this extensively in our previous systematic review on the health effects of cold spells.[6] 

As per the specific notion on heaters, this topic is subject to much speculation. Improperly adjusted heaters can produce supraoptimal indoor temperatures during cold weather. This can lead to paradoxical heat stress. The use of heaters can disturb acclimation processes and result in both negative and positive effects on health. The role of different types of heating systems can be different during different seasons. A household can have several complementary sources of heating (e.g. electrical heating and a fireplace), and these heating sources can be subject to different adjustments and different reasons for adjustments. Heaters are not the only components influencing the thermal environment in human residences (thermal capacitance of the surface and inner construction materials, mode and rate of ventilation, et cetera, being just a few others). We prefer not to emphasize the role heating systems in this paper because of the speculative nature of the topic; because heaters are not sole determinants of indoor temperature; because the role of heaters was not assessed. 

We hope that the extensive expansion of discussion (page 15) will satisfy the Reviewer. 

Comment:  
3. According to Table 2, spring has lower mean temperature, larger ranges of temperature changes, and more cold spells than autumn. Authors also claimed that cold spells were associated with increased risk of SCD. So, why did seasonal analysis show that autumn had higher association of cold spells and SCD than spring? The seasonal differences seemed inconsistent with distribution of cold spells in terms of autumn and spring. 

RESPONSE: 

We have considered this carefully and agree that Table 2 was not clear. However, there are no inconsistencies between Table 2 and the effect estimates. To address this comment in a comprehensive manner, we would first like to elaborate the interpretation of the old Table 2, and then describe the improvements in the manuscript to ensure clarity, i.e. a revised Table 2. 

Interpretation (old Table 2): As shown in the old Table 2, the proportion of individual cold days is roughly similar across the seasons. F.ex. there are 14436 and 14415 individual days with Tmin below threshold during winter and summer, respectively. We defined cold spells conceptually as periods of ≥3 consecutive days that are unusually cold (Tmin below threshold) for the respective place and time of the year. The variation in the number of cold spells reflects seasonal differences in the duration of these unusually cold periods, e.g. more cold spells of ≥3 consecutive days during winter than during summer, while a similar number of single cold days can occur in a non-consecutive manner during summer. The association between the occurrence of cold spells and the risk of SCD reflects the empirical observation how commonly a cold spell precedes a SCD (hazard periods) compared to the occurrence of a cold spell during the 50 reference periods in the same location and calendar time. Thus the seasonal differences in the distribution of cold spells do not per se influence their effect on the risk of SCD. 

Improvements (revised Table 2): All temperature indices in the old Table 2 were derived from the case-based matrices, i.e. they comprised of the days of the hazard and reference periods only. After careful consideration, we conclude that this is not an optimal way to describe the general weather in the study area during the study period, and we suggest replacing Table 2 with a revised Table 2 which is not dependent on the case-based matrices. The revised Table 2 is elaborated in the Methods-section (page 7): “To describe the general weather during the study, we formed a time series of minimum, mean, and maximum daily temperatures at the central coordinate 27.39E/64.79N of the province of Oulu, Finland, over the study period 1961-2011, using the same databases, programs, and methods as described above.”
The revised title clearly indicates this: “Table 2. The distribution parameters of daily temperatures (T) in the province of Oulu, Finland, over the study period 1961-2011.”

The new values do not affect interpretation of the results. Both presentations are accurate, but we believe the new Table 2 is more suitable for communicating what kind of weather the study area had during the study period. It should be noted that the contents of the old Table 2 do not influence the analyses for reasons described in the beginning of this response, and since each case of SCD has their own frequency distribution. Therefore, the relevance of old Table 2 is limited. We believe these values should be left out of the manuscript to avoid confusion, and we have removed the old Table 2 from the manuscript.

We have also modified the manuscript (page 13): “The contrast in effect estimates between autumn and spring, both of which have similar temperature indices (Table 2), and similar incidence of SCD (Table 1), is noteworthy”. This is now in line with the revised Table 2.

We hope these answers and the subsequent modifications to the manuscript satisfy the reviewer.

Comment:
4. The percentage in parentheses is quite confusing in Table 1. For example, readers might interpret the second column as 80.25% of men died from ischemic etiology and 77.66% of men were non-ischemic. It should be polished for better understanding.

RESPONSE:
We have considered this carefully and agree with the Reviewer that the percentages were not clear. We have revised the Table 1 as requested (page 9).

Comment:
5. I would like to recommend a figure to illustrate temperature distribution in Table 2, so that readers can easily understand the temperature changes in different seasons.

RESPONSE:
We have considered this carefully and believe these characteristics can be easily understood from the parametric presentation of the revised Table 2. The suggested presentation might be more meaningful if we had one shared frequency distribution and one cold spell definition based on that frequency distribution, like in most previous studies on cold spells and health effects. Such studies are based on shared exposures using aggregate weather data. However, we conducted an individual-level study and have 3614 individual frequency distributions. We believe that focusing on the general weather parameters during the study period is more informative (Table 2).

Comment:
6. Table 3 is also confusing. What constitutes the control group in this analysis? Authors should address this clearly.

RESPONSE:
We have considered this carefully and made several adjustments to the manuscript to improve clarity. Table 3 presents the season-specific effect estimates for the relation between the occurrence of cold spells and the risk of SCD, i.e. the data are stratified by season and therefore none of the seasons represents “a control group”. The four season-specific effect estimates are from different statistical models. The comparison in this case-crossover design is made between hazard and reference periods.
First, we have clarified the difference between stratified analyses and subgroup analyses in the Methods-section (page 7-8): “We conducted a priori stratified analyses according to an increasing number of cold days preceding death and according to the season of death. Calendar time was used to define the 4 seasons (autumn Sept-Nov, winter Dec-Feb, spring Mar-May, summer, Jun-Aug). We conducted a priori subgroup analyses using gender, age, and ischaemic versus non-ischaemic aetiology of the death confirmed by the autopsy finding as BY-variables in conditional logistic regression. Cases aged under 35 were excluded from the subgroup analyses a priori because of the few numbers (n=40) and expected heterogeneity.” (Note: A typo of the excluded number of cases has been corrected).

Then, we have made small modifications throughout the manuscript to systematically and explicitly use the terms “stratified analysis” and “subgroup analysis”. In the Results-section (page 11): “In the analyses stratified by season, distinct seasonal differences in the association between cold spells and SCD were observed”; In the Results-section (page 11): “We conducted several subgroup analyses for the purpose of elaborating effect modification of the relation between cold spells and SCD (Table 4)”;

Title of Table 3 (page 11): “Table 3. The relation between the occurrence of cold spells and the risk of SCD based on stratified analyses by season”; Legend of Figure 1 (page 23): “.. Each effect estimate is derived by a stratified analysis according to the number of days, an integer being the exact number of consecutive days, and “≥” indicating the minimum number of consecutive days (e.g. at least n consecutive days).”

Finally, we have relocated all numerical effect estimates of the subgroup analyses from the main text in the Results-section into a new Table 4 in the Results-section (page 11): “Table 4. The relation between the occurrence of cold spells and the risk of SCD by various subgroups of cases over 34 years of age”. Please note that the new table does not include any new information: all estimates were previously given in the main text. Please note that no estimates are missing, they have just been relocated. Please note that while we have removed the numerical effect estimates from the sentences, the sentences themselves remain as they were with 2 minor exceptions: 1) We have changed “those aged under 65” to “those aged 34-64” for clarity, and; 2) We have added “..confirmed by Q-statistic 3.85, p 0.05 (Table 4)”, which links the main text and the table and helps in the interpretation of the results.

Comment:
7. Why did authors intend to present Figure 1 in this way that groups in Y-axis seem repetitive? I would recommend authors to adjust the component in Y-axis or add more discussion about Figure 1.

RESPONSE:
We have considered this carefully and modified the manuscript accordingly, but chose not to change Figure 1. The effect estimates presented in Figure 1 are organized according to an increasing exposure to cold days and they convey the central message: the risk of SCD increases according to increasing duration of exposure. Although these effect estimates are based on partially overlapping exposure ranges, there is nothing incorrect in the presentation.

As a response to this comment, we have modified the legend of Figure 1 (page 23): “Figure 1. The relation between sudden cardiac death and consecutive days below threshold temperature. X-axis presents the odds ratios (OR) and 95% confidence intervals (95% CI) on a logarithmic scale; y-axis presents the number of consecutive days with daily minimum temperature below threshold occurring during the week preceding death. Each effect estimate is derived by a stratified analysis according to the number of days, an integer being the exact number of consecutive days, and “≥” indicating the minimum number of consecutive days (e.g. at least n consecutive days).”

We have also made several clarifications throughout the manuscript to elaborate the different analyses (stratified analyses and subgroup analyses), please see our response to the previous comment # 6.

We thank the reviewer for expert advice. We believe that the manuscript has been improved during revision.
GENERAL COMMENTS

This paper is well conceived and written. The definition and identification of cold spells and the use of autopsy in all cases to confirm underlying cause of sudden death is robust. The conclusion that cold spells are associated with sudden cardiac death due to underlying ischaemic but not non-ischaemic causes is novel and merits publication. The methodology focusing on abrupt cold spells at any time of the year rather than the coldest weather of the year is interesting and provides evidence that extends to autumn the time of year in which cold spells can trigger SCD events. The limitations of the study are well-defined in particular the lack of data on the effects of air pollution or the effects of an individual’s protective clothing. I am not aware of the lack of effect of cold spells on non-ischaemic causes of SCD being previously demonstrated. It would be interesting to know the causes of SCD in these cases. Do the authors have any data on the underlying cause of SCD in these 855 patients? Did they have cardiomyopathy, SADS, known ion channelopathy or myocarditis? This data should be provided as the effect of cold in the setting of CAD has been well described but there is really no data in non-ischaemic SCD.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Joseph Galvin
Institution and Country: Mater Hospital, UCD, Dublin, Ireland Please state any competing interests or state ‘None declared’: None declared
Please leave your comments for the authors below

Comment:
This paper is well conceived and written.

RESPONSE:
Thank you.

Comment:
The definition and identification of cold spells and the use of autopsy in all cases to confirm underlying cause of sudden death is robust.

RESPONSE:
We agree.
Comment:
The conclusion that cold spells are associated with sudden cardiac death due to underlying ischaemic but not non-ischaemic causes is novel and merits publication.

RESPONSE:
Thank you, we agree.

Comment:
The methodology focusing on abrupt cold spells at any time of the year rather than the coldest weather of the year is interesting and provides evidence that extends to autumn the time of year in which cold spells can trigger SCD events.

RESPONSE:
We agree, this is very interesting.

Comment:
The limitations of the study are well-defined in particular the lack of data on the effects of air pollution or the effects of an individual's protective clothing.

RESPONSE:
Thank you.

Comment:
I am not aware of the lack of effect of cold spells on non-ischaemic causes of SCD being previously demonstrated. It would be interesting to know the causes of SCD in these cases. Do the authors have any data on the underlying cause of SCD in these 855 patients? Did they have cardiomyopathy, SADS, known ion channelopathy or myocarditis? This data should be provided as the effect of cold in the setting of CAD has been well described but there is really no data in non-ischaemic SCD.

RESPONSE:
We have provided the suggested information by including a Supplementary Table S1 "Underlying cardiac disease of victims of non-ischaemic SCD based on autopsy finding." We believe that this information is best presented as Supplementary Material, since these subgroups are small, since this is not the focus of the paper, and since non-ischaemic SCD was not associated with cold spells.
We have included a description of the table to Results: "Table S1 elaborates the underlying cardiac disease of the victims of non-ischaemic SCD, with diagnostic criteria previously reported.[16]"
The cited paper includes a thorough description of the autopsy findings and methods leading to each diagnosis.
We have made no other changes to the manuscript. We believe to have addressed all concerns and comments of the reviewers and the edito