Response to reviewers

Regarding the manuscript "Future scenarios of thunderstorm environments in Fennoscandia based on a regional climate model" by M. Virman , T. K. Laurila , A. Mäkelä , P. Lind , A. Luomaranta , and H. Gregow

Author response

We thank the two reviewers for taking the time to review this manuscript and for their insightful comments and suggestions on how to improve it. We have gone through both reviews and have taken into account all comments.

We provide one response document for both reviewers because some of the comments addressed similar aspects of the manuscript. For clarity, we first present our responses to Reviewer 1, and then to Reviewer 2, with cross-references where suitable.

We provide a point-by-point response to each comment below. The original comment is first presented in **bold**, followed by our response.

Comments by Reviewer 1

Summary:

This study investigates the change in thunderstorm occurrence in Fennoscandia in a warming climate. The HARMONIE convection-permitting climate simulations (3 km grid spacing) are used with boundary conditions from either EC-EARTH or GFDL. Furthermore, RCP scenarios 4.5 and 8.5, as well as mid and late century changes are compared to the historical period. Simple thresholds for combinations of CAPE, precipitation, and deep wind shear are used to assess the changes in thunderstorms, convective precipitation, and severe thunderstorms, respectively.

It is found that both thunderstorm days and severe thunderstorm days significantly increase by mid-century, with only secondary differences across the ensemble. The northern half of Fennoscandia is most affected. The driver of this increase is more frequent occurrence of CAPE above the threshold despite the relative frequency of precipitation and shear on days with CAPE showing slight decreases.

General comments:

I agree that the topic is worth studying given the large expected impact of climate change on thunderstorm occurrence. Overall, the article is in good shape, with clear structure, scientific writing and illustrations. However, the novelty of the study is a bit unclear to me. The approach of using simple convective parameters to study thunderstorm trends has been used in many studies. It is perhaps worth looking into regional trends in a specific area like Fennoscandia in detail (I agree that Fennoscandia is left out in too many studies), but the analysis is fairly shallow for such a regional focus. The impact of the study could be increased with a more sophisticated analysis (comment 2 below), more comparison with the literature (comment 1), and a clearer justification for the novelty of the study (comment 3).

Specific major comments:

1. **There is insufficient comparison to other studies. Convection-permitting climate simulation over Europe are mentioned, but no direct comparisons are made, even though these studies include at least part of southern Scandinavia (Kahramann et al. 2025, Thurnherr et al. 2025). Recent global studies based on convective parameters in climate models are not even mentioned (Battaglioli et al. 2026, Raupach et al. 2026, Zhang et al. 2026). Do these studies agree or disagree with the results?**

   Thurnherr, I., Cui, R., Velasquez, P., Wernli, H., & Schär, C. (2025). The effect of 3°C global warming on hail over Europe. GRL. https://doi.org/https://doi.org/10.22541/au.173809555.59545480/v1

   Battaglioli, F., Taszarek, M., Groenemeijer, P., Púčik, T., & Rädler, A. (2026). Contrasting trends in very large hail events and related economic losses across the globe. Nature Geoscience, 19(January), 52–59. https://doi.org/10.1038/s41561-025-01868-0

   Raupach, T.H., Portmann, R., Siderius, C. et al. Shifting hail hazard under global warming and effects on crop hail risk. Clim. Chang. 16, 696–703 (2026). https://doi.org/10.1038/s41558-026-02660-7

   Zhang, S., Zhang, Q., Allen, J. T., & Lin, X. (2026). Rising global hail damage potential in a warming world. Nature, 653(8116), 1069–1077. https://doi.org/10.1038/s41586-026-10543-2

Author response

We thank and agree with the reviewer for pointing out that the manuscript would be stronger if it included more comparison with previous studies. We have thus broadened the discussion to include comparison with previous studies that have utilized convective parameters, lightning and hail proxies in regional and global models, as well as those that utilize process-based lightning and hail parameterizations or proxies in convection-permitting climate models. We have also made changes in the introduction, including references to the more recent studies. Please see lines 66-72 in the introduction and 435-479 in the discussion section of the revised manuscript.

The reviewer asks whether the results compare with those from previous studies, including hail studies. As far as we are aware, previous studies suggest more numerous conditions favourable for (severe) thunderstorms and increased lightning counts in Fennoscandia using global, regional and convection-permitting climate models (Pucik et al. 2017, Rädler et al. 2017, Finney et al. 2018, Chen et al. 2021, Kahraman et al. 2022). As for hail projections in Fennoscandia, it appears that previous studies show a bit mixed results. The global study of Battaglioli et al. 2026 shows only very small absolute changes or no changes at all in the occurrence of very large hail (≥5 cm), although from the color scale used in their figures it is a bit difficult to see whether the changes are zero or not. On the other hand, the global study of Raupach et al. 2026 shows increases in hail prone days. The statistical model of Rädler et al. 2019 projects increased large hail (≥2 cm) in Fennoscandia using EURO-CORDEX models. Thurnherr et al. 2025 shows, using a convection-permitting climate model, that intense hail (1.25-2 cm) might increase in southern Sweden whereas for small (1–1.25 cm) and severe (>2 cm) hail the results vary spatially in southern Sweden and Norway. In Kahraman et al. 2025, summertime severe hail (>2 cm) potential is projected to increase, decrease or undergo no changes depending on the season and region of southern Sweden and Norway. Zhang et al., 2026 also suggests increasing hail-related impacts, with particularly strong increases across many mid-latitude regions. However, the changes in hail damage potential over Fennoscandia appear less clear.

We find that a detailed quantitative comparison with previous studies is challenging in many ways. First, some signals over Fennoscandia are a bit difficult to discern in global or regional studies, sometimes simply due to the scaling used in maps

presenting wide areas with different thunderstorm climatologies. Second, the convection-permitting climate model studies generally cover only a small region of Fennoscandia. Third, our proxy does not include detailed descriptions of thermodynamical or microphysical profiles that are particularly important for the formation of lightning and hail, and our proxy has not been trained with hail observations.

Nevertheless, in general, our results agree with previous studies of environmental conditions and lightning proxies, namely, HCLIM projects that unstable conditions become more common, leading to more situations with favourable conditions for the occurrence of convective storms. Previous studies have suggested that over Fennoscandia, the increase in CAPE might dominate over the projected decrease in cloud ice content (Chen et al. 2021, Kahramann et al. 2022), leading to increased flash rates. However, our proxy does not include information about microphysical properties or the cloud electrification layer, so it is unclear whether HCLIM projects the same.

Our results show increases in the severe thunderstorm environment proxy, which captures situations with instability and large shear. Tuovinen et al. (2015) showed that, in Finland, mean deep layer shear in environments of significant hail cases was 17.5 m/s. We use a shear threshold of 12.5 m/s, so it likely captures large hail environments. However, we feel that this alone cannot be used to directly imply that hail might become more common in Fennoscandia, since our proxy does not include processes governing the production of hail (such as large updrafts in hail growth zones), and therefore comparison with previous studies is challenging.

2. **Related to comment 3 below, I don't understand why convection-permitting climate simulations are performed but the resolved convection is not analyzed. The novelty of recent studies like Kahramann et al. (2025) and Thurnherr et al. (2025) lies in their sophisticated analyses of resolved convection. Your study could make an important contribution here as these other studies omit parts of Fennoscandia. I suggest for example looking into trends of intense updraft occurrence in the simulations.**

Author response

We agree that a sophisticated analysis of resolved convection is one of the key advantages of convection-permitting climate simulations, and that such analyses have provided useful insights in recent studies (e.g. Kahraman et al. 2022, 2025 and Thurnherr et al. 2025). We also recognize that Fennoscandia remains relatively underrepresented in such assessments, and that further analysis of resolved convection in convection-permitting models in this region would be especially valuable.

However, the HCLIM simulations used in this study do not include online hail or lightning parameterization schemes. Moreover, the archived output is limited to 3-hourly snapshots of temperature and vertical velocity on selected pressure levels together with vertically integrated ice water path. We therefore do not have access to the variables used by several established hail or lightning diagnostics (e.g. Thurnherr et al. 2025, Kahraman et al. 2022, 2025, Cui et al. 2025).

We acknowledge that previous studies, including Kahraman et al. 2024, 2025, have derived hail diagnostics from 3-hourly convection-permitting model output. They argued that such output can still capture the most intense storms.

We briefly tested whether the available output could nevertheless provide useful information on changes in resolved convective updrafts. As a simple test, we examined the occurrence of days with at least one 3-hourly time step where vertical velocity at 500 hPa exceeded 1 m/s, using this as a very crude proxy for a convective updraft. Fig. 1 below indicates that the frequency of these events is *substantially* lower than the observed frequency of thunder days, despite the relatively weak updraft threshold applied. We concluded that the 3-hourly instantaneous output of vertical velocity misses a large fraction of convective updrafts and therefore underestimates the occurrence of convective storms. Consequently, we consider it quite difficult to derive reliable diagnostics of hail, lightning, or severe convection from the currently available data.

The test analysis does indicate an increase in the regional occurrence of days with at least one resolved updraft exceeding 1 m/s at 500 hPa (Fig. 2 below). However, given the limitations described above, we do not consider this metric sufficiently robust to form the basis of a comprehensive analysis of resolved convection.

We have added a clarification for why resolved-convection diagnostics are not included in the present study (please see line 96-100 and 521-525 in the revised manuscript).

We would also like to clarify that we view the current work as a first step toward developing more comprehensive climate projections of (severe) convective storms for Fennoscandia. A new set of HCLIM simulations is currently being produced with output designed to better support analyses of convective storms, hail, and lightning, which will enable more detailed investigations in future work. We are aware of the limitations in the proxies used in this study, and the limitations are pointed out in the manuscript.

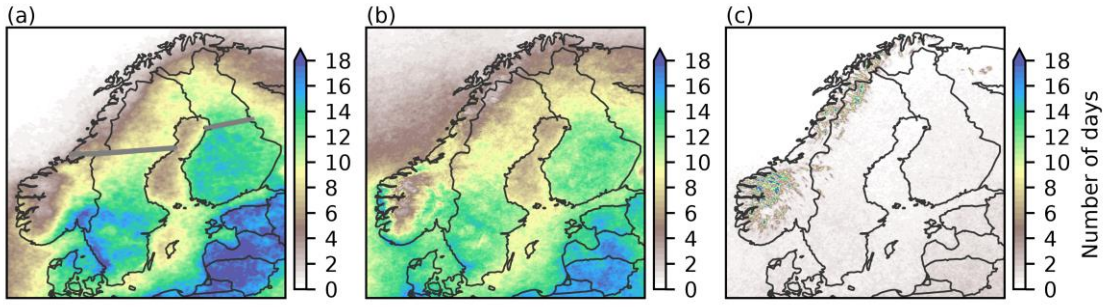


Fig. 1. Seasonal (May-September) mean number of (a) observed thunder days from NORDLIS, as well as the number of days with at least one 3-hourly (b) joint occurrence of CAPE > 150 J/kg and precipitation > 0.5 mm/3h and (c) 500 hPa vertical velocity exceeding 1m/s in HCLIM-ERAI in 2002-2018.

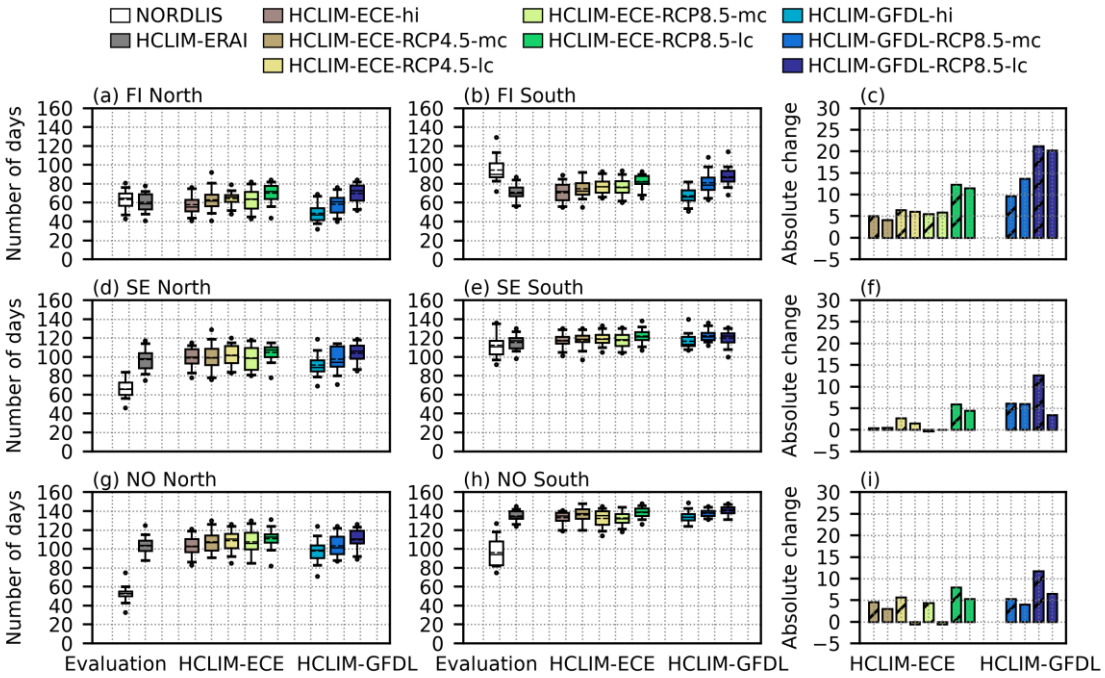


Fig. 2. Regional number of days with at least one 3-hourly occurrence of vertical velocity at the 500 hPa level exceeding 1 m/s (left, middle), and the change in its annual mean with respect to the historical period (right), from HCLIM-ECE and HCLIM-GFDL for two subregions of (a)-(c) Finland, (d)-(f) Sweden and (g)-(i) Norway in September-May. Periods "hi", "mc", and "lc" denote 1986-2005, 2041-2060, and 2081-2100, respectively. Corresponding values from HCLIM-ERAI and the observed regional thunder days from NORDLIS are shown for 2002-2018. Boxplots (left, middle) represent interannual variability, with middle solid (dashed) line representing the mean (median), boxes the 25th and 75th percentiles and whiskers the 1st and 99th percentiles. Southern and northern regions are marked with empty and hatched bars, respectively.

3. **Lines 70-77 and 426-428: In my opinion there is a major inconsistency in the justification of the study here. I don't see an obvious reason why the capability of HARMONIE to resolve convection necessarily means the ingredients are better simulated than in coarser simulations? In other words, the fact that convection is parameterized does not mean the environment (i.e., CAPE) is modelled less accurately in my opinion. If you decide against the additional analyses in comment 2, can you provide references or other justification for why the HARMONIE simulations are much better than coarser climate model**

simulations when analysing environmental parameters like CAPE? For example, are boundary-layer processes especially over mountains better resolved? Is the resolved precipitation much more accurate than if parameterized?

Author response

We feel that there may have been a misunderstanding about the justification and goal of this manuscript. As far as we are aware, we do not state that convection-permitting climate model such as HCLIM necessarily provides more accurate representation of CAPE or vertical wind shear, but we can see that the formulation of parts in the introduction and discussion may give the impression that we think that is the case. We apologize for not being clear, and are glad that we have the opportunity to clarify.

Our intention is to state that the high-resolution convection-permitting climate model designed for Fennoscandia can provide *a more detailed* representation of the regional climate system. HCLIM climate projections allow a more detailed representation of regional topography, coastlines, land-surface heterogeneity, boundary-layer processes and precipitation patterns, especially of summertime convective precipitation (Medus et al. 2022). Furthermore, convection-permitting climate models generally also lead to more detailed convective storm initiation and evolution than coarser models (Lucas-Picher et al. 2021), e.g. than the EURO-CORDEX models run at 50 km resolution and used by Rädler et al. (2019) and Pucik et al. (2017).

We have revised the introduction to be clearer (please see lines 83-94 in the revised manuscript). We also added a statement that future studies could investigate the differences in convective parameters between different datasets (see lines 478-479). We think it is best not to include analysis of resolved updrafts in this study, please see response to comment 2 and lines 96-100 and 521-525 in the revised manuscript for the reasoning of this.

4. **Line 116 and following: HCLIM-ERAI and ERA5 are somewhat treated as "truth" in this study. I'm missing justification of this. It is known that reanalyses have biases in convective parameters (Taszarek et al 2021).**

   Connected to this, if I didn't miss it, there is no explanation how convective parameters such as CAPE are calculated from the model fields. If I interpret it

correctly, it's a standard model output that is being used? Taszarek et al (2021) suggest calculating e.g., CAPE from the model levels, which is not done in the standard ERA5 and ERA-Interim output as far as I know. Also see e.g., Taszarek et al. (2018) for some of the potential downsides of using Reanalysis data.

Taszarek, M., H. E. Brooks, B. Czernecki, P. Szuster, and K. Fortuniak, 2018: Climatological Aspects of Convective Parameters over Europe: A Comparison of ERA-Interim and Sounding Data. J. Climate, 31, 4281–4308, https://doi.org/10.1175/JCLI-D-17-0596.1.

Taszarek, M., Pilguj, N., Allen, J. T., Gensini, V., Brooks, H. E., & Szuster, P. (2021). Comparison of Convective Parameters Derived from ERA5 and MERRA-2 with Rawinsonde Data over Europe and North America. Journal of Climate, 34(8), 3211–3237. https://doi.org/10.1175/JCLI-D-20-0484.1

Author response

We thank the reviewer for pointing out that HCLIM-ERAI and ERA5 appear to be treated as "truth". That is by no means our intention. We have revised the text to make it clearer that ERA5 is used as a reference dataset rather than as ground truth.

Nevertheless, we are aware of the biases in ERA5, and we have shortly mentioned this in the beginning of Section 3 together with citation to Taszarek et al. 2021. ERA5 is used to provide context that the convective parameters from HCLIM are reasonable. Despite its deficiencies, ERA5 is used here as it is considered the best reference data. Based on the comment from the reviewer, we have, however, revised the text so that it is clearer that the biases we refer to are also in convective parameters, as stated by Taszarek et al. 2021 (please see lines 227-231 in the revised manuscript).

As for how CAPE is calculated, this is explained in lines 182-183 and 188-189 of the revised manuscript. Yes, we use standard model output from HCLIM and ERA5. We are aware of Taszarek et al. (2021) work that recommends the use of model level data, and, due to the reviewer's comment, we now acknowledge this in line 189-190 of revised manuscript. However, we decided to use the standard model output because downloading the full model level data of numerous variables is computationally very expensive. We also cannot access the full model level data

from HCLIM since data is stored only at certain pressure levels. Furthermore, the ERA5 comparison is used to provide context about the reasonability of HCLIM, not to argue that HCLIM is better than ERA5 at representing convective parameters. As noted in the manuscript, there are some differences in the distribution of ERA5 and HCLIM CAPE, which may arise from different calculation methods, spatial resolutions or model physics/dynamics (please see lines 227-231, 244-247). Thus, while calculating the ERA5 CAPE from model levels might somewhat alter the CAPE signals, we believe that it would not alter the main conclusions of this manuscript.

Other minor comments and suggestions:

5. **Line 28: what observations are you referring to? Based on the references probably severe weather reports? The phrasing might be misleading because there are other more indirect observations like satellite or radar? Perhaps specify to "observations in form of direct reports"?**

   Author response

   We initially referred to direct observations such as severe weather reports and weather station data. While it is true that indirect measurements provided by radar, satellite and lightning detection network can give a more consistent picture of convective storm occurrence, rspatially and temporally coherent and quality controlled radar and satellite time series applicable to climatological convective storm studies for the whole Fennoscandian region are quite limited. Thus, also the lack of indirect measurements can make climatological studies in Fennoscandia difficult. We thank the reviewer for pointing out this misleading sentence and we have revised it, please see lines 29-34 in the revised manuscript.

6. **Line 38: Shouldn't it be „convective available potential energy"**

   Author response

   Yes, thank you, we have corrected this.

7. **Lines 71-73: Related to comment 3 above, can you be a bit more specific here? Isn't the main problem with these studies that the statistical model was trained mainly for different climatological regions than Fennoscandia? Or is is this what you meant?**

   Author response

   This paragraph has been modified in the revised manuscript to be more specific (please see lines 83-94).

   We agree that one key justification for a study focusing specifically on Fennoscandia is that broader studies may apply models and proxies trained and validated with pan-European and global observations. We now mention this in line 474-475 of the revised manuscript. However, we also meant that another key justification for a Fennoscandian-wide study, as well as for using HCLIM model data, is the fact that we have a model that has been developed and validated for the Fennoscandian region, including more detailed topography and precipitation patterns. Please also see response to comment 3.

8. **Fig. 2: thunderstorm days determined from environmental parameters are typically higher than observed lightning days because there are at least a few days where no storms are forming, e.g., due to too much CIN (Fig. 3 in Taszarek et al. 2019). Here you show the opposite, which I'm a bit confused about given that the analysis is also partly based on ERA Interim and similar thresholds are used as in Taszarek's study. Or am I missing something? Perhaps connected to this, the numbers in Fig 2a seem a bit higher than in Taszareks anaylsis. Do you have an explanation for this?**

   Author response

   We thank the reviewer for pointing out the study of Taszarek et al. (2019) and for the interesting questions.

   Indeed, the number of observed thunder days (manuscript Fig. 2a) is a bit higher than the number of thunderstorm environments found by HCLIM (manuscript Fig. 2b). The difference between HCLIM and observations might arise from a too high CAPE or precipitation threshold in some regions. We use the threshold of 150 J/kg,

adopted from previous studies, but found that for some regions, a slightly lower CAPE threshold might provide better quantitative comparison with observations. On the other hand, the same is true for the precipitation threshold, for which it is really difficult to define a physically justifiable value. Furthermore, HCLIM might contain biases, and the lightning detection network contains uncertainties especially near the edges of the model domain. Thus, we found that expecting a quantitative correspondence is not meaningful as there is also no universal (or Fennoscandian-wide) threshold for CAPE and precipitation that distinguishes between a thunderstorm and a non-thunderstorm. These points are raised in numerous sections in the manuscript (see lines 232-236, 278-286, 501-509). We added a mention of the sensitivity to CAPE threshold in line 283-284. We also found a fraction of about 0-0.2 of thunderstorm environments occur with model CIN > 75 J/kg, as discussed in lines 495 and shown in Fig. A4 (we note that we've slightly modified the colorbar in this figure to include an even scaling around zero for clarity).

We also examined the differences between Taszarek et al. (2019) and the current study. To our understanding, Taszarek et al. 2019 defines thunderstorm environment from ERA-Interim data using a MLCAPE threshold of 150 J/kg and convective precipitation threshold of 0.075 mm/h. We use the CAPE threshold of 150 J/kg and precipitation threshold of 0.5 mm/h. The different thresholds likely partly explain the differences.

However, we believe that the difference in the number of thunderstorm environments in HCLIM and ERA-Interim (or ERA5) arises partly from the different qualities of the datasets. For example, ERA-Interim and ERA5 have larger grid size than HCLIM, which is typically associated with "smoothing" of precipitation fields compared to higher resolution models and resulting in overestimation of weak precipitation. The reanalysis data is also based on a model that uses a deep convection parameterization. We also tested the use of our proxy criteria on ERA5, as shown in Fig. A1c-d in the manuscript. When the same criteria are applied to ERA5 (using convective precipitation), the number of thunderstorm environments is much higher than that from HCLIM, despite the fact that CAPE environments in ERA5 and HCLIM are quite similar, even though the number of (convective) precipitation environments from ERA5 is a bit smaller than the number of ("total") precipitation environments from HCLIM. This might mean that in ERA5, convective precipitation occurs often when CAPE occurs, whereas in HCLIM (where convection

initiation is not parameterized), precipitation occurs less often when CAPE is sufficient.

We also compared the number of observed thunder days in Taszarek et al. 2019 Fig. 3 and our manuscript Fig. 2a. Taszarek defines a thunder day when two flashes are detected in a grid box (and use the time period 2008-2017). Our definition of a thunder day includes days with at least one flash per (a smaller) grid box (and from a time period of 2002-2018).

Comments by Reviewer 2

In my understanding, this study aims to build on existing literature relating to proxies for thunderstorm activity in Europe by studying the Fennoscandia region specifically. The work demonstrates the value of locally tuned models, in this case HARMOIE. It also shows that thunderstorm environments are expected to increase in frequency in this region, but with wide uncertainty.

This is a good piece of work, but I believe the authors could go further in contextualising the results and addressing the underlying physical processes, and that there is value in doing so. That CAPE increases in Europe in climate models is fairly well known but the most interesting analysis would be to compare Fennoscandia with other regions.

1. **Lines 71-73 raise an important point. What are 'processes relevant' and how are they different in the Fennoscandia region compared to continental Europe?**

   Author response

   We thank the reviewer for pointing out the unclear sentence, as was also mentioned by Reviewer 1. We have clarified this paragraph in the revised manuscript (please see lines 83-94 in the revised manuscript). We now explicitly state the processes and characteristics that may be better represented in HCLIM compared to coarser models and earlier studies. This revision is also related to Reviewer 1 comment 3.

2. **Lines 153-156 outline the thresholds used. From the literature, I believe the DLS threshold comes from Púčik, where sounding observations suggested that DLS above 15 m/s was likely to be indicative of supercells. Are we observing supercells in this more northerly region?**

   Author response

   Yes, supercells are a relatively common type of convective storm in Fennoscandia, as described in the study of Tuovinen et al. 2015. However, many supercells likely go undetected. Furthermore, as far as we are aware, there is no Fennoscandian-wide study of supercell occurrence. Nevertheless, Tuovinen et al. (2015) found that the mean 0-6 km shear during significant hail-prone days in Finland was 17.5 m/s and that the large majority of these cases were associated with supercells. Tuovinen et al. (2015) also note that, interestingly, in most of the storms producing significant hail in Finland, around 40% did not have the minimum shear of 15 m/s commonly expected for very large hail in other regions.

   We have added a brief mention of the results of Tuovinen et al. in the revised manuscript (please see lines 195-198).

3. **Lines 258-260, can you expand a bit on this and what may actually cause the differences observed? We expect some differences but are they due to noise and definitely not due to missing any of the physical processes.**

   Author response

   We believe that it is mainly the thresholds set for CAPE and precipitation that explain the differences. Using a slightly smaller threshold for CAPE (we use the threshold 150 J/kg) would have led to better correspondence in some regions and may have been as physically justifiable. For example, a machine learning based study from Finland found that a (ERA-Interim) CAPE threshold of 113 J/kg was a good predictor for lightning in Finland. On the other hand, in HCLIM, our proxy with the threshold of 150 J/kg also overestimates the numbers compared to observations in some regions, and a lower threshold would overestimate it even more. This highlights the fact that no universal threshold for CAPE exists. Furthermore, changing the threshold for precipitation would also change the numbers, and for that it is very difficult to set a physically justifiable threshold (i.e.

which precipitation 3-hourly amount distinguishes convection initiation from cases where convection does not initiate).

Based on the reviewer's comment, we have expanded discussion about this in the revised manuscript (see lines 278-286). We also discuss the limitations regarding the use of fixed thresholds in lines 232-236, 278-286 and 501-509.

4. **Section 4.3, the discussion here is good but could benefit from greater depth. What causes a decrease in DLS environments and does this not reduce the number of severe storms given DLS was identified as necessary for severe convection likely to lead to greater hazards.**

   Author response

   We hypothesize that the decrease in wind shear may be associated with a weakening of the mid-latitude summer circulation due to Arctic Amplification (Coumou et al. 2018). A similar decrease was also seen in Pucik et al. 2017 annual data using EURO-CORDEX as well as by Thurnherr et al. 2025 using a convection-permitting climate model in southern Fennoscandia.

   However, indeed, we find that despite the overall decrease in shear, the number of cases with co-occurring CAPE, DLS and precipitation are shown to increase. This suggests more frequent conditions favourable for severe weather in Fennoscandia and that the decrease in DLS occurs partly outside of the unstable situations. Due to the comment by the reviewer, we discuss in more detail in the Discussion section (please see lines 457-463 of the revised manuscript).

5. **Line 410 onwards, aims to put the results in context of other studies. I think this paper needs to go further here. I believe other works such as Púčik predicted minimal change in Fennoscandia and I have seen others such as Battoglioli suggesting minimal thunderstorm activity in the Scandi region based on proxies developed in central Europe. I think it would be of great interest to assess in more detail whether the results from a local model are different to other works and if so why? If the background processes leading to thunderstorm days are**

not well captured by proxies based on continental Europe then this would be an interesting result.

Author response

We agree with the reviewer that a more thorough comparison with previous studies would improve the manuscript. We have therefore included a more detailed comparison to previous studies in the Discussion section, including the study of Battoglioli. Please see lines 442-479 in the discussion section of the revised manuscript. Please also see response to Reviewer 1 comment 1, which is related to this revision.

6. **Lines 474-476, this is an important limitation in the literature buried near the end of this paper.**

Author response

This limitation is now also mentioned in the introduction of the revised manuscript. Please see lines 29-34 in the revised manuscript and response to Reviewer 1 comment 5.

Additional revision made by the authors

Due to large revisions made in the introduction and discussion sections of the manuscript, we have omitted parts in the conclusions section to avoid repetition of previously discussed limitations.

References

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