Response to referees (egusphere-2025-3663)

Sea-effect snowfall in the Baltic Sea area in 1998-2018 derived from convection-permitting climate model data

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We thank both referees for their insightful comments and constructive feedback. We have carefully considered all comments made by the referees and revised the manuscript accordingly. Please find our point-by-point response to each comment below. The original comment is presented in bold, followed by our responses. In the response, we indicate the corresponding revised manuscript line numbers in red.

Referee 1

Summary:

The authors present a 21 year climatology of sea-effect snowfall events over the Baltic Sea using the HCLIM model. I think the paper is clearly written, the method is tested and shown to work well for the intended purpose, and limitations are discussed. I only have two minor substantive comments and spotted a couple of typos/grammar errors. I recommend publication after minor revisions.

Substantive comments:

1. Could you add some discussion about different potential methods to detect snowbands (e.g., using model reflectivity or satellite data), and why you chose your more indirect method. Are direct methods too complex and hence error-prone (maybe involving pattern recognition etc.)?

We thank the referee for the interesting question. To our knowledge, there is no radar or satellite dataset that would cover the entire Baltic Sea region, span multiple decades, be quality controlled, and without temporal or spatial gaps. Furthermore, objectively identifying sea-effect snowfall systems from those datasets (by e.g. pattern recognition) is difficult as the systems are typically shallow and quite local and may be embedded within larger scale snowfall. The snowfall associated with snowbands is often also missed by the relatively coarse surface observation station network. Therefore, forming a coherent climatology of sea-effect snowfall using direct methods is complicated. Similarly to previous studies, we investigate the climatology of sea-effect snowfall using a convection-permitting climate model that simulates a wide spectrum of dynamical processes in the atmosphere.

The limited availability of useful datasets for climatological studies of sea-effect snowfall is briefly discussed in the introduction of the revised manuscript (see L58-63 in the revised manuscript).

2. I didn't understand why Fig. 9 was divided into subregions--what is the purpose of introducing these regions?

By dividing the Baltic Sea into smaller subregions, we can provide estimates of the snowfall distribution over land, not only over the sea. By defining a relatively small subregion and a relatively narrow range of low-level wind directions, we narrow down our sample of snowband days into those that are more likely associated with sea-effect snowfall systems that eventually drift over land.

Typos/grammar:

3. Line 7: Add "the" before majority

The manuscript is revised accordingly (see L9 in the revised manuscript).

4. Line 67: The previously made studies --> Previous studies

The manuscript is revised accordingly (see line L74 in the revised manuscript).

5. Line 124: "... criteria of OLS2022 were adopted from OLS2022" sounds a little redundant.

Perhaps the referee misread the text, as the sentence in the original manuscript was "The snowband criteria of OLS2022 were adopted from OLS2020", highlighting that the criteria of Olsson et al. 2022 are similar to those from Olsson et al. 2020. Nevertheless, this sentence is slightly edited in the revised manuscript to make it clearer (see L154-156 in the revised manuscript).

Please note that in the revised manuscript, we corrected a typo and refer to the study as OLS2023, not OLS2022.

6. Line 136 (and elsewhere): From hereon --> Hereafter

The manuscript is revised accordingly (see L45 and L49-50 in the revised manuscript).

7. Line 276: into --> of

The manuscript is revised accordingly (see L206 in the revised manuscript).

8. Line 188: Could add a short one-sentence subsection summary?

A subsection is already devoted to a summary (see Section 3.2.5, "Summary," in the original and revised manuscript). Therefore, after considering the suggestion, we decided not to add an additional one-sentence summary as suggested by the referee, but instead added a sentence referring to Section 3.2.5 (see L218 in the revised manuscript).

9. Line 200: Doesn't Fig. 1 include southern Finland?

The referee is likely confused due to a misplaced "not shown" statement, which was meant to point out that a snowband system drifted over southern Finland on 3 February, but this was not shown in Fig. 1 of the original manuscript. We have revised the sentence in the manuscript (see L236-237 in the revised manuscript).

10. Line 245: Is the most snowfall related to the orientation of the coast relative to the winds, or to the fetch?

We thank the referee for the intriguing question. We first note that the sentence in question refers to Fig. 7 in the original manuscript (Fig. 6 in the revised manuscript), which shows the number of snowband days rather than the amount of snowfall. We are therefore slightly uncertain about the specific aspect the referee is referring to.

In the original manuscript at line 245, we intended to highlight that for winds from 0–180°, many snowband days occur near the eastern coast of Sweden and the southern coast of Finland (i.e. downwind of the sea areas). Snowband days in these regions typically occur when northerly to southeasterly winds transport cold arctic or continental air masses over the Baltic Sea and provide a long fetch over open water. In order to avoid any confusion, we have revised the paragraph to reflect our meaning more clearly (see L282-290 in the revised manuscript).

We further speculate in this response that conditions favorable for snowband days may occur more frequently near the eastern coast of Sweden than, for example, the southern coast of Finland, since the fetch over the Bothnian Sea is long for a wider range of wind directions than over the narrower Gulf of Finland.

11. Line 264: Detected *snowband* days?

We have revised the manuscript accordingly (see L305 in the revised manuscript).

12. Line 304: Add "the" before majority

We have revised the manuscript accordingly (see L357 in the revised manuscript).

13. Line 306: Above mentioned

We have revised the manuscript accordingly (see L359 in the revised manuscript).

14. Line 334, 371: use --> utility of HCLIM

We have revised the manuscript accordingly (see L389 and L426 in the revised manuscript).

15. Line 375: seek for --> seek to identify?

We have revised the manuscript accordingly (see L430 in the revised manuscript).

16. Line 381: Maybe use the plural (uncertainties)? Also, I wasn't sure what you were trying to say. Do you mean temporal variability with period on the order of 20 years? Natural variability by itself does not render climatologies inaccurate (no?).

We thank the referee for pointing out the unclear sentence. The goal of the sentence was to point out that since the 21-year period is a bit short for climatological studies, some of the local occurrence minima and maxima are likely influenced by the stochastic nature of the snowbands. If we would compare our results to those obtained from a longer dataset, there might be some local-scale differences due to natural spatial and temporal variability of the snowbands. We have clarified the sentence in the revised manuscript (see L434-438 in the revised manuscript).

17. Line 389: I'm not sure if the last paragraph/advertisement is needed (but will leave it up to you).

After careful consideration, we have decided not to remove this paragraph as, in our opinion, it provides useful context for the applicability of the results. However, the paragraph is slightly edited due to referee 2 comment 23 (see L445-449 in the revised manuscript).

Referee 2

This study uses a 21-year convection-permitting regional climate simulation over the Baltic Sea to derive a climatology of sea-effect snowfall events in the form of convective snowbands. To this end, a previously established algorithm is used to identify snowbands in the climate simulation. The analysis is straightforward and the authors present interesting regional differences in the frequency of snowbands, and they show that most events occur in situations with specific low-level wind directions varying between regions. While these climatological results are per se all fine, the paper so far misses the opportunity to learn more about the synoptic and mesoscale dynamics associated with these events. Given the fact that the authors submitted their study to a dynamics journal (and not, e.g., to the Int. J. Climatology), I would like to request the authors to perform a few additional analyses, which would greatly increase the attractiveness of this paper for the WCD readership. A few suggestions are given below, together with more minor suggestions for amendments.

Major comments

1. The WCD readership would be very interested in learning more about these snowfall events, in particular about the synoptic and mesoscale environment in which they occur. Currently the reader is only presented with maps of snowband frequency and intensity. An interesting and not very difficult-to-perform extension would be to focus on maybe three snowband hotspot regions in different parts of the Baltic Sea, and then show composite maps of, for instance, sea-level pressure (to see the location of cyclones), equivalent potential temperature at 850 hPa (to see the location of fronts), and geopotential height at 500 hPa (to get an idea of the associated upper-level Rossby wave pattern) on snowband days in the three regions. It would also be interesting then to see how strongly the composites differ between the regions. Without such an analysis, this is a pure climatology paper, which would miss the core aspect of the journal.

We appreciate the insightful suggestion. As suggested by the referee, we provide an additional analysis in the revised manuscript regarding the composite synoptic environment associated with snowband days occurring near the eastern coast of Sweden, southern coast of Finland and northern coast of Poland (see new Fig. 9 in the revised manuscript). We focus on the three subregions with frequent snowband days presented in Sect. 3.5 and Fig. 9 of the revised manuscript and calculate the composite mean 500 hPa-level geopotential height and mean sea-level pressure using HCLIM data. Additionally, we provide new maps of 500 hPa-level geopotential height, mean sea-level pressure, 850 hPa-level temperature and sea ice extent in HCLIM for the four case studies presented in Sect.

3.2 and Fig. 4 of the revised manuscript (see new Fig. A2 in the revised manuscript). We have revised the text in Sect. 3 accordingly to implement brief discussion on the synoptic environments associated with snowbands in the regions, and make comparisons with previous studies where suitable (see L224-227, L230-235, L251-253, L258-260 in Sect. 3.2 and L339-349 in the new Sect. 3.5).

To summarize, the revisions include the addition of new Fig. 9 showing the composite mean synoptic environment associated with snowband days in selected regions, new Fig. A2 showing the synoptic environment associated with the four snowband cases, and inclusion of new text on see L224-227, L230-235, L251-253, L258-260 in Sect. 3.2 as well as on L339-349 in the new Sect. 3.5.

2. The introduction quickly goes into listing many papers that documented snowband events or regional climatologies, but I am missing a bit more information about the characteristics and physics of these events. What are typical spatial and temporal scales? How fast do they propagate? What is known about the physical processes that lead to the formation of snowbands? A concise review on these aspects would greatly complement the mainly descriptive parts of the introduction. Some of this comes in L56-60, but there it appears as algorithmic criteria. I would find it more interesting, if this was presented earlier and with specific references, e.g., about the 13 K vertical temperature difference etc.

We agree with the referee and have modified the introduction of the manuscript to include more detailed descriptions of the dynamics and properties of sea-effect snowfall systems. We describe the key factors affecting the formation of sea-effect snowfall and most intense snowbands as well as the typical size, shape and duration of the systems over the Baltic Sea by citing earlier studies on these topics. We have moved the algorithmic criteria referred to by the referee to the methods section (Sect. 2.2). Please see revised text on L22-45 and L143-154 in the revised manuscript.

Minor comments

3. I suggest that you add in the abstract a brief description of how you defined / identified "convective snowbands" (main criteria about sea surface temperature and vertical temperature difference).

Thank you for pointing this out. We have revised the manuscript accordingly (see L5-7 in the revised manuscript).

4. L8: please specific at what level these winds are.

We have revised the manuscript accordingly (see L10 in the revised manuscript).

5. L16: what data is used in Figure 1?

Figure 1 of the original manuscript presents observed radar reflectivity in units of dBz. As requested by referee 2 in their comment 16, Figure 1 of the original manuscript has been integrated into Figure 5 of the original manuscript (please see new Fig. 4 in the revised manuscript). The radar reflectivity data used in the new Fig. 4 is obtained from the BALTEX Radar Data Centre. This has been clarified in the revised manuscript (see L221-222 in the revised manuscript).

6. L64: "a deep BL is necessary …" – I think that the deep BL is an effect of the strong convection and intense surface fluxes, and not the other way round.

We thank the referee for pointing out this inconsistency. We have revised the sentence in the manuscript (see L150-151 in the revised manuscript).

7. L65: strange sentence, of course convection can be dry and without precipitation, and it could also be that rainfall occurs and then it would still be convection. I don't think that snowfall is strictly a criterion for convection.

We appreciate the referee for pointing out the unclear sentence. The occurrence of precipitation is typically included to serve as a proxy for the initiation of precipitating convection when the environmental conditions are favorable. Since we focus on sea-effect snowfall, a threshold for snowfall is used. We have modified the sentence in the revised manuscript (see L151-153 in the revised manuscript).

8. L70: you correctly write that your simulation explicitly resolves "deep convection" but snowbands involve mainly shallow convection … do you still have a shallow convection parameterization in your model setup? The answer is yes (L107) – please make sure that this is already clear in the introduction.

Yes, the shallow convection parameterization is turned on in the model, as stated in Sect. 2.1 of the original and revised manuscript. We have revised the introduction to clarify that wintertime convection is typically shallower and less intense than summertime deep convection. It was not our intention to imply otherwise, and we therefore thank the referee for pointing this out. However, we expect that a convection-permitting climate model with high spatial resolution and non-hydrostatic dynamics, such as HCLIM, provides added value for simulating the spatio-temporal evolution, structure, organization, and intensity of convective precipitation systems also in the cold season, compared to coarser models.

Please see the modified text on L77-87 in the revised manuscript as well as our response to comment 22.

9. L76: I agree that this is likely the case but maybe remind the reader that winter convection is typically shallow and not deep as in summer and therefore still parameterized.

We have revised the manuscript. Please see L77-87 in the revised manuscript and response to comment 8 and 22.

10. L113: should read "if all criteria are …".

We have revised the manuscript accordingly (see L136 in the revised manuscript).

11. L115: here I am slightly confused: don't you have a criterion about the spatial extent of the snowbands? Is it possible that a snowband day fulfils the criteria only at a single grid point?

We do not have a criterion for the spatial extent of the snowbands. In Figs. 3-7 of the revised manuscript, in each grid box, a snowband day is found if that grid box fulfills all the criteria. In Tables 2 and 3 and Figs. 2 and 8-10 of the revised manuscript, a snowband day in a subregion is found if there is at least one grid box at least once per day that fulfills all the criteria. Therefore, as stated on L270-273 of the revised manuscript, the criteria do not always capture the entire areal extent or the duration of the individual snowbands (this is evident especially for the case presented in Figure 4i-l of the revised manuscript). This is partly due to the relatively high threshold used for snowfall amount. We note that a similar choice has been made in the studies of Olsson et al. (2020, 2023) to which the detection method and criteria are based on and comparison is made with in the present study.

All in all, the detection method certainly has its limitations, as is discussed in Sect. 3.2, Sect. 4 and Sect. 5 (see L270-273, L390-394 and L429-434 in the revised manuscript), and therefore there is some uncertainty in the absolute number of detected snowband days. However, case studies and comparison to previous studies suggest that the detection method and HCLIM can represent snowbands and the areas most at risk from snowbands rather well.

12. L124: I don't understand … if OLS2022 is the same as OLS2020 plus a snow depth criterion, and then you skip the snow depth criterion, then this means that you use the OLS2020 criteria, why then do you call it the OLS2022 criteria (L127)?

Thank you for the careful reading. We have included the criteria of OLS2022 in the current study as that study, not OLS2020, is used in the comparative analysis of Sect. 3.1. Therefore, we thought it best to refer to the criteria as those of OLS2022.

Please note that in the revised manuscript, we corrected a typo and refer to the study as OLS2023, not OLS2022.

13. L142: this is very difficult to read, please write “the median is … and the 98th percentile is …” (or similar).

We have revised the manuscript accordingly (please see L172-174 in the revised manuscript).

14. L156: should read “Fig. 3”. Similarly in other places.

Thank you for pointing this out. We have revised the manuscript accordingly.

15. L165: “observed” is misleading, I assume that snow depth criterion was for simulated snow?

In the study of OLS2023, the snow depth was from observations. However, we agree and revised the sentence accordingly (see L195 in the revised manuscript).

16. Figures 1 and 5: it would be much more interesting to have the snowfall from radar and the simulation next to each other and plotted in the same domain. I suggest integrating Fig. 1 as additional panels in Fig. 5.

We thank the referee for the nice suggestion and have integrated the radar reflectivity to the figure showing simulated snowfall. Please see new Fig. 4 in the revised manuscript.

17. L236: is your conclusion then that the loose criteria are better?

Our conclusion is not that the loose criteria are better, we simply provide this as an interesting sensitivity test. While the loose criteria detect more grid boxes than the other criteria, it may also more easily detect non-convective snowfall. We have slightly edited the sentence (see L273-274 in the revised manuscript).

18. L245: should read “indicates”.

We have revised the manuscript accordingly (see L283).

19. L261: is “Gulf of Bothnia” the same as “Bothnian Bay” in Fig. 1? If yes, then please use consistent terminology and refer the reader to Fig. 1.

We have revised this sentence so that it is in line with the areas defined in the map (Fig. 1 of the revised manuscript). Gulf of Bothnia contains both the Bothnian Bay and Sea of Bothnia, which are indicated in Fig. 1. See L302-304 in the revised manuscript.

20. Figure 8: could you insert here the climatological sea ice edge for each month? And this brings me to the question how sea ice is treated in the model simulations?

We have added the monthly mean sea ice edge from HCLIM into Fig. 7 of the revised manuscript (see also L298-300 in the revised manuscript).

In HCLIM, sea ice fractions are updated from the forcing model every 6 hours, while temperatures of the sea ice are computed using the Simple ICE (SICE) parameterization (Batrak et al. 2018). We have provided more detailed information on how sea ice is treated in the model in the revised manuscript (see L117-124 in the revised manuscript).

21. Figures 10 and 11 could be combined into one figure (3 panels from Fig. 10 on the left, and 3 panels from Fig. 11 on the right).

We have revised the figures as suggested. Please see Fig. 9 in the revised manuscript.

22. L325: I am not sure that deep convection plays an important role for these cold season snowbands?

We have reformulated this sentence in the revised manuscript (see L377-382 in the revised manuscript). HCLIM has a superior spatial resolution (3 km), uses non-hydrostatic dynamics and explicitly resolves deep convection. Even though the resolved convection and high resolution likely bring more added value for summer than for winter convection, we expect them to be beneficial also in the latter.

Please note that this point is also related to referee 2 comments 8–9, based on which we have clarified in the introduction of the revised manuscript that sea-effect snowfall is typically associated with shallower and less intense convection than summertime deep convective systems.

23. L391: I don't understand how the climatology helps operational forecasting. Can you explain or omit this statement (also in the abstract).

We believe that understanding the local climatology and favorable wind directions provides useful context for weather forecasters, however we have reformulated this sentence in both the main text and abstract of the revised manuscript (see L13-14 and L445-449 in the revised manuscript).

Additional changes made by authors:

To avoid any confusion, we would like to point out for the referees that we noticed a typo in the frequently used and abbreviated citation provided for Olsson et al. 2022 (OLS2022).

The correct citation and abbreviation is Olsson et al. 2023 and OLS2023. We have corrected the typo in the revised manuscript.

References:

Batrak, Y., Kourzeneva, E., and Homleid, M.: Implementation of a simple thermodynamic sea ice scheme, SICE version 1.0- 38h1, within the ALADIN-HIRLAM numerical weather prediction system version 38h1, Geosci. Model Dev., 11, 3347–3368, https://doi.org/10.5194/gmd-11-3347-2018, 2018.

Olsson, T., Luomaranta, A., Jylhä, K., Jeworrek, J., Perttula, T., Dieterich, C., et al.: Statistics of sea-effect snowfall along the Finnish coastline based on regional climate model data, Advances in Science and Research, 17, 87–104, https://doi.org/10.5194/asr-17-87-2020, 2020.

Olsson, T., Luomaranta, A., Nyman, H., and Jylhä, K.: Climatology of sea-effect snow in Finland, International Journal of Climatology, pp. 1–18, https://doi.org/10.1002/joc.7801, 2023.