Response to referees (egusphere-2025-3663)

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

By M. Virman, T. Olsson, P. Lind and K. Jylhä

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 below our point-by-point response to each comment, with the original comment presented in bold, followed by our response.

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 reviewer 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 will be briefly discussed in the introduction of 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

We will revise the manuscript accordingly.

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

We will revise the manuscript accordingly.

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 study of Olsson et al. 2022 is adopted from Olsson et al. 2020. Nevertheless, we will revise this sentence in the manuscript to make it clearer.

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

We will revise the manuscript accordingly.

7. Line 276: into --> of

We will revise the manuscript accordingly.

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 manuscript). Therefore, we do not plan to add an additional one-sentence summary at L188 of the original manuscript but instead will add a sentence referring to Section 3.2.5.

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 will revise the manuscript accordingly.

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, 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 will revise the paragraph to reflect our meaning more clearly.

We speculate 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 will revise the manuscript accordingly.

12. Line 304: Add "the" before majority

We will revise the manuscript accordingly.

13. Line 306: Above mentioned

We will revise the manuscript accordingly.

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

We will revise the manuscript accordingly.

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

We will revise the manuscript accordingly.

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 reviewer 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 will clarify the sentence 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.

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 will 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. We will focus on the three subregions with frequent snowband days presented in Sect. 3.4 and Figs. 10-11 of the original manuscript and calculate the composite mean 500 hPa-level geopotential height and mean sea-level pressure using HCLIM data. Additionally, we will provide 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. 5 of the original manuscript. We will revise 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.

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 will modify the introduction of the manuscript to include more detailed descriptions of the dynamics and properties of sea-effect snowfall systems. We will 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 plan to move the algorithmic criteria referred to by the referee to the methods section (Sect. 2.2).

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).

We will revise the manuscript accordingly.

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

We will revise the manuscript accordingly.

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 will be integrated into Figure 5 of the original manuscript. The radar reflectivity data used in the new figure will also be described more clearly 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 reviewer for pointing out this inconsistency. We will revise the sentence in the 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 will clarify the sentence 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 manuscript. We will revise 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 reviewer 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 also see our response to comments 9 and 22 of referee 2.

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 will revise the manuscript accordingly. Please see response to referee 2 comment 8.

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

We will revise the manuscript accordingly.

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. 4-8 of the original 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. 9-11 of the original manuscript, a snowband day in a subregion is found if there is at least one grid box at least once per day that fullfills all the criteria. Therefore, as stated in Sect. 3.2.5 and Sect. 5 of the original 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 5g-h of the original 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 of the original 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. Since the study used for comparison is OLS2022, we refer to the criteria as OLS2022. Furthermore, OLS2020 did not have a criterion for observed snow depth, and they did not limit their analysis into the five subregions (with different wind direction criteria) used by the referee. Nevertheless, we will clarify this sentence in the revised manuscript.

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

We will revise the manuscript accordingly.

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

Thank you for pointing this out. We will revise the manuscript accordingly.

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

We agree, and will revise the sentence accordingly.

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 will integrate the radar reflectivity images presented in Figure 1 into Figure 5 and omit the original Figure 1 from 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.

18. L245: should read "indicates".

We will revise the manuscript accordingly.

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 will revise this sentence so that it is in line with the areas defined in Figure 2 of the original manuscript. Gulf of Bothnia contains both the Bothnian Bay and Sea of Bothnia, which are indicated in Figure 2.

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 will revise the figure to add the monthly mean sea ice edge from HCLIM into Figure 8 of the original 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 will provide more detailed information on how sea ice is treated in the model 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 will revise the figures accordingly.

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

We will reformulate this sentence 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 will clarify in 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 favourable wind directions provides useful context for weather forecasters, however we will reformulate this sentence in both the main text and abstract of 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 will correct this in the revised manuscript.

References:

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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.

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