Interactive comment on "Characteristics of Convective Snow Bands in the Baltic Sea Area" by Julia Jeworrek et al.

Response to Referee #1

We would like to thank Reviewer #1 for the helpful and insightful comments on the manuscript.

1) We suggest a change of the title to "Characteristics of Convective Snow Bands along the Swedish East Coast" to be more clear about the region of interest for this study. We agree that other areas affected by Convective Snow Bands in the Baltic Sea area should be mentioned in the introduction as well. However, the focus is on the Swedish East coast because the atmospheric conditions causing lake effect snow in Sweden have clearly repeating patterns, while other areas could experience snow bands under different atmospheric conditions (e.g. other wind directions due to other coastal orientations). Convective snow bands in the Gulf of Finland have been studied widely before. Extending our method to the entire Baltic Sea would require a different approach with more lose and generous criteria. This could lead to capturing other precipitation events, which are unrelated to convective snow bands, also in different regions and would manipulate the climatological results. This will be further clarified in the introduction.

2) A regional climate model was chosen instead of a numerical weather prediction (NWP) model to evaluate the potential of applying climate model and make climatological studies (in contrast to most previous studies being higher resolution process studies). Performing simulations at a high resolution is computationally expensive and time consuming, and therefore it is not reasonable to run a NWP model at a climatological time scale. The challenge in choosing an appropriate model for climatological studies of meso-scale phenomena is to find a balance between computational expense and accuracy of the simulated physical processes. In order to understand how precisely the RCA model performs for the atmospheric conditions associated with convective snow bands, case studies have been carried out and different setups of the RCA model were evaluated specifically for those case studies. Being aware of the benefits and weaknesses of the chosen model setup helps for the interpretation of the climatological results.
The new contribution, in terms of the modelling, is to show that a relatively coarse-resolution model can be used with the potential of applying it to climatological studies. The conclusions are (and here we agree with the reviewer, that the results are not very surprising) that introducing higher resolution makes a difference and that the better SST makes a difference. This has the implication that we can make a climatology based on a regional climate model, and it would also be possible to investigate the impact of climate change on the frequency of occurrence (or distribution). Additional text will be added in the manuscript to clarify this.

3) We will clarify in the paper that the snowfall occurs not only at the coast, but also over the sea.

4) The purpose is not to make a through analysis of the agreement between remote sensing products and modelling results. We make a sensitivity analysis on the factors in the mode (set-up, resolution etc.) influencing the results. The different set-ups of the

model are compared with observed precipitation to identify problems in the model in reproducing the accurate precipitation. We agree with the reviewer that it would be very interesting to evaluate the precipitation rates based on radar products. We, however, consider this to be outside the scope of the paper. The satellite image is included merely as an illustration of the analysed situation, rather than to be used as a measure on the accuracy of the models. We would like to keep the image.

5) The aim is to define simplified conditions reflecting the local and large scale for the occurrence of precipitation related to convective snowbands. Large scale conditions are expected to be enough reflected in the wind and temperature conditions (defined over the larger area). This will be additionally discussed in the text.

6) More references will be reviewed and cited in the introduction. However, although my criteria are based on other references, they have been adjusted to the region by investigating the resulting days by hand and confirming them with satellite images. Anyhow, the criteria are logical and generally valid. Including weaker criteria in another category also gives another 'buffer zone' for a pool of days to be careful with in the dataset.

7) The data shown in the figures 4, 5 and 6 represent the accumulated hourly snowfall rates. By definition of the snowfall parameter in the model, there is no rain included. As explained in page 6, line 33 and following, the criteria in table 1 were only applied for the specific sectors (as in figure 2). The temperature differences are therefore only fulfilled for the reference sectors, not over the Gdansk region. Showing a larger area than the selected reference sector of interest helps determining other potential areas which can be affected by lake affect snow under the same conditions as the Swedish east coast experiences them. References for convective snow bands affecting the Gdansk region can be included in the revised version of this paper.

Specific comments

"The precipitation amounts connected to these snow bands are not large or exceptional, even 17 mm per 24h is too low to call extreme. (the paragraph beginning at p 6 line 22)"

– Right! Probably because of the low resolution of the model. All precipitation values are underrepresented, so perhaps in reality they are extreme. However, I removed the word 'extreme' in the paper.

"Why only the precipitation for the sector shown in Fig 4 is presented? Are other data missing or the days selected could not be associated with precipitation in other regions?"

– We are showing a larger environment around the applied sector to see effects in other regions (such as in Gdansk). No data is missing, but we wanted to keep the focus on the Baltic Sea and not confuse the results with other precipitation areas that are not associated with convective snow bands. We could extend the area to the Gulf of Finland, that could be interesting too, but we would have to be careful with the interpretation.

Response to Referee #2

We would like to thank Reviewer #2 for the helpful and insightful comments on the manuscript.

1) Following the comments by both reviewers we suggest the title to “Characteristics of Convective Snow Bands along the Swedish East Coast”.

2) The second section of the paper provides a small literature review about previous studies and current knowledge about convective snow bands to the extent required to motivate the method of this paper. However, more literature will be reviewed in the introduction to give a better background on the state of art. The objective and research questions will be formulated more clearly.

3) The first two sections will be combined in the final paper to merge objectives and research questions better with the literature review of previous studies.
The synoptic situations leading to convective snow bands can be very different. What they have in common are the strong pressure gradients over the Baltic Sea guiding cold air masses from the northeast over the warm water surface towards Sweden. Cold fronts move commonly from west to east, while snow bands only affect the Swedish east coast at strong prevailing NE winds. This unusual synoptic flow can be caused by a deep low pressure system southeast of the Baltic Sea and/or indirectly by a local high pressure developing over the cold north of Scandinavia. Since the large scale synoptic situation for convective snow bands cannot be identified by typical conditions and only the strong NE winds with small vertical shear advecting cold air matter in connection with the local conditions, I consider the present description to be sufficient.
Sea ice limits the heat fluxes from the sea surface and changes the coastline. The development of snow bands requires an ice-free and open water surface. This was discussed at different parts of the paper. However, it can be clarified in the introduction once again.

4) The paragraph repeating information shown in the table will be shortened or even skipped.

5) An 11-year dataset cannot be understood as climatology and the mean results are clearly biased by single events. The phrase “climatology” was therefore not used in the paper. However, with various snow band events occurring per year, a dataset covering 11 years is able to represent qualitative distributions of affected regions. It can therefore be assumed that Gävle and Västervik experience the most intense convective snow band events in the studied area, the Swedish east coast. That also other locations at the Baltic Sea are affected under similar atmospheric conditions has been seen in the Gdansk region. The focus of this study is on the Swedish East coast because the atmospheric conditions causing lake effect snow in Sweden have clearly repeating patterns, while other areas could experience snow bands under different atmospheric conditions (e.g. other wind directions due to other coastal orientations). Convective snow bands in the Gulf of Finland have been studied widely before. Extending our method to the entire Baltic Sea would require a different approach with more lose and generous criteria. This could lead to capturing other precipitation events, which are unrelated to convective snow bands, also in different regions and would manipulate the results.

6) These are the months with the highest frequency, this should be clarified in the text.

7) We apologize for the confusion. We mean northeast winds, which are related to cold air outbreaks from Finland or Russia. Westerly winds are usually not cold enough in winter and could either way not generate snow bands affecting the Swedish coast, because they align with the wind direction.

8) The figure numerations are corrected now.

9) The typo has been corrected now.

10) The reference has been corrected.