Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-25-AC1, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "The sensitivity of snowfall to weather states over Sweden" *by* Lars Norin et al.

Lars Norin et al.

lars.norin@smhi.se

Received and published: 28 June 2017

We thank the referee for the time and effort devoted to review this manuscript as well as for the very constructive comments and suggestions. Below, please find a point-by-point reply to the comments (reproduced in italics).

General comments to the authors:

1. This seems like a precipitation science paper, but has been submitted to a journal that generally deals with advances to methods of atmospheric measurement. If you can make a few additions that steer the paper slightly in that direction — for example by describing how your methodology could (or could not) be applied in

analysis of other snowy regions — that would make it a better fit for the journal.

We agree that the manuscript is more scientific in nature. But one of the main reasons for submitting in AMTD was to demonstrate the usefulness of long-term radar observations in Sweden and how the synergy with the latest space-based observations (specifically CloudSat) can be exploited to study snowfall. We believe such study can also be done for the other regions that have long-term radar observations.

2. In a few parts of the paper, the effects of the frequency/intensity of snowfall in a given weather state on one hand, and the frequency of that weather state on the other, are mixed. Maybe you could add a table that shows the relative frequencies of the different weather states; that would help decouple those effects.

We thanks the referee for the suggestion. A table listing the number of days containing the different weather states (as well as how many other other weather states occurred for those days) has been added to the revised manuscript.

3. Related to point 2, the statistical analysis is somewhat muddled by the fact that the occurrences of the different weather states are likely not independent. The prevailing winds, the NAO index and the occurrence of high/low pressure systems are probably correlated with each other at least to some degree. This should also be quantified, and if there are strong correlations, the impact of those on the results should be discussed.

As mentioned above, a table listing dates with simultaneous weather states has been added. We agree that there are correlations between some of the different weather states. However, within the different groups of weather states (wind directions, high and low MSLP, and different NAO indices) there are no overlapping dates. Nevertheless, the stronger correlations are pointed out in the revised manuscript.

4. The analysis relies on the "prevailing" wind and pressure conditions, but Sweden is a big country and there can be quite some variability in those conditions between the southern and northern extremes. For example, Figs. 1b and 1d show that there can be about 90 deg difference in the 850 hPa wind direction between the north and the south. In fact, in "northeasterly" wind conditions in Fig. 1b, the prevailing winds in the far north appear to be from the northwest! And if one follows the streamlines in that figure, it appears that the flow to most of Sweden appears to originate from the Norwegian Sea in the (north)west rather than from the Barents Sea in the northeast. I wonder how much effect this has on the results, especially as the authors discuss differences between northern and southern Sweden in several places in the paper.

The weather states were calculated in a paper by Thomas and Devasthale (Atmos. Chem. Phys., 14, 11545–11555, 2014). In their paper they evaluate the weather states using measurements from an area positioned over southern Sweden. This is the reason why certain wind directions can appear differently in the northernmost part of the country. The description of their methodology has been expanded in the manuscript. The area in which the weather states were calculated has been added to figures 1 and 2.

5. Since there are already studies that investigated the connections of rainfall patterns to weather states, it would be reasonable for the authors to devote a paragraph or two to discussing the similarities and differences that can be found between the responses of rain and snow events to large-scale weather states.

Following the referee suggestion, a paragraph discussing difference in circulation regimes during summer and winter seasons is added in the revised manuscript. Perhaps the most comprehensive evaluation of monthly circulation types over Sweden is done by Chen (2000) and Busuioc et al. (2001). Chen (2000) has shown that while the large-scale circulation patterns over North East Atlantic favouring westerly and northwesterly winds dominate 35–50% of the time during the summer months (June

through August), the wintertime circulation patterns are characterized by nearly equal dominance of cyclonic and anticyclonic conditions. Similarly, Busuioc et al. (2001) showed that during winter months (especially beginning of year), NAO explained 41% of MSLP variability followed by the dipole structure with centres located over eastern Scandinavia and East Atlantic explained about 24% variability in January and the third mode represented cyclonic/anticyclonic conditions. However, during the summer rainfall months, the cyclonic/anticyclonic conditions dominated circulation types representing about 37% variability.

Specific comments:

Page 2:
Line 1: "Selected" previous studies? Should this say "several" instead?

Done.

Page 5:
Lines 7-8: Did you investigate if there were systematic differences in surface snowfall between 2CSNOW and the ground-based radar?

Yes, a comparison of the surface snowfall from 2CSNOW and Nordrad was investigated and described in the paper by Norin et al. (Atmos. Meas. Tech., 8, 5009–5021, 2015). A short paragraph has been added to further describe the results of that study.

Line 14: I think OCO-2 is currently the A-Train lead satellite.

True. This is now corrected in the manuscript.

*Lines 27-28: This should probably say "more than one standard deviation *above/below*

the mean"*

Done.

Page 6:
Line 22: I think that the average is only for cases where it is snowing, i.e. zero snowfall is not included in the average? Or am I mistaken? In any case, this should be clarified.

Correct. This is now clarified.

Page 7:
Lines 23-25: "However, snowfall intensities are..." This is a confusing sentence, I am not sure what you are trying to say.

The sentence has been rewritten.

Line 25: Temperature anomalies relative to what?

The temperatures anomalies are relative to the average temperature for the investigated years. This is now clarified in the manuscript.

Page 8:
Line 3: See my previous comment: what is the reference for calculating specific humidity anomalies?

The specific humidity anomalies are relative to the average specific humidity for the investigated years. This is now clarified in the manuscript.

Page 9:
Line 16: Comparing the mean lines Figs. 9a and 9b, it is rather hard to see any differ-

ence between them near the surface.

Agreed. The sentence has been rewritten.

Page 10:
Line 10: "Relative strengths of snowfall" — again, relative to what?

Here relative snowfall means that the sum of all four snowfall rates are equal to one. A sentence has been added to clarify this.

Figs. 1 and 2: Since the wind speed is already encoded in the arrows, the using the color to also show that is somewhat redundant. On the other hand, the pressure patterns might be interesting. How about using color to show the pressure instead?

We thank the referee for the suggestion. Although it would likely be an improvement it is unfortunately not possible for us within this project.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-25, 2017.