

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

Anonymous Referee #1

Received and published: 11 March 2017

Review of "The sensitivity of snowfall to weather states over Sweden" by Norin et al.

This paper describes an analysis of the dependence of snowfall in Sweden on large-scale patterns of atmospheric circulation. The effects of prevailing wind direction, high or low pressure as well as positive and negative North Atlantic Oscillation (NAO) are considered.

The analysis methods the authors use appear sound, and the for the most part, the conclusions are consistent with the presented data. The study only concerns Sweden and as such has little applicability for other regions, but it could have an impact on understanding the origin of snowfall events in that particular region, and affect societal readiness for responding to those events. The manuscript is generally clearly written; there are minor grammatical issues here and there, but these have minimal impact on

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the readability and can be fixed in copyediting, so I will not comment further on them here.

I think that the remarks given below should be addressed before the study is published.

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.

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.

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.

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.

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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.

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.

Specific comments:

Page 2:

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

Page 5:

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

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

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

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.

Page 7:

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

Line 25: Temperature anomalies relative to what?

Page 8:

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

Page 9:

Line 16: Comparing the mean lines Figs. 9a and 9b, it is rather hard to see any difference between them near the surface.

Page 10:

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

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?

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

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