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Discussions

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

Anonymous Referee #2

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Review of "The sensitivity of snowfall to weather states over Sweden"

The goals of the paper are well stated, the analysis is appropriate for achieving the goals, and the writing and figures are clear. I do have some questions about how the analysis was performed.

General comments

1. Previous research has identified the 8 states the paper analyzes. While investigating snowfall relative to these states is appropriate, I think the authors should note that these states are not independent from one another. I would like to know how many events comprise each category, and if individual dates can be included in multiple categories.

2. Are there no snowfall observations over Sweden that can be used to further vali-

date the remotely sensed data? At times the authors refer to "snowfall accumulation". As I understand the analysis performed, no statements can be made about snowfall accumulation, as the duration of the snowfall events does not appear to be considered.

3. Figures 1 and 2 – are these composites (means) of all events? Or individual events that are representative of the composites?

4. On the use of CloudSat – As you discuss, data represents a "snapshot" taken once or twice per day. Is snowfall occurring during all of these snapshots? How many snapshots are there per event? I am concerned about how representative these snapshots are for the events analyzed using the other more frequent data sources, as comparisons are made between them.

5. I would like to know more about how the wind directions were defined. The NAO is straightforward and I can see how identifying the presence of a pressure center over land is tractable. But wind direction can be highly variable over the domains shown in the figures.

6. I think it would be useful to the reader to see figures that show the radar coverages referred to in the text (48-82 km from a Swedish radar station), so we can have an idea of how representative the data is with respect to the entire country.

7. Related to the above comment, were the intensities for each event defined by averages over each radar volume? How many times were used in each event?

8. I see that Norin et al. (2015) describes how the snowfall product is generated. Given the fact that your conclusions are heavily dependent upon the quality of the data that results from the methods used to define the product, I think it would be worthwhile to further discuss the limitations of the radar-based data.

9. Section 4.2, line 15. I see what you are referring to in Figure 9a and b, but I am not sure how this resolves the apparent paradox discussed in the previous paragraph. Perhaps you could be more explicit.

Minor comments

1. In line 5, it is stated that the NAO correlates strongly with precipitation, but explains only 32 to 54% of precipitation variability locally (Busuioc et al. 2001). In line 17, Linderson (2001) further observes a robust correspondence between local precipitation and large-scale circulation during winter months over southern Sweden. As written, it seems these results are contradictory. If this is the case, it should be highlighted as motivation for this study. If this isn't the case, perhaps the text could more clearly explain the differences in the studies.

2. Line 22 – Such a characterization also provides guidance concerning the performance of NWP models by quantifying their strengths and limitations in wintertime regimes – could you state more clearly how your work relates to NWP verification?

3. Line 30 – snowfall distribution and frequency is very inhomogeneous meridionally across Sweden. How does it vary?

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