

## ***Interactive comment on “Snow depth on Arctic sea ice from historical in situ data” by Elena V. Shalina and Stein Sandven***

### **Anonymous Referee #3**

Received and published: 1 February 2018

The manuscript presents an analysis on the snow data collected during the Sever expeditions with the objective of producing an improved climatology over the historical climatology by Warren et al., 1999. The analysis provides useful statistics on the sampling scheme from the Sever expedition, and demonstrates relationships between snow depth and morphological features. However, there are critical limitations to the Sever snow data set, as addressed in Warren et al., 1999 (pages 1825-1827). These limitations were considered too biased to incorporate into the historical climatology. The manuscript's main conclusions overlook these limitations and over-interpret how representative the Sever data set is. More descriptions are needed on the assumptions made in the methodology and approach in the statistical analyses in consideration of these limitations. Please find specific comments below that I hope the authors will find

Printer-friendly version

Discussion paper



useful:

- In general, there's a lack of references throughout the manuscript; more references would help bolster the explanations of snow processes and interpretation of the results.

Page 2, Lines 15-30+. There have been numerous campaigns that have sampled snow on sea ice outside of the list presented here.

Page 3, Line 1. Figure 1 in Warren et al., 1999 shows that more than two stations were regularly present in a given year.

Page 3, Line 2. The instrumental errors were likely quite small. What do the authors mean here exactly?

Page 6, Lines 6-7. Snow lines were selected on a flat ice surface is contradictory to the description in Radionov et al., 1997 and Warren et al., 1999.

Page 6, Line 20. Please provide information on the spatial domain in which the 10-20 random snow thickness measurements were made.

Page 6, Lines 23-24. This indicates that the sampling was biased towards level first year sea ice, which is one reason to question how representative the Sever data set is.

Page 6, Lines 27-28. Does the 10 cm threshold for sample size introduce an additional bias to the data set?

Figure 4. What is meant by the prevailing landing area ice? Are these measurements from the runway or surrounding sea ice? Do these data also include sastrugi and ridge measurements?

Page 9, Lines 8-9. How was the 2.0 m threshold chosen, and how sensitive are the results to this threshold?

Page 9, Lines 13-14. It's not clear why only multiyear sea ice observations were included in the analysis here. Please provide more explanation on this decision, and

[Printer-friendly version](#)[Discussion paper](#)

whether it is a valid assumption for creating an historical climatology for the Sever region.

Page 11, Line 3. Which measurements (ridge, sastrugi) are included in the average snow depth?

Page 11, Lines 16-17. This statement needs support (quantitative results) from a statistical analysis.

Page 12, Lines 6-8. It would be helpful to state that this relationship is dependent on the season, spatial domain, and sea ice type.

Figure 8. This figure doesn't show new information from Figure 16 in Warren et al., 1999.

Page 14, Lines 6-7. How much did spatial variation between landing sites affect the difference between the March and May values, rather than the conclusion that it's an increase? Were there equal sample sizes between the months of March, April, and May at the same sites?

Page 15, Lines 12-13. Is this a representative statistic if the landings were biased towards level first year sea ice?

Page 15, Lines 20-22. This is not correct. Multiyear ice has more variable surface relief, which acts to create more variability in snow depth distributions than level first year sea ice. Wind speed is not a valid explanation for the difference considering the observed frequency of blowing snow events.

Page 16, Lines 23-24. This finding is unclear.

Page 17, Line 1. The wind speed needs to be at least 5 m/s in order for snow to drift and redistribute. Wind speed is not a valid explanation for the difference.

Page 17, Lines 12-15. Where did these results come from if fast ice observations were not made during the Sever expeditions?

[Printer-friendly version](#)[Discussion paper](#)

Page 20, Lines 5-9. Why were these adjustments made?

Figure 12. The buoy data need to be quality checked. Buoy 2013F does not show a realistic snowfall event.

---

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-278>, 2017.

TCD

---

Interactive  
comment

Printer-friendly version

Discussion paper

