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

Anonymous Referee #2
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The authors would like to thank the reviewer for his time and valuable comments. The corresponding changes and refinements have been made in the revised paper and are also summarized in our reply below. Authors' responses are in blue. Reviewer's comments are in black. When our manuscript is cited, it is shown in italics.

This paper incorporates a wealth of data measured from the Russian Sever expeditions to improve our historical knowledge of snow depth on sea ice, in particular in the marginal seas. The data used represent a massive effort spanning several decades and I am happy to see such a study done. The paper is thorough and generally well written, though I do have a few points I would like to see addressed.
An updated climatology to that produced by Warren et al., 1999 is one of the main results of the paper. However, in the Warren paper the Sever data were examined but not used for these reasons quoted in the paper:
"It is puzzling that the snow should be so much deeper around hummocks (45 cm) than behind ridges. The geographical patterns are also puzzling. Because some of the variation in average snow depth across the Arctic seen in Fig. 9 is probably due to different areal coverages of sastrugi, ridges, and hummocks, one would expect the geographical gradients of snow within these classifications to be smaller than those of Fig. 9. However, this is not the case. The snow depth behind ridges appears to decrease toward Canada, while the height of snow around hummocks increases. These strange patterns cause us to question the representativeness of the measurements made at the aircraft landing sites. We favor the measurements made at the NP stations that were conducted more systematically."
I believe these points need to be addressed directly by the authors prior to publication.
In addition to the points raised in the Warren paper, I would like to see a better explanation for why the new climatology was produced using only the sastrugi and landing snow depth. The Warren climatology used data from snow lines which contains a mixture of snow depth from level ice as well as deformed ice, I'm not sure the snow depths produced from the Sever data would be equivalent. Perhaps a statistical analysis could be done to better relate data from the snow lines to that sampled in the Sever data.

In the Warren et al., 1999 (W99) paper the authors mention only data from Sever expeditions collected in April. However there were also quite sufficient number of measurements collected in March and May. In our paper we have processed observations collected in March, April and May (the MAM months) that indicates enlarging the amount of processed data in our case. It is difficult to say how larger the amount of data in our case is since in W99 there is no information about the number of processed observations. The Sever data set from NSIDC (that we use) contains snow depth measurements distributed over the MAM months in the following proportion: 46% of all measurements were done in April, 27% in March and 27% in May. Thus, March and May observations make a substantial addition to data collected in April. It is stated in W99 that geographical sampling of landings was very uneven, however the distribution of sampling sites used in the analysis is not shown and it is not possible to compare it with what was available for the analysis in our case. Contrarily to W99, we openly show the distribution of sampling sites and the number of processed observations. The suspect that we and W99 have processed different data sets is confirmed by comparing W99's estimates of the average snow depths behind pressure ridges and near hummocks. W99 indicates the "puzzling" (page 1826) fact that the estimates of the mentioned parameters are very different. In our case the parameters are comparable (see Table 4 and Fig. 9 of the paper) and their estimations are different from W99's. Decreasing snow

depth behind ridges towards Canada mentioned by W99 (page 1826) is obviously caused by the deficiency of data in Sever dataset collected in the region north of the Canadian Archipelago.
NP measurements were conducted on the MY ice and on the same floe throughout the life of the NP station. Snow line measurements catch very well natural variability of snow conditions on the spot, however, taking into account limited number of NP expeditions and unevenness of the distribution of their measurements in the Arctic, one may choose to find additional sources of data. Furthermore, one could also find strange geographical patterns in the W99 climatology: for example, according to W99 the snow depth in the area to the east of Greenland changed from 40 cm in March and April to 34 cm in May and in June it increased up to about 46 cm.
As to comparability of NP data and Sever data used in new climatology, there is no information about ice conditions, corresponded to NP snow line measurements, that complicates building a valid imitation. Ice conditions were not observed and did not described, so we do not know how much deformed ice affected the average snow depth measured along the lines. We read in W99: " The deep snow at about one-third of the way along the line in March **is probably** in a snow dune or in a drift near a pressure ridge" (Page 1817), which shows that there were no certainty about causes of snow depth variations among the authors. We built the new climatology basing on the snow depth measured in the vicinity of landing site + the height of sastrugi, weighted in proportion to the sastrugi area. The height of snow attached to ice ridges was not included into calculations because we do not have estimations of the area covered by such features. The effect of not including that data in the computation results in some underestimation of the average snow depth.
In the paper:
The height of snow attached to ice ridges was not included in the calculations because 1) the Sever measurements in the Western part of the Arctic Ocean are too scarce, and 2) there are no estimations of the area covered by such features from the Sever expeditions. The effect of not including that data results in some underestimation of the average snow depth. The SHEBA observations indicated that in April and May 1998 about 3.9% of the examined area was covered by deep snow (>80cm) associated with ice ridges (Sturm et al., 2002). The underestimation is most important in the western Arctic, especially north of the Canadian Archipelago, where the highest concentration of the ridged ice is expected (Bourke and McLaren, 1992, Makshtas et al., 2003, Shoutilin et al., 2005).

Specific comments

P1 L23-24: In comparing to the Warren climatology it is necessary to state what is being compared: level ice snow or does it mix in snow from deformed ice too?

The referee refers to the text in the Abstract. We updated that text:
The main result of the study is a new snow depth climatology for the late winter using data from both the Sever expeditions and the North Pole drifting stations. The Sever snow depth measurements related to undisturbed snow and sastrugi are used in the computation. The height of snow accumulated near the ice ridges was not included in the calculations because those Sever measurements are unevenly distributed in the Arctic and are too scarce in some regions, besides, there are no estimations of the area covered by such features from the Sever expeditions. The effect of not including that data results in some underestimation of the average snow depth.
All details are discussed in the paper.

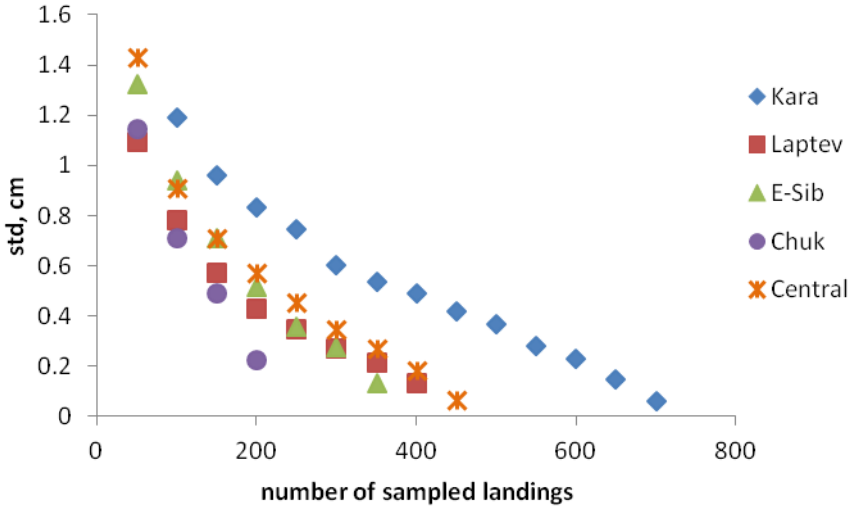
P3 L1-2: The Warren climatology gives a representation of the mean error in the form of the interannual variability, so I don't think this statement is correct here. The climatology did not have adequate sampling to provide information on the errors due to spatial variability, so I suggest this statement be revised to reflect this aspect.

The sentence "*Due to the high spatial and temporal variability of the snow depth it is difficult to estimate the errors of the mean values of the W99 climatology*. " is removed.

P6 L20-34: In the W99 paper there is significant discussion about the representativeness of the sampling. While this section describes the sampling method, this important point has not been addressed. I note particular the analysis done with random samplings

of the same population to see how the error for a given set of measurements changes with sample size and snow depth.

We have done the analysis of sampling statistics. For every sea and for the central Arctic random samplings of all available observations have been generated. The subsets contained 50, 100, 150, 200, and more (where possible) observations with the increment 50. 100 subsets were generated for every number of subset population in every region. Average snow depth was calculated for every subset population. Variability between averages was estimated by standard deviation.



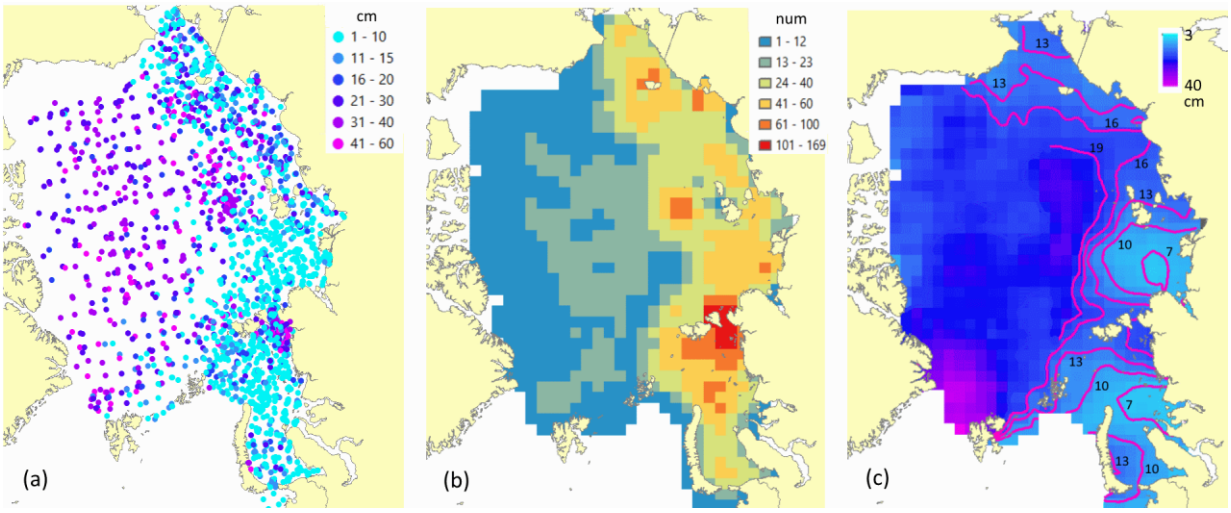
Additional Figure. Standard deviation of the average of a set of snow depth measurements as a function of the number of landings used in the average computation. Measurements were randomly selected from the whole set of data for every region. The amount of randomly generated subsets was 100 for every number of sampled landings.

P7 L15-16: This statement seems like it belongs more in the caption for Figure 4.

This text describes what is shown in Fig.4. It is not the caption, just the text of the paper. The format that we have to follow does not differentiate between the text of the figure caption and the text of the paper.

Figure 5: It would be easier to read if the figure panel with snow depth had the units labeled.

Please see the updated figure. We have tried to take into account all comments regarding it.



P9 L19: I don't understand the last sentence, particularly with regard to the word "implying". Were all snow measurements used or was the MY subset used?

All measurements collected in the marginal seas were used:
"*By using the same ice thickness threshold in the marginal seas, the fraction of data coming from MY ice was 9% in the Kara Sea, 11% in the Laptev Sea, 34% in the East-Siberian Sea and 23% in the Chukchi Sea. These fractions of MY ice seem reliable, thus all snow observations in the marginal seas were used in the subsequent analysis*" (rewritten)

P10 and throughout: "fastice" should be "fast ice"
Done.

P13 L4-8: I'm confused by this section as the regression equation implies the sastrugi height is simply a constant 15.5 cm higher than the undisturbed snow.
This result is a surprise for us too. Please keep in mind that it's an average over the whole dataset.

P20 L5-8: Why were only the snow on the landing area and sastrugi data used and not any of the others described?
Snow attached to ice ridges and hummocks is not included into calculation because there is no data describing their density. The updated text is shown here:
In order to produce an integrated map that describe the average state of snow cover in the MAM months, the Sever and NP measurements have been combined by gridding all Sever data on snow depth on the level ice together with NP data. The height of sastrugi weighted in proportion to the sastrugi area was added for each Sever grid cell snow depth using the formula: $H_{sev} = H_s + P_{sas}^{reg} \cdot H_{sas}$, *where* H_s *is the depth of snow measured on the level ice (described by data providers as the snow depth on the prevailing ice in the landing area),* H_{sas} *is the height of sastrugi, and* P_{sas}^{reg} *is the average portion of the ice surface covered by sastrugi in the region, to which snow measurement belongs (see Table 3). In the central Arctic, where the ice was mainly perennial at the time of measurements, only observations made on the MY ice during Sever expeditions have been included as was explained in Sect. 3. The height of snow attached to ice ridges was not included in the calculations because 1) the Sever measurements in the Western part of the Arctic Ocean are too scarce, and 2) there are no estimations of the area covered by such features from the Sever expeditions. The effect of not including that data results in some underestimation of the average snow depth. The SHEBA observations indicated that in April and May 1998 about 3.9% of the examined area was covered by deep snow (>80cm) associated with ice ridges (Sturm et al., 2002). The underestimation is most important in the western Arctic, especially north of the Canadian Archipelago, where the highest concentration of the ridged ice is expected (Bourke and McLaren, 1992, Makshtas et al., 2003, Shoutilin et al., 2005).*

P20 L15-18: Although the detail of the data is lost in the quadratic fit, an advantage of the W99 climatology is that the fit coefficients were provided such that others could easily reproduce the climatology values. I suggest the authors put the fit coefficients in here.
The coefficients are calculated:

Coeff	value
H_0	35.05
1	-4.96E-06
2	-1.46E-06
3	-2.27E-12
4	2.91E-12
5	-1.16E-12

1-5: coefficients in the regression equation. Snow depth: $H_s = H_0 + C_1*x + C_2*y + C_3*x^2 + C_4*xy + C_5*y^2$.
Coordinates are in meters. Projection is North Pole Stereographic, datum WGS_1984, latitude of origin = 90.0, central meridian = 20.0.
The text in the updated paper:

The two-dimensional quadratic fit has been calculated as

$H_s = H_0 + C_1 \cdot x + C_2 \cdot y + C_3 \cdot x^2 + C_4 \cdot xy + C_5 \cdot y^2$,
where $H_0 = 35.05$ cm, $C_1 = -4.69 \cdot 10^{-6}$, $C_2 = -1.46 \cdot 10^{-6}$, $C_3 = -2.27 \cdot 10^{-12}$, $C_4 = 2.91 \cdot 10^{-12}$, $C_5 = -1.16 \cdot 10^{-12}$, and x and y are coordinates in the North-Pole Stereographic projection. Units of x and y are meters.

P25 L34-35: Petty et al., 2016 (The Cryosphere) found FY feature heights of around 1
m which might be a more thorough comparison to the Sever data.
In Petty et al., 2016, the authors derive information regarding the heights of the sea ice topographic features using the elevation threshold of 20 cm. Though the snow elevations including sastrugi have to be captured by methodology, it is difficult to identify them and separate from the ice features.

A number of minor grammar errors are present throughout the text.

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