The authors would like to the referee for the invaluable comments. The following revisions and corresponding replies are made for each comment (in *green italic* font). Also, a revised version of the manuscript is provided as attachment. The replies to comments are as follows, and the revisions are highlighted in the manuscript in yellow.

Reply to comments of Referee #1:

The data used for the study are well presented. But the diagnosis performed are sometimes not enough explained or do not cleary serve the objective. Moreover, it is not fully clear to me which scientific objective is pursued with this analysis. Perhaps two many numbers dilute the aim...

Reply to the general comment: the authors thank the reviewer for pointing out the shortcomings of manuscript in conveying the main idea and results of the study. We would like to emphasize that the purpose of the study is to learn about the **variability** of the measured/retrieved sea ice thickness parameters by current airborne and satellite remote sensing. Thickness parameters include: **radar and laser freeboard**, and **snow depth**, and there are 3 topics for study: **variability, the scaling of variability, and co-variability** (between snow and freeboard). The analysis of variability may be greatly affected by measurement/data-processing errors, therefore, we pay attention especially how these uncertainties may affect the estimation of the true, inherent physical variability of these parameters. Therefore, the main purpose is two-fold: (1) on the observational technique side, we want to study the variability and scaling by different approaches; and (2) on the scientific side, the inherent physical behavior of variability scaling and implications of the processes. Inevitably, these two issues are entangled in many cases, including this study.

Specific comments

[page#3, row#25-27] "On the other hand, by adopting the same the geophysical corrections of CS-2, Yi et al. (2018) effectively aligns the retrieved freeboard across CS-2 products and greatly reduces the systematic differences." Could you clarify?

Reply: in order to clarify, we revise this sentence as: "*On the other hand, with the same geophysical correction of CS-2 and snow depth correction based on OIB SnowRadar, the mean freeboards from four CS-2 retrackers are all in agreement with ATM by 0.05 m (Yi et al. 2018)*".

[page#4, row#22] "sea-surface height correction" "correction" is not relevant here, I would simply write "sea surface height"

Reply: corrected.

[page#4, row#24] "in the freeboard estimation of the sea floes" I would remove "of the sea floes"

Reply: corrected by removing "*of the sea floes*".

[page#4, row#25] "the freeboard uncertainty that is associated with SSH correction" SSH is not a correction. I would say SSH estimation or SSH retrieval

Reply: corrected to "… associated with SSH estimations".

[page#6, row#33-34] "Since SSH height information are shared among freeboard data, we treat this uncertainty as bias and ignore it in the scaling analysis" I don't fully agree. Sea

level anomaly (SLA) interpolation between the leads includes mean sea surface (MSS) error which is not necessary a bias. At the scale you are focus on, the explanation you give on page#8 row#5-7 seems more appropriate.
Reply: the authors have revised this sentence to be more accurate, as follows: *Since along-track SSH's are usually constructed with observations on much larger spatial scales (over 100 km), their uncertainty is not considered in the variability scaling which involves local averaging within several kilometers*.

[page#7, row#9&18&32] "SSH correction" Not appropriate. Do you mean geophysical corrections (troposphere, ionosphere, tide)? Or Sea level estimation?
Reply: "*SSH correction*" is revised to "*mean SSH estimation and local sea-level correction*", which is a more precise statement.

[page#7, row#15] equation (4) I prefer the (equivalent) formulation by Kurtz 2014 F_i=F_r+h_s (1-c_s/c)
Reply: the equation (no. 4) is revised as indicated. The approximation to the coefficient for the correction (1-c_s/c) is taken as 0.25 according to Tilling et al. (2018, ASR). If it is computed according to Kurtz et al., (2014, TC) under the snow density assumption of 330kg/m^3, this coefficient is about 0.22. With either estimation, the major result of F_r (radar freeboard) is not affected.

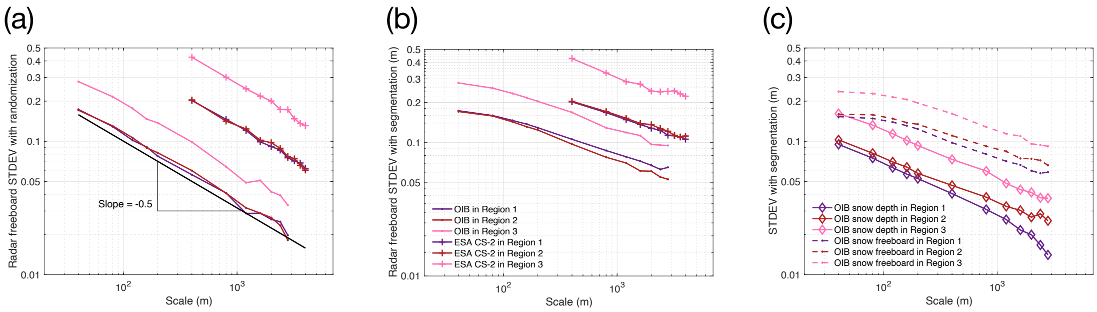
[page#11, row#14] "Since with random samples, the effects of [. . .] inhomogeneity are very limited" Could you explain?
Reply: since there exists: (1) autocorrelation of nearby samples and (2) inhomogeneity of the sea ice cover (within 37.5km by 37.5 km) that is sampled, samples randomly chosen will be physically away from each other, which will attenuate the effect of BOTH local correlation AND inhomogeneity of the region where the samples are collected. With random sampling strategy, the variability is expected to decrease with respect to sample count for averaging (STDEV decreasing with the square root of M). This is indeed observed in Fig. 3a. Since it does not provide any further insight into the scaling of the parameters, this strategy is only provided as a baseline for reference.

*[page#12, fig#3] Could you explain how each point of the curve has been computed? Does the size of the considered area change for each point? (fig3a) On fig.3b do you change M for each point, leading to a scale = M*resolution? I don't understand why STDEV is larger when averaging (3b vs 3a).*
Reply: for the 3 subfigures in Fig. 3, each color corresponds a specific local region in Fig. 2(b-d), which are local regions that contain good OIB coverage. Each curve in Fig. 3 is produced with averaging several OIB samples, and the referee is correct that the scale is computed as linear to the sample count. For example, 800 m corresponds to 20 OIB samples, which involves averaging of 20 random (or local) samples for subfigure a (or b and c). The different behavior of variability decrease (slower decrease in b than a) is mainly due to two factors. First, the local positive correlation of the parameter (Hs or Fr or Fs) causes the variability to decrease slower. Second, the sea ice cover within the region of study is inherently inhomogeneous, so local averaging will not attenuate the variability that is present on the spatial scale that is larger than the sample footprint. The larger variability with local averaging (b) than random sampling (a) is actually fully expected. As mentioned in Sec. 3.1, when we use the randomized sampling, the STDEV decreases with the square root of sample count M, which follows the theory for independent samples quite well. On the contrary, local averaging is the typical manner of scaling analysis, which is more informative of the physical

variability of the sea ice cover. And indeed, it shows slower variability decrease with larger scale (subfigure b). This figure is further revised to align the range in y-axis (STDEV) for clearer viewing.

[page#12, row#5] "SSH correction" Not appropriate. Do you mean geophysical corrections (troposphere, ionosphere, tide) ? Or Sea level estimation ?
Reply: "SSH correction" is revised to "SSH estimation", which is the correct term as used in Ricker et al., (2014).

[page#12, row#11-12] It is not so easy to compare fig#3b and #3c as the y-scale are different.
Reply: the updated figure (also shown below) contains aligned scale for the y-axis.



[page#12, row#15-16] "However, Fs is controlled by both sea ice thickness and snow distribution, and it shows comparable variability as Fr at larger scales." But Fr(OIB) is linked to hs as it is estimated using Fs and hs.
Reply: the authors agree with the referee on the comment, therefore, the sentence is revised as follows: "*Fs, similar to Fr, is controlled by both sea ice thickness and snow depth, and it also shows comparable variability as Fr at larger scales*."

[page#13, row#6] "Each point represents a local region" Does it mean that 1 point corresponds to the STDEV over 37.5x37.5km^2?
Reply: the referee is correct that each point corresponds to variability (STDEV) over 37.5x37.5km^2.

[page#13, row#10] "As is shown, there exists statistically significant correlation between OIB and CS-2" It seems not so significant to me. . . and it is even worse at 400m.
Reply: the authors have revised the figures to include information of correlation and statistical fittings: r for the correlation coefficient (specifically, Pearson's product moment correlation coefficient adopted here), the linear fitting relationship, and p-value for the statistical significance of the correlation. All the fitting lines contain significant correlation between the freeboard variances at 0.01 level (i.e., $p < 0.01$). See also below for further info.

[page#13, row#11&15&30] $p < 0.01$ Could you explain what is p? What does it mean?
Reply: p is the probability value (*p-value*). In statistical hypothesis testing, it is frequently adopted for the testing the null hypothesis that there exists no relationship between the observed phenomena. If p-value is smaller than a certain significance level (of which 0.05 is adopted by many practices), then the corresponding correlation (indicated by r) is considered significant. A p-value lower than 0.01 (which holds for all the fittings in the figure) indicates that the positive correlation is highly significant.

[page#13, row#13] "more strict waveform filtering in AWI's protocol as compared with ESA" Could you explain? Is it linked to waveform classification or editing?

Reply: the relevant differences between ESA (Baseline-C) and AWI are formally listed as below: (1) ESA uses a 70% threshold for the amplitude of the first peak for floe echoes surface tracking point while AWI use a 50% threshold at the first maximum of radar-echo-power-based method to determine surface elevation for leads and floes; (2) in AWI's protocol, radar freeboards (on the per-waveform level) that are too large or small are not use for further processing, while in ESA's product, there is no such filtering (Bouffard et al., ASR, 2018). Therefore, this sentence is rewritten as: "*more strict waveform filtering in AWI's protocol than ESA's, in order to eliminate outliers according to radar freeboard values*".

[page#13, row#29-30] "However, there still exists statistically significant ($p < 0.01$) correlation between VAR of CS-2 and that of OIB/CryoVEx." I am not convinced that the correlation is significant. . .

Reply: the statistical information (r and corresponding p-value) is added in Fig. 3c. After re-checking, we do confirm that indeed the positive correlation is significant at 0.01 level ($p=0.001$). In order to further support our argument, we have included in Fig. 3 the analysis with AWI CS-2 Fr product, along-side ESA Fr (baseline-C). As shown in Fig. 3d/e/f (which compare against Fig. 3a/b/c), all the fittings show significant correlation at 0.01 level between AWI's product and OIB.

[page#13, row#32-34] "For a given location, if the sea ice cover with larger (smaller) variability of Fr on the small spatial scale, CS-2 also consistently produces Fr samples that indicate higher (lower) variability." I don't understand this sentence; could you clarify?

Reply: The sentence contained a grammatical error, and also for the sake of clarity, it is revised as: "*For a given location, if the sea ice cover shows larger (smaller) Fr variance on the small scale, CS-2 also consistently produces Fr samples that contain larger (smaller) variance*".

[page#14, fig#4] What does mean $p<0.01$? On fig#3b it seems that OIB variance is almost killed when Fr is averaged over 10 points. It seems not inline with fig#3b.

Reply: the explanation for p-value (which is an indicator for the significance level of the correlation) is added as reply above. Fig. 4 is also updated with related statistical information.

[page#14, row#1-2] "By using ESA CS-2 Fr product and following Fig. 4.a, we deduce the variability at OIB scale of 40 m (STDEV40m) using CS-2 samples (STDEVcs2)" Could you explain the scientific interest to do so?

Reply: the purpose of deducing Fr variability at small scale (i.e., OIB) is that this info is NOT generally available across the Arctic basin. This is mainly due to the spatially and temporally limited coverage of OIB. Given the established relationship with collocating OIB and CS-2 data, we can use this statistical relationship to attain a basin-scale Fr variability estimation with CS-2. This info can be further applied in many studies such as the thickness retrieval with CS-2 (Xu et al., Rem. Sens., 2018).

[page#15, row#9-10] "First, after eliminating the effect of random error of ICESat ($\sigma = 5$cm) from its sample variance" Could you explain how do you proceed?

Reply: in specific, the random error variance ($\sigma^2 = 25\text{cm}^2$) is subtracted from the sample variance, which is what we mean by "eliminating". The square root of the resulting value (as standard deviation) is then used to construct the PDF in Fig. 5c.

[page#15, row#31-32] "Both smaller footprints and wider coverage (through more heterogeneity) could induce larger variability in Fs" It not so clear to me how you can conclude this. . .

Reply: the authors apologize for the ambiguous statement. The revisions include: (1) a new paragraph (page 16, line 1 to 8) to simulate OIB with ICESat interval of 175m, accompanied by added sub-figures to Fig. 5 (on page 17); (2) the revised sentence to the following: "*Two factors affects the variability in Fs. First, with the increase in the aggregate footprint size, the variability decreases. Second, if the spatial coverage of samples increase while keeping the total footprint size constant, there is even more effective dampening in variability. This indicates that portion of variability on the local scale increases with wider coverage of local samplings*."

[page#16, row#1] "covariability" (also on pages#16-18) Do you mean covariance?

Reply: the referee is correct that by "covariability" we mean the relation of co-varying between snow depth and freeboard, which is estimated through sample covariance.

[page#17, row#8] "that the thicker snow cover induce higher total freeboard" But over MYI, there could be ridges impacting Fs. . . I would have expect more correlation between low hs and low Fs over FYI.

Reply: the authors agree with the referee that over the small spatial scale, sea ice ridges greatly impact Fs. Furthermore, we would like to acknowledge two facts. First, snow distribution and ice freeboard (or topographic features) may be dominated by difference processes and hence feature independent variability. Besides the negative covariance between the two, both Fi and Hs have large part of variability that are not included in (or explained by) this negative covariability. Second, as pointed out by the referee, ice features such as ridges might pose extra problem to our analysis. For example, it is shown that on highly deformed ridges, OIB's snow radar may not be able to produce trustworthy retrieval of snow depth due to undetectable snow-ice and air-snow interfaces. In the manuscript we do note that specific versions of the OIB products (especially for snow depth) may quantitatively alter the results in this study. Therefore for revisions, we have added extra discussion in OIB part in Sec. 4, especially on this issue.

[page#17, row#15] "Type-I error" Could you explain what does it mean?

Reply: Type-I error is often referred to as *false positive*, which in this context corresponds to that the null hypothesis (that no negative covariability exists between Fi and Hs) can be more possibly falsely rejected, if the random error in Hs is not accounted for during the estimation of covariance.

[page#17, row#21-22] "This result indicates that at small scale, there is complementary relationship between snow depth and ice freeboard." Could you explain what you mean? I would say they are uncorrelated (opposite variations).

Reply: the authors would like to clarify that by "complementary" we actually mean the relationship of negative covariance between Fi and Hs: when Hs is higher (thicker snow), Fi is lower; when Hs is lower, Fi tends to be higher. This relationship is also reflected in various field studies. For example, in Sturm (2002) and Sturm et al. (2002), it is shown that small-scale interaction between the snow and ice produces thicker snow above pond ice (with lower Fi) than nearby hummocks (see Fig. 13 of the reference). As reported in Sec. 3.4 for 40m scale, the negative covariance between Fi and Hs is significant for 97% MYI regions and 72% FYI regions (0.05 significance level).

[page#18, row#13] "complementary effect" Could you explain what do you mean? masking effect?

Reply: the authors would like to clarify that by "complementary effect", we mean that the ice topography is attenuated by the snow's distribution, which masks out the overall ice topography and reducing the variability in freeboard.

[page#18, fig#6] Even if the covariance is positive, it is still around zero; so I don't see any clear correlation between hs and Fs on this figure.

Reply: the authors would like to point out that the PDF in Fig. 6 is based on sample covariance, which is in the unit of m^2. Therefore, the absolute value seemingly near 0 does not indicate that the correlation coefficient is low or the correlation is not significant. Actually, as indicated in Sec. 3.4, over 95% local regions show statistically significant positive correlation ($p<0.05$) at 40m scale, with over 90% at 800m. Since many works have shown specific examples of relationship between Fs and Hs (such as Kwok et al. 2011, Zhou et al., 2018), we do not show any example here.

[page#21, row#7] "This covariability is also reported by other works, including Kwok et al. (2011)" I think that the graphs used in this paper from Kwok are more relevant and easier to understand.

Reply: the authors agree with the referee that a specific example is more indicative of this positive correlation.

[page#21, row#31] "snow cover tends to complement sea ice topography" Do you mean mask?

Reply: the authors acknowledge that the referee is correct that by "complement" we mean that the snow cover tends to attenuate the sea ice topography, and snow distribution that masks out ice topographic features might be a dominant factor.