Author Responses (ARs) to each reviewer comment are in Bold.

Interactive comment on “Estimating snow depth over Arctic sea ice from calibrated dual-frequency radar freeboards” by Isobel Lawrence et al.

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Review for "Estimating snow depth over arctic sea ice from calibrated dual-frequency radar freeboards" by Lawrence et al.

General comments:

The study "Estimating snow depth over arctic sea ice from calibrated dual-frequency radar freeboards " by Lawrence et al. uses a combination of altimeters operating at different frequencies (Ku and Ka bands) and flying over the Arctic at the same period (2013-2016) in order to estimate snow depth at the top of sea ice.

Based on previous studies, the authors consider that the main return of the Ka-band radar signal arises from an upper part of the snowpack and that the main return of the Ku-band radar signal originates from a lower part of the snow pack. Using this difference of penetration depth into the snowpack, they estimate snow depth at the top of sea ice by calculating the difference of freeboard height between SARAL/AltiKa (Ka-band) and CryoSat-2 (Ku-band). Before processing the freeboard difference, the authors correct freeboard biases related to radar penetration/surface state. To correct these biases, they fit their Ka and Ku freeboard measurements using laser and radar measurements from the Operation Ice Bridge (OIB) 2013-2016 campaigns. To validate their "Dual-altimeter Snow Depth" estimates, the authors use "independent" snow depth measurements from the Operation Ice Bridge airborne campaigns. Further, they show that the methodology derived with CryoSat-2 and AltiKa can be reproduced using Envisat (Ku-band) and ICESat (Laser).

The paper focuses on a very relevant topic as snow depth is one of the most important sources of uncertainties when converting ice freeboard to ice thickness. Hence, measuring snow depth at pan-arctic scale with a good temporal resolution could strongly help to improve current sea ice thickness estimates. In addition, snow depth is a key thermodynamics parameter as it isolates sea ice from the cold atmosphere in winter and reflects an important amount of solar radiations in summer. Being able to measure snow depth at large scale could therefore truly help to improve our understanding of sea ice growth and melt processes.

In my opinion, the approach of using a combination of altimeter measurements to estimate snow depth deserves publication. However, I have some major remarks that must be addressed before the paper can be published:

1) While the authors considers that the Ka and Ku radar signals do not penetrate identically into the snowpack, it is not clearly stated where the main returns arise from. The authors quote Armitage and Ridout (2015) and Guerreiro et al. (2016), which draw different conclusions, but they don’t clearly give their thoughts. This precision is crucial as one needs to know if the freeboard fit they perform with OIB is used to correct footprint effects or/and penetration effects.

***Author Response (AR)*: The idea of correcting for both biases due to footprint effects / surface state and physical penetration at once is adopted in order to avoid quantifying the actual penetration of each satellite. Based on radiative transfer theory we assume in general that Ku will penetrate further into the snowpack than Ka but we make no assumptions about how far either is penetrating. The final section of the introduction (lines 3-15 page 4) has been re-written to clarify this.**

The authors say “Freeboard estimates from CryoSat-2 (Ku-band) and AltiKa (Ka-band) are calibrated against data from NASA’s Operation IceBridge (OIB) to align AltiKa to the snow surface and CryoSat-2 to the ice snow interface*”.* Considering this sentence, I assume that they consider (as in Armitage and Ridout (2015)) that Ku does not fully penetrate the snowpack while Ka does penetrate it a little bit, right?

***AR*: We assume that Ku penetrates further into the snow pack than Ka, and therefore choose to correct CryoSat-2 to the ice/snow interface and Ka to the snow surface. We make no assumptions about how far into the snow each is penetrating since we do not think the effects of snow penetration can be separated from biases due to footprint size and surface effects.**

If yes, this raises an important question that should be answered more clearly: why the penetration of the Ka and Ku-bands would change from one area to another (Figure 1 and 2 show that the corrections are not constant)?

***AR*: Figures 1 and 2 do not demonstrate that the snow penetration varies but rather that the combined effects of snow penetration and footprint/surface biases vary from one area to another. Again, the idea with this methodology is that nowhere do we separate the influence of the two. Ideas for why the combined effects of snow penetration and footprint/surface biases (Δf) varies from one area to another are given in the analysis of figures 1 and 2 (lines 18-25 page 8 and line 32 page 8 onwards).**

Also, this assumption seems to not take into account the results shown in Kurtz et al. (2014) and Guerreiro et al. (2017), why?

***AR*: Kurtz et al. (2014) and Guerreiro et al. (2017) demonstrate the importance for elevation retrieval of surface properties and footprint size respectively. Due to these findings, we do not attribute the deviation of retrieved freeboards from snow and ice freeboard respectively as being due to snow penetration differences but a combination of penetration differences and biases due to sampling area. A reference to the findings of Kurtz et al. (2014), not included in the original manuscript, is now included in section 2.3 (lines 16 – 19 page 6), while references to the findings of Guerreiro et al. (2017) are given in lines 27-31 page 3, lines 6-9 page 4, and lines 12-14 page 5.**

2) The "error calculation section" (4.2) deserves some improvements. First of all, the authors calculate the uncertainty from an error propagation using a quadratic formula with variables that are clearly not independent. The variables covariance should be taken into account to avoid this issue.

***AR*: We agree that covariance is required and have updated the formula and discussion in this section to account for this.**

3) Also, they consider that AltiKa and CryoSat-2 have a similar standard deviation on sea surface estimate and they come to the conclusion that, since AltiKa coverage is better than CryoSat-2, AltiKa freeboard error is smaller than that of CryoSat-2. In my opinion, this cannot be true even with the better coverage of AltiKa in the studied region. To derive appropriate

errors, the authors should calculate the standard deviation on sea surface for each satellite mission before injecting it in equation (6).

***AR*: Following your suggestion, we now calculate the error on the sea-level interpolation for AltiKa and CryoSat-2 independently. The methodology for this is outlined in section 3.2.**

Finally, I am not sure what the authors mean by “correlation coefficient” in section 5.1. According to the values in Table 2, I am guessing that they calculate the fit regression line slope. I think it would be preferable to provide a Pearson coefficient R, which is a more common parameter.

***AR*: We have replaced the correlation coefficient with the Pearson coefficient as suggested.**

4) I acknowledge that contemporary large-scale snow depth measurements are extremely rare and that using the same dataset for calibration and validation is one of the only existing options. Having said that, I would suggest to modify the plan of section 5.1 by not considering the year 2016 as a particular one (Figure 6). At the end of the day, Figure 6 (2016) and Figure 7 (2013-2014-2015) are almost identical: you remove observation from the considered year to evaluate your DuST snow depth. Thus, it does not require two figures nor two comments/conclusions.

***AR*: We have combined evaluations for each year into a single figure and conclusion section as suggested.**

Minor comments:

Page 1 L 4-6: "Freeboard estimates . . . ice/snow interface". Does it mean that Ka/Ku don’t stop at the air-snow/snow ice interface?

***AR*: We make no assumption about where Ka/Ku penetrate to, only that Ku penetrates further than Ka and therefore we ‘raise’ Ka to the snow surface and ‘push’ Ku to the ice/snow interface. We feel this is now adequately explained in the introduction and does not require a clarification in the abstract.**

L 23: As you mention Envisat above, you should also quote Giles et al. (2008).

***AR*: We have included Giles et al. (2008)**

Page 2: L15-18: This is arguable. For LRM altimeters, the uncertainty related to free-board height is at least as large as the one related to snow depth.

AR: We no longer reference the results of Giles et al. (2007) in this section and now state that “For both the radar and laser case, snow depth is one of the dominant sources of sea ice thickness uncertainty”. Please refer to page 1, lines 21 onwards.

Page 3: L 31: To be more precise, Guerreiro et al. (2017) suggest that the Ka-band signal stops within the first few centimetres and that the Ku-band signal can stop before the snow-ice interface in case of large snow grains.

***AR*: We have added this clarification (lines 9 to 18, page 3)**

L33: This is not exact: The first study that showed AltiKa freeboard measurements was the one by Maheshwari et al. (2015).

***AR*: We have removed this claim.**

Page 4:

L13: Here and elsewhere, can you mention which footprint you talk about (beam-limited or pulse-limited).

***AR*: We have specified which footprint we are referring to in the manuscript.**

L15: So here, you choose to follow the conclusion given in Armitage and Ridout (2015), which is that the Ka and Ku signals stop within the snow pack, right? If yes, could you state it more clearly? Also, considering the literature you quoted (or not) (Kurtz et al., 2014; Maheshwari et al., 2015; Guerreiro et al., 2016; Schwegman et al., 2015), could you please explain this choice. This is indeed a major point as your entire study is based on this assumption.

***AR*: In response to your major criticism number (1), we feel we have now addressed this point. Lines 3 onwards, page 4, have been re-written accordingly.**

L29: Not exact: see my previous comments.

***AR*: We have removed this claim.**

Page 6:

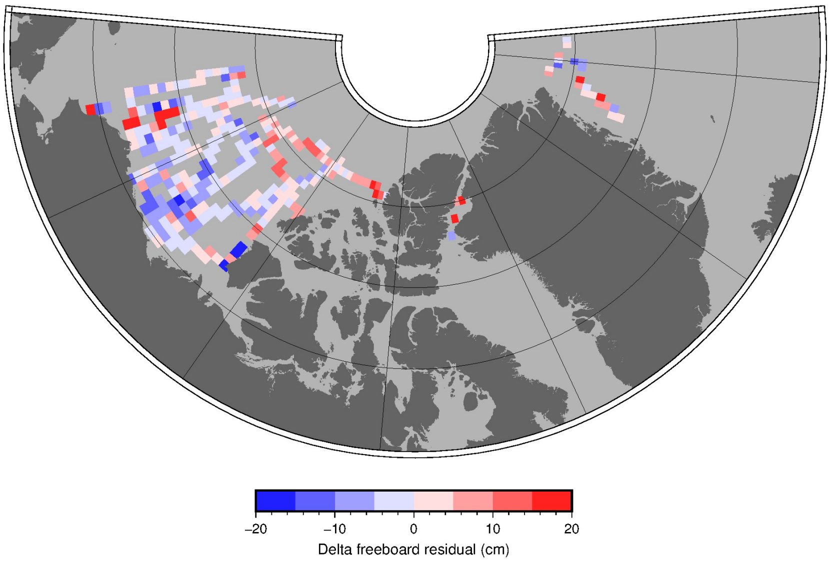
L3: In Armitage and Ridout (2015), I believe that the authors follow another condition related to the Leading Edge Width (see supplementary material). Could you check on that please?

***AR*: In the AltiKa processing, the Leading Edge Width (LEW) is a criterion for identifying 'valid' waveforms but it applies equally to leads and floes: both must have a LEW less than 2 range bins else they are discarded (Armitage and Ridout, 2015, supplementary). LEW therefore is not used to discriminate leads from floes, which is the focus of our discussion in this section. Having said this, the backscatter coefficient Sigma0 is used to identify leads for AltiKa, and for CS-2 the Stack Standard Deviation (SSD) is used to differentiate leads from floes. Details of this have been added (page 5 lines 21 - 23)**

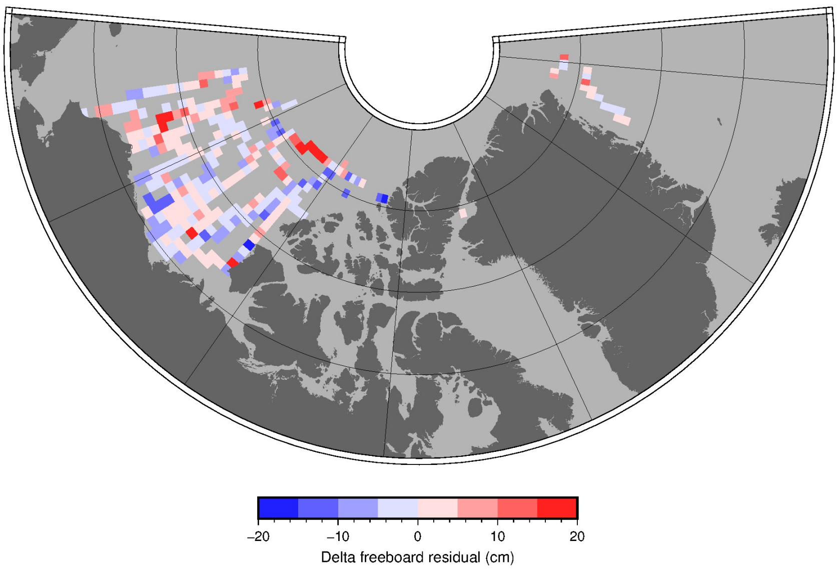
L26-31: To me, this way to proceed raises an important question: As you mention it above, the altimeter range can be biased by waveform hooking due to the proximity of specular reflections. Thus, if you calibrate your freeboards in a particular region (the one overflown by OIB for example), the calibration will likely depend on the density of Off-Nadir reflections found in this region. Consequently, the derived calibration might not work in regions where the density of Off-Nadir reflections is different. To check if your calibration is region-dependent or not, a simple test can be operated: you can plot the residuals of Figures 1 and 2 on a map and check if you observe regional patterns or not. This figure could be provided in the supplementary material.

***AR*: Thank you for this suggestion; this is something we originally considered but did not include since there was no evident regional dependence to the linear regression residuals. We have included the plots below for reference. The calibrations themselves, i.e. the extent to which satellite freeboard deviates from the snow or ice freeboard is of course region dependent and this is why we choose pulse peakiness as a means to characterize the surface. Our methodology assumes that surface properties including density of leads are sufficiently accounted for with the pulse peakiness criteria (low peakiness regions correspond to thicker multi-year ice where less leads are present; conversely we would expect highly peaky regions to be lead-dense) to extend the calibration beyond the region sampled by IceBridge.**

AK delta freeboard calibration residuals map



CS2 delta freeboard calibration residuals map



Page 7:

L16-19: How do you evaluate the spatial and temporal resolution?

***AR*: The spatial and temporal resolution that give the most number of grid cells with a minimum of 50 OIB and satellite points in each. This has been clarified in the manuscript (page 8 lines 7-8).**

L28-30: Could you give the correlation coefficient (Pearson's)?

***AR*: This is now provided.**

Page 9:

L2-4: As you consider that the bias you fit is due to penetration effects, then yes, a fs > 0 would imply that the Ku-band signal penetrates through sea ice. However, if one considers that this bias is also due to surface properties (roughness for example), positive fs values would simply suggest that the empirical retracking you use is not adapted to sea ice surfaces. This was clearly demonstrated in the study by Kurtz et al. (2014). Could you provide with a more detailed comment by integrating this other aspect?

***AR:* This is a good point, thank you for this suggestion. The results of Kurtz et al (2014) are now discussed in section 2.3 (lines 16 – 19 page 6). Lines 1-4 page 9 have been updated to include this consideration.**

Page 11:

L-15-16: As you do not clearly mention why you need to calibrate AltiKa and CryoSat-2 freeboards (penetration depth? surface properties?, . . .), this conclusion is hard to understand. Why would your calibration be different from on region to another? Because of snow properties? Lead density? Surface diffusivity? You need to give more details in order to provide a more convincing conclusion.

AR: This sentence has been removed and this is now discussed on page 13, lines 8-12. We hope that the clarifications made previously* (lines 3-15 page 4) *will now make this discussion coherent.

L29-31: Same remark as above.

Page 12:

L3: Shouldn't the title be "Uncertainty calculation"? An error should be relative to a "truth measurement". . .

***AR:* Section title changed accordingly.**

Eq 6: As fAk and fAk are clearly not independent (see Figure 1), you must take into account their covariance to calculate the uncertainty.

***AR:* We now consider variable covariance in our uncertainty calculation.**

Page 14:

L1-2: Why do you apply the same standard variation value as for CryoSat-2 (4 cm)? As far as I know AltiKa sea level standard deviation is much larger than that of CryoSat-2. I would recommend to re-calculate a standard deviation for the two datasets here in order to make sure you have the right values.

AR: **We now calculate CryoSat-2 and AltiKa freeboard uncertainty independently (section 4.2)**

Page 15: Figure 6: I am quite surprised about the r value you provide (0.73) considering the figure you show. How do you calculate this coefficient? It seems to me that you provide with the fit regression line slope. Am I right? If yes, I think it would be preferable to provide a Pearson coefficient R, which is more common parameter.

AR: **This is now provided.**

Page 16:

Table 2: Same remark as above.

Section 5.1: I don't understand why you consider 2016 as a particular year. As suggested by Table 2, the comparison for 2016 is almost identical as for the other years (except that you don't use 2016 to calibrate your DuST snow depth for the 2013-2015 periods). In my opinion you should not make any distinction between 2013-2015 and 2016 and re-write this section as such.

AR: **We have combined evaluations for each year into a single figure and conclusion section as suggested.**

L22: There is no link between the footprint size and the bandwidth. Also, you can have a similar footprint with 2 different frequencies depending on the antenna size.

AR: **Lines 16-17 page 18 updated accordingly.**

Page 17:

L1: "Onto a" is written twice

AR: **corrected**

L3-5: This description should be moved into the figure caption.

AR: **corrected**

L10: What does 30+ mean? Can you provide with a range of values instead?

AR: **corrected**

Page 18:

L19: Considering that you did not use validation data to validate your results, I would not use "demonstrated" here...

AR: **We have changed this to 'tested'**