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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.

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

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. 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). 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.

3) 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.

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?

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

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

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.

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

Page 4:

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

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.

L29: Not exact: see my previous comments

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?

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.

Page 7:

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

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

Page 9:

L2-4: As you consider that the bias you fit is due to penetration effects, then yes, a Δfs > 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 Δfs 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?

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.

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"...

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

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.

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.

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.

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.

Page 17:

L1: "Onto a" is written twice

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

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

Page 18:

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

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