The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
https://doi.org/10.5194/tc-2021-79-RC2, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Review of 'Advances in altimetric snow depth estimates using bi-frequency SARAL/CryoSat-2 Ka/Ku measurements' by Garnier et al

Anonymous Referee #2

Referee comment on "Advances in altimetric snow depth estimates using bi-frequency SARAL and CryoSat-2 Ka–Ku measurements" by Florent Garnier et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-79-RC2, 2021

The manuscript "Advances in altimetric snow depth estimates using bi-frequency SARAL/CryoSat-2 Ka/Ku measurements." by Garner et al. shows interesting results on satellite derived snow depth on sea ice in both the Arctic and the Antarctic. This study is timely and relevant within the context of changing Arctic and Antarctic sea ice regime and how snow depth in particular is a big challenge to retrieve from satellites.

However, I found the manuscript not ready to be immediately accepted to TC due to the following brief reasons (detailed comments to follow), and therefore recommend Major Revisions. I am willing to review the revised manuscript. Please note that authors have the right to accept/follow or reject my comments, as the comments are from my scientific perspective and welcome to be challenged.

a) Since the study exclusively focuses on snow depth retrievals, I expected the authors to have a dedicated discussion on how snow geophysical properties in general has an effect on the retrieval biases. This is crucial since snow on FYI and MYI exhibit variations in their geophysical characteristics. Between Arctic and the Antarctic, there is almost a contrasting difference in snow covers (for e.g. substantial flooding on Antarctic sea ice and resultant slush/snow-ice formation that can hamper snow depth retreivals). Unfortunately, I did not see any of these critical discussions affecting the retrievals. In general, I found the authors neglecting snow properties impacting snow depth retrievals. This is clear in both introduction, results and dicusssions.

The authors simply washed out the 6.5 cm bias between ASD and DuST, due to recalibration issues. But I do not think that is the only reason. Although I appreciate authors briefly discussing the effect of snow properties (lines 580 to 590), I think they need to be 'tied' with what the biases show both in the Arctic and the Antarctic.

Moreover, it is right now, difficult to differentiate between the results from the Arctic and the Antarctic in the discussion section. I would strongly suggest to have sub-sections for the discussion and NOT to merge with the conclusions. The new discussion section could have discussing results separated by hemispheres, discuss results based on bias from different products, and also disucss the impact of snow properties on these SD retrievals.

b) Overall, I found the paper a bit difficult to follow, especially with a lot of grammatical errors, use of unsual sea ice/remote sensing terminologies (e.g. emerged fraction of sea ice, sea ice sinking etc), and difficulties to follow the figures. Also, Iit was a bit difficult to follow the paper objective (lines 125 to 135). Although, I understood what the objective is, it needs to be stated clearly in these lines. I totally understand there are a lot of datasets, models and climatologies to compare and report to, it may be easier for a reader to follow the objectives if the datasets are not included in the objectives description, but keep them separate. Just a suggestion.

c) There are a lot of acronyms in this paper, which makes it difficult to follow. Suggest to make a list of acrnoyms table so that the readers can follow?

Detailed Comments

Abstract: Although the authors have reported results from the Arctic, there are none reported from the Antarctic. Nor, there are any statements about how the snow depth products are reliable from the two hemispheres. The abstract would read more impactful if these are addressed.

Line 5-6: Maybe its just me, but when I talk about dual-frequency penetration of Ka- and Ku-band frequencies from air/snow and snow/sea ice interfaces, I write Ka-band SARAL/AltiKa and Ku-band CryoSat-2, than the other way around with CryoSat-2 written first. Also, note frequency followed by the satellite, than vice-versa.

Line 11: Anyways, the authors mention all model and satellite data. So why not also mention in situ IMB and airborne OIB in the validation sentence.

Line 12: "space and time patterns" sound unusual in the context. Suggest to use 'a consistent spatiotemporal snow depth solution'

Introduction: The authors provide a good overview of snow depth products that are available from satellite data, in situ and airborne data and models/reanalysis. But there are lit review about the uncertainity caused by snow properties affecting snow depth retrievals (although I noticed few lines about it in the discussion section). This is

important to address as the Ka- and Ku-band penetration and differences has a strong sensitivity to snow property and their spatial and temporal variaiblity.

Line 24: no need to captialize 'Sea Ice Thickness'

Line 25: What do you mean by emerged fraction of sea ice? I am sure you meant fraction of sea ice above sea ice ? Keep it simple.

Line 26: definition of leads is implied in sea ice. Suggest to delete phrases in brackets.

Line 30: remove 'the' before snow depth. Use SD right after snow depth. Add 'adding to the overall SIT uncertainty'

Lines 31-33: Not just snow loading and speed effects, but also affecting snow properties (both geophysical and thermodynamic) from freeze-up to melt-onset.

Lines 33-40: I think its better to move this section towards the beginning of intro, as it fits well with the general importance of snow on sea ice. If authors plan to do so, please also add how snow affects volume budget calculations, light penetration affecting primary productivity and impacting sea ice stability affecting migration and hunting.

Line 39: air/ice drag and not the other way. Also what surface roughness are you mentioning? snow surface or ice surface or both?

Line 41: remove 'the' before snow cover on sea ice. Check throughout the manuscript. Its not the snow cover that is unknown. Its the snow depth. Correct?

Line 42: Move Warren et al. 1999 from Line 44 to after W99m in Line 42.

Line 45: I wouldn't call in outdated. That's a bold statement consisdering that W99 is still a baseline snow depth data. I would call W99 data to be old. That's all.

Line 48: remove apostrophe from all years.

Lines 50-68: This section could be cleaned, seperated by hemispheres. RIght now, both Arctic and Antarctic lit review are mixed up. Reviewer 1 also mentioned about references that needs to be changed and so I do not repeat the comment. In addition to IMB as an in situ data, there should be mention about snow depths from magnaprobes. There are many campaigns from NP drifting stations, SHEBA, CryoVex, even recently concluded MOSAiC campaigns, where magnaprobe-derived snow depths have been integral in validation.

Lines 70-124: This section could also be cleaned, seperated by hemispheres. RIght now, both Arctic and Antarctic lit review are mixed up. What I miss from lines 50-124 are the overall limitations of these datasets/methods/retrievals that has led to motivating your study. It would be good to clarify that, before moving ahead to objectives.

Lines 125-136: As mentioned earlier, the objectives are a bit too specific and confusing. It would be nice to keep an overarching obejctive, followed by sub-objectives based on datasets and methods used.

Line 126: datasets is one word.

Line 127: The main part.... Remove this line. reads redundant to lines 125 and 126

Line 128: The ASD product... Aren't you developing an updated version of ASD product? So before comparison with other data, you should showcase the upgraded version correct? Moreover all the AMSR-2, DUST, OIB, CryoVex and IMB are all 'derived' datasets. Please indicate that.

Lines 129-131: I guess reviewer 1 already makes a strong point about validation of Antarctic snow depth data.

Line 134: First discuss and then conclude, not the other way around. Also suggest a seperate discussion section.

Data Processing of ASD:

Line 140: provide the data link here

Lines 144-148: Introduce AltiKa here. Moreover, I found this a bit awkward. Ka-band

assumed to be "reflected near the top of the snow pack" and Ku-band "near the snow/ice interface" are NOT EQUIVALENT to "air/snow" or "snow/ice" interfaces. This needs to be addressed as "near" can be either below or at the air/snow interface or above or below the snow/ice interface. Maybe its just a phrasing issue, but its a mistake in terms of radar scattering assumption.

Lines 148-155: You talk about Baseline C product handbook although Baseline B products are used. Clarify

Lines 156: Use Ku-band and Ka-band

Line 169-185: Its TFMRA, not TMFRA. We need to take into account of 'decrease' in velocity of Ku-band waves, as it penetrates through the snow pack. Also, epsilon can be mistaken as dielectric permittivity, therefore suggest to change the uncertainty symbol to something else.

The uncertainty calculation seems to be estimated dependent only on the snow density aspect, and not taking into account of other snow properties such as temperature, salinity or microstructure.

Especially in the Antarctic, where the ASD product uncertainty is dramatically low (only upto like 5 cm?). That's a bit odd since, as authors would know, that many of the Antarctic sea ice sectors are flooded, especially in the Weddell or the Bellingshausen sectors. The slush layers that form itself inducs significant retrieval errors in snow depth. Considering that, the low uncertainty values needs to be rechecked, since the calculation is just based on snow density estimates. Makes sense?

Also interesting are no data points within the CAA, except for few points around the Hudson Bay region? Please clarify.

Lines 190-195: Very vague description of results from Figure 1. Keeping in mind that other products are to compared against ASD, the explanation of Figure 1 is weak. Also to note that this is the first time ASD is produced in the Antarctic. Although, there are a multitude of uncertainities, I would expect the authors to expand on the ASD results in the Antarctic and discuss the biases.

Satellite Data

DuST: Similar 'to'. I see SARAL everywhere although, AltiKa is the sensor. I would suggest to replace SARAL with AltiKa, everywhere. Move 'The DuST data are...." to the beginning of the section.

AMSR-2: Since both DuST and AMSR-2 products are derived from different spatial resolutions, there needs to be an explanation about how these product resampling is carried out.

Model Data:

One of the critical comments I have in this section is why authors choose three models to compare. I couldnt find any rationale for this. Is it because one model product is better than the other or does one have a better coverage? Please clarify.

I guess MERCATOR also gives snow depth in the Antarctic? Clarify.

Validation Data

For all validation datasets, right now, it is a bit confusing from where in the Arctic and in the Antarctic, the data are collected from. Although, authors do mention when data was collected, they dont mention from where. I would strongly suggest to have a hemispherical map showing the flight tracks and locations, or even a table with location and coordinates would be very useful for this paper and also a good source of citation for data tracks. This is applicable to both Cryovex, OIB and IMB datasets.

4.1 Methodology

Line 277: 500 by 500 km?

Line 281: climate annual mean: average of all snow depth monthly maps from all years or average of every month for all years?

Line 282: .. average of snow depth annual standard deviation? Also, previously snow depth was referred as SD, here it is shown as sd. Please correct.

Line 287: I didnt understand what Ny means. The authors say, number of year in the considered time period. It will be 1 always correct? A bit confused.

Line 297: I guess reviewer 1 already mentioned about snow depth data in the Antarctic. So would be useful to incorporate that in the revised version and analysis (if data is publicly available).

4.2.1 Results in the Arctic

In the following sections, my comments will be more detailed and less on grammatical issues (will wait for the next round of review to work on them).

Lines 300-314: Ok, now here is my issue with Figure 3 ASD and DuST mean and standard deviation towards the East Siberian Sea and east of Baffin Bay. The authors mention about thin ice in these regions (btw its not Queen Elizabeth Islands, they are far north of where the snow depths are shown here), and thinner snow pack in these regions. But please keep in mind that the temporal window you have chosen is March and April when the ice thickness is the maximum. Food for thought and an anlaysis to revisit. Why does the AMSR-2 data shows thicker snow in these regions? A repeated question on why there are no CAA data shown (even for AMSR-2 data?). Please clarify.

Lines 320-330: An issue here with the analysis is that authors blindly talk about ice types here, but there is no data or discrimination of ice types shown here. I understand an additional plot would add to analysis complexity, but it would be ideal to have an ice type product like OSI-SAF to relate the snow depths to (as a function of ice types). Then statements such as 'patterns of deep snow over MYI' would make better sense.

4.2.1 Results in Antarctica

The first line of this paragraph is the impactful aspect to this paper. Yes, it is the only snow depth product. But I am a bit disappointed with how the results are discussed in this section. Both AMSR and GIOMAS shows strong regional-scale snow depth variability, as compared to almost very low in the ASD product. This strong bias between radar altimetry, models and passive microwave radiometry opens up a big avenue of disucssion towards the altimetry retreival challenges in snow depth on Antarctic sea ice. I think authors have shown the capability of ASD in the Antarctic (and Congrats for that), but needs strong foundational explanation of why this strong bias is occuring. Like I mentioned earlier, one of the big differences is the thick snow in certain sectors that cause reduced penetration of Ku-band microwaves, causing these biases. Think about it and maybe explain potential reasons causing these biases. It would be unfortunate and a waste of analysis if these are unaccounted for.

4.3 Comparisons with in situ and air borne data

Lines 358-365 and Table 1: Although, the reason for the over and underestimations with OIB vs ASD and DuST are attributed to OIB product quality, it still do not 'completely' answer the estimation errors. I think the authors could 'speculate' these biases linked to spatiotemporal changes in snow properties.

Figure 7: I think if table 1 is used, then figure 7 is almost redundant, since it does not provide any additional information. If figure 7 is used, then remove 'fitting line' legend. You may indicate that in the figure caption

Lines 380-395: There are a lot of 'assumptions' without any evidence reported here. 'Ka-ASR estimation exhibits very thin snow thickness. Although it might be expected in this area, this solution still contains unrealistic negative values due to the fact that ASIRAS and KAREN freeboards are nearly equivalent over FYI (without negative values).' So my first query is how did you 'expect' thin snow cover in these areas?

AMSR2B and DuST nearly 'always' overestimate the snow depth. Why does it 'always' overestimate? I read through the paragraph and found the analysis very vague. Suggest to rewrite the section.

Towards an ASD snow depth climatology

Line 396: That's a bold statement showing that ASD data shows good results? But do authors conclude from analysis with airborne and ins situ data ASD is better? From the analysis, the ASD snow depth are a first-time estimate in the Antarctic, but not good. Also till lines 403, the ASD-clim is presently valid only for Arctic correct?

Also Figure 10: are the blue points from all tracks? It would be good to show them with different color for different years. The legend seems to have a problem. Please fix it.

Impacts of snow depth on SIT

Lines 429-430. Need references for these values.

Lines 444-452: I do not have a clue why freshwater budget came into the analysis, out of

the blue. Suggest to delete the entire lines if there it is irrelevant to the paper.

Figure 12: Either the caption is wrong or the panel titles are wrong. The first column says min, while caption says mean. Please correct. Also suggest to rescale levels of values in the color bar for both top and bottom plots.

The authors also mention 'global' means. I would stick to using 'pan-Arctic' means (please correct everywhere in this section), as 'global' referes to all sea ice occuring everywhere in the planet.

Conclusions and Discussions

I have already made my suggestions to split conclusions and discussions. Also, in the discussion section, please have sub-sections for both Arctic and the Antarctic, also discuss the study limitations in terms of validation data and also issues in biases due to snow properties.