MS Record: tc-2021-791:

Title: Advances in altimetric snow depth estimates using bi-frequency SARAL/CryoSat-2 Ka/Ku measurements.

Florent Garnier, Sara Fleury, Gilles Garric, Jérôme Bouffard, Michel tsamados, Antoine Laforge, Marion Bocquet, Rénée Mie Fredensborg Hansen and Frédérique Rémy

Referee#2 global comment

The manuscript "Advances in altimetric snow depth estimates using bi-frequency SARAL/CryoSat-2 Ka/Ku measurements." by Garnier 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.

Answers to the anonymous referee#2 comments

We would like to thank the reviewer for her/his careful reading of the manuscript and for the relevant and constructive remarks. In order to fit with your global comment, we have made an complete revision of the manuscript that should have considerably improved the quality of the writing and the readability of the document. We hope that the modifications we have made will meet your requirements. Please find below the details on how your general and specific comments have been taken into account. In this « author's response document », the referee's comments are in bold type, the answers are in italic type, and the corrections to the revised manuscript are in normal type.

Answers to the ananymous referee#2 : general comments

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 retrievals). 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 discussions.

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 discuss the impact of snow properties on these SD retrievals.

After discussions with co-authors, we have decided to keep a single "Discussion and conclusion" section. The main reason is that adding a full section on snow properties would confuse the main purpose of this article. The objective is to present our new snow depth product from altimetry (ASD) in order to enhance future studies that would, among others, analyse the impacts of snow properties. In order to be sure that this is clear from the beginning of the manuscript, we have modified the last part of the introduction (please see answer to comment b))

We think that the ASD product could definitely help to better characterize snow properties but the analysis is very complex and would need a dedicated article. Anyway, we agree that snow properties are a key point that was not enough mentioned. For that purpose, we have added several references on snow properties in the revised document (of course also in the last section). Note that we have also better separated the results in the Arctic from those in the Antarctic in the last section.

Since both DuST and ASD products are based on the difference of the same Saral Ka-band and CryoSat-2 Ku-band data, the deviations between the two datasets should mainly come from the difference of the methodology used to compute snow depth. For instance, DuST is constructed from SAR mode data while ASD from pLRM data. The bias between LRM and SAR due to roughness is corrected from a calibration with OIB. Nevertheless, we agree that a thorough examination could reveal correlations with snow properties. These points have been added several times in the revised document. For instance :

L227-230: However, unlike the ASD product (which uses the CS-2 PLRM data, see Sect. **??**), the DuST product uses SAR mode freeboard estimates of CS-2. The CS-2 and SARAL data are then calibrated against data from National Aeronautics and Space Administration's (NASA's) OIB airborne measurements to align the freeboard observations from SARAL/AltiKa with the snow/air interface, and the freeboard observations from CS-2 with the snow/ice interface.

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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, it 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.

We apologize for the editorial oversights in the manuscript. We have proofread the manuscript thoroughly to correct as many grammatical errors as possible. Also, In order to increase the clarify and better define the objectives of the paper we have modified the end of the introduction as follows:

L 132: A recent study of Zhou et al. (2020b) presented an inter-comparison of available snow depth products from re-analyses, passive radiometry and altimetry (DuST). Similar, this paper reviews the state-of-the-art by comparing current main snow depth estimations. Yet, the main objective is to present and assess the upgraded version of the ASD product (see Sect. 2), covering the 2013-2019 period in both hemispheres. The article fits in with the upcoming HPCM CRISTAL mission (Kern et al., 2020) and aims to demonstrate the potential of such snow depth data to further specific studies on, for instance, improved sea ice volume estimations, freshwater budgets, snow properties or data assimilation. Except from an analyse of the impact of the snow depth uncertainty on SIT retrieval, it doesn't explicitly address these questions.

The paper is organized as follows:

- First, we detail the methodology to process the ASD product and present all the datasets used in this study.
- We compare ASD with the main other existing snow depth satellite and model data in both hemispheres.
- The datasets are then assessed against OIB, CryoVEx and IMB validation data in the Arctic.

– To circumvent the temporal limitation induced by SARAL, we propose a preliminary snow depth climatology based on ASD.

– The last section aims to quantify the SIT level of uncertainty due to the snow depth from an ensemble of SIT estimations calculated from the satellite and model snow depth datasets presented in the previous sections.

– We finally discuss and conclude, emphasizing current needs for snow depth data in sea ice studies.

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

As suggested we have added a list of acronyms table (table A5). We now only explicit in the text the most important acronyms, which reduces multiple line-long passages put into parenthesis.

Answers to the Referee#2: Detailed Comments

Abstract

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.

These corrections have been made in the whole revised document.

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

We agree that it is better to also mention all the validation data. We have modified the sentence as follows:

L11-12: The ASD product is further validated in the Arctic against the Ice Mass Balance (IMB) buoys, the CRYOsat Validation EXperiment (CryoVEx) and Operation Ice Bridge's (OIB) airborne measurements.

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

We have modified the sentence as follows:

L13: These comparisons demonstrate that ASD is a relevant snow depth solution, with spatiotemporal patterns consistent with those of the alternative Ka/Ku DuST product, but with a mean bias of about 6.5 cm.

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 uncertainty 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 variability.

As suggested in the general comments we have added several mentions to snow properties in the revised manuscript.

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

This comment has been taken into account in all the revised document.

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.

In order to reflect this comment we have modified the sentence as follows:

L26: The principle is to measure the fraction of the sea ice above the sea level...

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

We agree that the definition of leads is implied in sea ice but we would like to keep the introduction easily understandable for potential reader that are not familiar with it.

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

We have modified the sentence L30 according to your comment.

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.

We have underline that snow properties impacts snow depth measurements:

L32-37 : For example, it is necessary to account for the snow loading (Laxon et al., 2013) and for the decrease in altimetric radar speed as it penetrates into the snow pack (Kwok and Cunningham, 2015; Mallett et al., 2020). In fact, variabilities of snow properties affect radar signals and then the snow depth measurements. More generally, snow cover has strong impacts on the sea ice (e.g., Massom et al., 2001; Powell et al., 2005; Bin et al., 2008; Sturm and Massom, 2009; Ricker et al., 2014) that affects the entire climate system (e.g., Ingram et al., 1989; Ledley, 1991; Eicken et al., 1995; Singarayer et al., 2006).

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.

Following your comment, we tried to put this part at the very beginning of the introduction but we found that it breaks the logical progression of the introduction. Also, this part is still at the beginning of the introduction. However, we have taken into account your comment and add the following sentences :

L40-46: Such processes of sea ice formation and melting govern sea ice physical and chemical properties that impact the biological processes in sea ice (Van Leeuwe et al., 2018). The vertical distribution of light under sea ice that control biological processes and biogeochemical fluxes is also strongly linked with the snow depth (e.g., Perovich, 2007; Arndt and Nicolaus, 2014; Arndt et al., 2017). In addition snow accumulated over sea ice from precipitations represent a tank of freshwater likely to be carried into the ocean. Recent increasing of seasonal ice has promoted the amount of snow water discharge in the ocean which impacts the freshwater budget (Andersen et al., 2019; Overland et al., 2019).

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

We have modified the revised document as you suggested. We are mentioning the snow surface roughness.

L46: Snow cover also modifies surface roughness that impacts the air/ice drag coefficient and transfer coefficients of latent and sensible heat fluxes (Andreas et al., 2005).

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?

We have removed "the" before "snow cover" in all the revised document. We have replaced snow cover by snow depth

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

This modification has been done.

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

To demonstrate the weakness of the W99 snow depth climatology is an important feature of the article. We would rather keep the word outdated since W99 is constructed from data prior to the first impacts of the climate change. Because of the fast and recent strong evolutions of the Arctic sea ice, W99 does not fit with the current period.

Line 48: remove apostrophe from all years.

This correction has been taken into account in the revised document.

Lines 50-68: This section could be cleaned, separated 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.

In the revised manuscript, this paragraph only refer to the Arctic. The following paragraph focus on the Antarctic in which we have carefully considered the comments of referee#1. In addition, we have added a mention to the MOSAIC expedition and the ICEBIRD campaign as follows:

L75-77: From September 2019 to October 2020, the international MOSAIC expedition monitored the central Arctic (Shupe et al., 2020) providing, among others, data on snow precipitation, snow water equivalent (SWE) and magnaprobe snow depth measurements (e.g., Munoz-Martin et al., 2020).
L87: Future validations in the Antarctic will also benefit from the recent AWI IceBird campaigns carrying a snow radar since 2019 (https://www.awi.de/en/science/climate-sciences/sea-ice-physics/projects/ice-bird.html).

Lines 70-124: This section could also be cleaned, separated 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.

The logical progression of the introduction is by type of observation. At level 1, we separate the validation data, the model and the satellite data. Within these "pseudo-sections", we then separate (as far as possible) the Arctic and the Antarctic. We agree that another progression could have been chosen. Our goal was to consider ASD as "a snow depth products for both hemispheres", even if we do understand that it is, right now, only validated for the Arctic. Since a separation by hemisphere almost need a full re-writing of the introduction we propose to keep this way. However, in order to consider your comment and to avoid confusions between the two hemispheres, we have modified some sentences and the order of some paragraphs. The main limitations are the space and time coverage (due to SARAL) and the hypothesis of the ku-band and ka-band dominant scattering surfaces (related) to snow properties. We clearly present these hypothesis in the revised manuscript

L159-161: Note that the validity of this hypothesis is strongly linked with the space and time variability of snow properties such as snow density, grain sizes, the liquid water content or the surface roughness.
L163-164: Note that SARAL orbit is limited to 81.5° in latitude, which reduces the coverage of snow depth data in the Arctic.

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

We have re-written the objectives and the plan of the manuscript in the revised version. Please see our response to general comments.

Line 126: datasets is one word.

We have written "dataset" (in one word) in all the revised document

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

we have removed this sentence.

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.

Indeed the ASD product presented here is an updated version of the product presented in Guerreiro et al. (2016). We do agree that first compare the two version and explain the differences would have been a consistent approach. However, the assumption on the difference of penetration between Ka-band and Ku-band is nearly the only common point between these 2 products. For instance, in the previous version of ASD, snow depth is only calculated at crossing points of SARAL and CryoSat-2 trajectories. Monthly maps are then an extrapolation of a few snow depth data. Also the pLRM CS-2 data were a prototype version computed by CNES.

Of course we have first investigated the deviations between the 2 products. It appears that the results are quite different, with significantly higher gradients in the previous version but it is difficult to draw some conclusion from the differences. Is it mainly due to extrapolations ? is it due to monthly mean freeboard smoothings ? along track better consistencies ? Note that, in addition, the previous ASD version hasn't been computed as a product for users but more as an experimental dataset for the work of a PhD. The data have been only computed for two winters (2013-2015) and only cover the central Arctic.

Since the article is already quite long and compare a larger amount of data we have chosen to avoid this comparison that, for our point of view, does not bring much information. Then apart from your imperative requirement for publication, we would prefer not to put more information concerning this previous product in this article.

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

We have carefully taken in to account the reviewer#1 comments to better specify the validation data in the Antarctic. For instance, instead of stating that OIB data are not available, which is not entirely true, we mention that only L1b data are available in the Antarctic. Please see answer to referee#1 for more informations.

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

Please see our response to general comments

Data Processing of ASD

Line 140: provide the data link here

The data link has been added to the proposed location in the document.

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.

We do agree that this phrasing might be subject to misunderstanding. The sentence as then be modified as follows:

L155-157: Snow depth calculation is based on the difference of penetration between the Ka-band range altimeter of SARAL (which is assumed to be reflected at the air/snow interface) and the Ku-band range altimeter of CS-2 (which is assumed to be reflected at the snow/ice interface).

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

We use the GOP CryoSat-2 Baseline B until 2017 and Baseline C from 2017 to 2019. The reason why we did not used the baseline C for all the time period is simply because the Baseline C was not available at the time we had processed the data. It is now available but it would need some times to reprocess all the data. Because the waveforms are exactly the same in the two Baselines (and then the computed snow depths), we have decided to keep this version for this article. Note that we are currently re-processing the data so that the ASD product will be computed over the 2013-2020 period in both hemisphere from the baseline C only . The only difference is then that it will also cover the SARIN mode zones. You are right that it is important to let the reader (and probable future users be aware of this). For that purpose the sentences L151-155 of the article has been modified as follows :

L167-170: Since the Baseline-C PLRM GOP product was only available from 2017 at the time we have computed the ASD data, we have used the Baseline B for the period 2013-2016. It does not impact ASD since we use only the L1b product levels which have identical waveforms on both baselines. However, the baseline B does not include the SARin data. Then, the ASD data does not yet cover the SARin mode zones. The next version of the ASD product will include SARin mode zones.

Lines 156: Use Ku-band and Ka-band

We have carefully modified the document in order to always use the spellings Ka-band (and Ku-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.

These mistakes have been corrected in the revised version.

Also, epsilon can be mistaken as dielectric permittivity, therefore suggest to change the uncertainty symbol to something else.

We now use the symbol δ instead of ϵ in the revised version of the manuscript.

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 up to 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 induces 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?

We totally agree that the methodology to compute uncertainties might be subject to comments. However, the methodology we used is quite common in altimetry. Ricker et al. (2014); Tilling et al. (2015); Di Bella et al. (2018) use very similar methodology to calculate CryoSat-2 and SARAL freeboard uncertainties (uncertainty on snow depth is derived from uncertainty on freeboards). The methodology is not to explicitly consider the various source of uncertainties such as snow/ice properties. Note that a lot of other source of uncertainties are not considered such as the use of a fixed threshold for the retracker, the lead/floe classification criteria, the SLA interpolations under floes.... The various sources of uncertainty are considered all together and calculated from the statistical dispersion of the the sea level anomaly (SLA) within a radius of 25km. We assume that within this radius SLA should be identical. Dispersion of SLA values is then the measurement of uncertainties.

We clearly agree that this methodology should underestimate the uncertainties and need to be refine as flooding, slush layers and snow properties should clearly impact the Ku-band and Ka-band signals However the quantification of their impact on snow depth retrieval is a work in itself and is clearly out of the scope of this study. Note that we are currently working within the ESA FDR4ALT project on methodology to improve uncertainty estimates. Hopefully, refined methodology to refine uncertainties should come in the next years.

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

This is because we did not include the SARIN mode zones of CryoSat-2. Please see our previous answers at the beginning of this section for more details.

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 uncertainties, I would expect the authors to expand on the ASD results in the Antarctic and discuss the biases.

The description of the ASD features is rather of the next sections. However, in order to take your comment into account we have added the following sentences:

L 208: In the Antarctic, thicker snow is located in the Weddell sea and, in a less extend, in the coastal zones of the bellingshausen and Amundsen seas. It is also relevant to identify the very low snow values associated with the Ross ice shelf. In the Arctic, the snow distribution follows well the dynamics of sea ice, the most characteristic of which is the export of MYI in the Beaufort gyre. This figure also shows that the ASD data are very different from the W99m climatology, where the W99m climatology tend to exhibit thicker snow layers over sea ice.

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.

As proposed, we have moved the sentence to the beginning of the section. Also Since we use the name of the satellite for CS-2, it is better to also do it for SARAL (for homogeneity). However, in order to clarify the name of the sensors we have modified the following sentence : L155-158: Snow depth calculation is based on the difference of penetration between AltiKa, the Ka-band

range altimeter of SARAL (which is assumed to be reflected at the air/snow interface) and SIRAL, the Ku-band range altimeter of CS-2 (which is assumed to be reflected at the snow/ice interface).

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.

The gridded model and snow depth monthly data are projected onto the EASE2 grid of ASD using a simple two-dimensional multivariate interpolation (griddata function in python). We have added this specification in the revised manuscript:

L297: Model and satellite snow depth estimations are projected onto a 12.5km pixel size EASE2 grid (similar of ASD) using a linear two-dimensional multivariate interpolation.

Model Data

One of the critical comments I have in this section is why authors choose three models to compare. I couldn't 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.

We are not sure to fully understand the comment.
We have chosen 3 of the main sea ice models to perform the comparisons. The recent SnowModel-LG is not studied but is part of the study of Zhou et al. (2020a)). There might be others but we thought that it is sufficient to perform relevant comparisons.

Indeed, the MERCATOR model provides snow depths in the Antarctic. It is part of the manuscript. Maybe you meant NE-SOSIM? We have discussed with Alek Petty and, at the time we made the study, NESOSIM was not computed in the Antarctic.

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 don't 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.

The maps showing the location of flight tracks are provided with the figures. We have added one sentence to specify this point :

L269 : The validation data presented in this section are only compared with ASD in the Arctic. As mentioned in the introduction CryoVEx and OIB data in the Antarctic are still only L1b. IMB are only in the Arctic. The geographical location of the validation data are shown in the section 4.3.

4.1 Methodology

Line 277: 500 by 500 km?

It is 500x500 pixels of 12.5km It has been clarified in the revised manuscript.

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

the climatic annual mean is the average of all snow depth monthly maps from all years. We have added this specification to the revised manuscript:

L303: 1) The climatic annual mean, which is simply the average of all monthly snow depth maps from 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.

We now always refer snow depth as sd in the revised version of the document. For each year, we compute at each grid point the standard deviation map of the 6 monthly mean maps. The MAV is the mean of these standard deviation maps. We have modify as follows:

L 304: The Mean Annual Variability (MAV), is the average, on the years y, of the annual snow depth standard deviation ($std_y(sd)$) maps computed from the 6 monthly mean maps.

Line 287: I didn't understand what Ny means. The authors say, number of year in the considered time period. It will be 1 always correct? A bit confused.

It is exactly the number of years in the time period considered for the calculations of the mean annual variability and the mean interannual variability. For instance to calculate the mean annual variability over the 2014-2018 time period, we have 5 winters → Ny = 5.

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).

We have carefully integrated all the references proposed by reviewer#1 in the revised manuscript.

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 analysis 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.

We think there is a misunderstanding about geographical locations. Looking at the map in figure 1, we see that the Queen Elizabeth Islands are between the north Groenland and the Banks island. We mentioned thick ice (and thick snow) in these regions, not thin snow.

We just specify that we have not "chosen" March and April. Figure 3 are March and April bi-monthly maps because the AMSR2B data are only available for these 2 months (over the entire Arctic zone). In addition DuST use OIB data for calibration which only occurs in March and April.

As mentioned in a previous comment we do not have CAA data because we do not yet included snow depths from SARIN mode. The next ASD version will. These areas are yet covered by AMSR-2 but data are masked in order to provide statistics with the exact same data for the different products.

We are not sure that we can possibly assert why AMSR-2 has higher value. We assume that it might be linked with the difficulty to retrieve thick snow (MYI has a spectral signature comparable to snow depth) and the calibration with OIB. Over MYI, or thick ice, even the upgraded algorithm to derive snow from brightness temperature is not fully adapted and lead to overestimations. This assumption is reinforced by the results in Antarctica which show some very thick snow patterns like in the weddell sea that looks quite unrealistic (maybe these zones have MYI).

The thinner snow pattern running along the Alaska coastline from cap Bathurst is represented in all products but looks more pronounced in the ASD data. This looks better in agreement with the Beaufort gyre circulation and the McKenzie shelf conditions.

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.

It is true that we do not highlight MYI and FYI in maps. However MYI and FYI distinction is a very common feature in sea ice studies. Scientific publications very rarely present such maps. It is possible to add a figure presenting OSI SAF sea ice types but we think that the article is already very long and include a lot of supplementaries. For now we prefer not to add such a plot.

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 discussion towards the altimetry retrieval chal-

Figure 1. Map of the Arctic to clarify geographical locations

lenges 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 occurring. 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.

In order to fit with your comment we have added the following sentences in the section :

L373-380: The presence of thicker snow in East Antarctic for ASD is consistent with Worby et al. (2008) which comparisons with ARISE in-situ data have shown radiometric measurements snow depth underestimations. Wet-snow conditions due to flooding might be responsible for radiometric brightness temperature contrasts. The low variabilities in the Weddell sea for ASD were not expected since winter snow properties are extremely variable (Massom et al., 1997). In addition the large bias with AMSR2-NSIDC raises questions. Since Ku-band is supposed to better penetrate in cold and dry snow (Willatt et al., 2011), one hypothesis is that the presence of saline and warm moist basal snow layers, even in the absence of flooding, (Massom et al., 2001; Perovich and Richter-Menge, 1994) lead to ASD underestimations. For AMSR2-NSIDC, the snow depth retrieval algorithm is probably not well adapted for rougher snow that can be compared to MYI in the Arctic.

L386-389: Because of highly dynamics weather conditions, with persistent strong winds in the Antarctic, snow thickness distribution is not directly related to snowfalls (Massom et al., 2001). Conversion from precipitation to snow depth is then very different to the Arctic and Antarctic snow pack is not an uniform slab. These features should partly explain model difficulties and differences between the two hemispheres

4.3 Comparisons with in-situ and airborne 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.

We have added the following sentence :

L405: The variabilities of snow properties from OIB daily basis data to the monthly means of the other datasets certainly explain some of the deviations.

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

We would prefer to keep the Table and the Figure 7 as we think the 2 informations are important. We have removed the “fitting line” legends and indicated 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?

We expect thin snow mainly because most of the Baffin Bay is covered with first year ice and free in summer. We have added a reference Landy et al. (2017) studying sea ice in the Baffin Bay.

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.

It is an observation from Figure 9. We have removed the word always which is confusing since only one track is analysed.

Towards an ASD 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?

We don’t really attempt to demonstrate that ASD is the "best" but the relevancy of these data. In this case, the use of the word "good" may not be the most appropriate. Indeed, the statement L396 come from the previous comparisons with validation data. At present we only have been able to present the ASD-clim valid in the Arctic. We agree that it is not fully clear in the sentence lines 403. We have modified in the revised version as follows:

L441-446: for that purpose, we have constructed a preliminary altimetric snow depth climatology in the Arctic by averaging all the ASD snow depth maps of each month during the 2013-2019 period (designated as the ASD-clim). An equivalent climatology could also be constructed for the Antarctic but the lack of validation prevents its validity to be demonstrated at present. To demonstrate the relevance of this climatology in the Arctic for the years prior to 2013, the ASD-clim data are projected on all the tracks of the four OIB missions occurring between 2009 and 2012, and presented in Fig. 10.

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.

We agree that using different colors could be interesting. However, the objective is precisely to consider those data all together, on a statistical point of view. For latter analysis we will keep this relevant proposition. We have modified the legend as follows:

Fig.10. Scatterplots comparing the ASD and the W99m climatologies with OIB by considering all the tracks of 2009, 2010, 2011 and 2012 OIB missions.

Impacts of snow depth on SIT

Lines 429-430. Need references for these values.

We have added references for these values :

L472-474: As in Ricker et al. (2014), we assume the sea water density (ρ_w) is set to 1024 kg/m^3. Consistently with the approach of Laxon et al. (2013), sea ice density (ρ_i) is set to 882 kg/m^3 for MYI and 917 kg/m^3 for FYI (Alexandrov et al., 2010). Furthermore, the snow density (ρ_s) is set to 300 kg/m^3.

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.

ASD snow depths would allow to provide relevant sea ice volume estimate that would allows to re-evaluate the freshwater budget. The aim of this paragraph is to highlight the fact that these data are also important for other studies. Since the 2 other referees did not mention this point we would prefer to keep this paragraph.

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.

We have corrected the mistake in Fig.12 caption.

Fig.12. Inter-annual minimum ($min(std_m)$, first column), mean ($\overline{std_m}$, second column) and maximum ($max(std_m)$, third column) maps of the standard deviation (first row) and the maximum deviation (second row) for the month of April ($m = 04$). The snow products used to compute the maps are ASD, DuST, AMSR2B and W99m. These maps correspond to the case « obs snow products ».

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.

We do agree that pan-Arctic is much more adapted than global. We have modified everywhere in the revised document.

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.

We have done our best to re-organize and add supplementary informations to this section. Please refer to our previous answer to your general comments

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