Responses to Reviewer

Authors' response: We appreciate Reviewer 1 for her/his dedicated comments. We have made significant revisions to the content and structure of the original manuscript to ensure it meets the standards of The Cryosphere. The original referee comment is in black, and our replies are written in blue.

Major comment 1. This work seeks to address several open research questions surrounding the amount of snow on sea ice, the ability of the CryoSat-2's radar waves to penetrate snow, and radar waveform interpretation. Reducing one of these uncertainties may move the subsequently retrieved value further away from "the truth" due to the presence of compensating biases, so it makes sense to consider several sources of uncertainty at once and then optimise. This work is therefore well motivated.
In this submission the authors have interchanged several aspects of the radar-freeboard to sea-ice-thickness processing chain. In particular, they have used two different "off the shelf" radar freeboard products (AWI and Bristol/UIT), several snow products (AMSR/W99, SnowModel-LG, NESOSIM, TOPAZ4, FY3B/MWRI), and have compared the assumption of full radar penetration of the assumed snowpack to an ice-type dependent radar penetration factor. These radar penetration factors were derived from comparison to several separate (but often not independent) data sources such as airborne estimates of sea ice freeboard/thickness and drifting ice-mass-balance buoys (Fig. 3). Having made these various interchanges, the authors appear to compare their new SIT data to similar evaluation data as before, which seems like a conflict between training and testing (Table 2; Fig. 5)
Authors' response: The reviewer mentioned that we used some kinds of dataset to calculate the radar penetration factors and used the same datasets to evaluate the derived sea ice thickness. We agree with the reviewer's comment. In the revised manuscript, the airborne and buoy measurements are no longer used to calculate radar penetration rates. We used the total freeboard from ICESat-2 (IS2), snow depth from FY-3B, the radar freeboard from LARM, and snow densities from SnowModel-LG driven by ERA5 to recalculate radar penetration rates. The new radar penetration rates are more representative which cover the most regions of Arctic Ocean. Therefore, conflicts between the training and test datasets can effectively be avoided. The details are as follows:

The radar penetration model proposed in this study is reserved.

$$\alpha = \frac{c_s\,(h_f - h_{fr})}{c \times h_s}, \quad (1)$$

Where h_f and h_s are the total freeboard and snow depth, and h_{fr} is the radar freeboard. c is the speed of light (3×10^8 m s^{-1}), and c_s is the radar propagation speed in the snow. In this study, c_s was obtained from a snow density (ρ_s)-dependent parameterization: $c_s = c(1 + 0.51\rho_s)^{-1.5}$ m s^{-1} (Ulaby et al., 1982).

In addition, the reviewer suggested us to discuss our optimised radar penetration factors with reference to Nab et al. (2023). In the Eq (1), the daily optimal interpolation CryoSat-2 (CS2) radar freeboard (LARM) and IS2 total freeboard are provided by Nab et al. (2023). They derived pan-Arctic radar penetration rates based on the relationship between radar freeboard (CPOM and LARM) and SnowModel-LG snow depth. They successfully demonstrated the winter Ku-band radar scattering above the snow-ice interface, giving us confidence to promote this work (in this revised manuscript). Similarly, regarding Eq. (1), once the snow depth and total freeboard are determined, estimating the radar penetration rate at the basin scale becomes possible.

The reasons for selecting these datasets are as follows:

- *CS2 radar freeboard (LARM):* Landy et al. (2020) developed a Lognormal Altimeter Retracker Model (LARM) and found that the radar freeboard derived from the LARM has lower errors compared with other retrackers. It should be acknowledged that there are still some difficulties in interpreting radar waveforms (Ricker et al., 2014), which can introduce potential bias in LARM-derived radar freeboard and further propagate to radar penetration rate calculation (Nab et al. 2023 also faces similar challenges).

- *IS2 total freeboard:* The NASA's ICESat-2 mission was launched in September 2018 with the primary goal of monitoring the height of ice sheets and sea ice with high accuracy, leading us to better understand changes in polar regions (Markus et al., 2017). ICESat-2 is capable of measuring the vertical distance from snow-covered surface on sea ice to the sea surface, which is total freeboard (or snow freeboard), and enables the estimation of sea ice thickness by assuming hydrostatic equilibrium (Petty et al., 2020). The total freeboard reached an uncertainty of 2–4 cm when compared with NASA's Operation IceBridge (OIB) airborne measurements data (Kwok et al., 2019).

- *FY3B/MWRI snow depth:* The FY-3B meteorological satellite is a second-generation polar-orbiting meteorological satellite from China launched in November 2010. The FY3B/MWRI snow depth was developed by Li et al. (2021) with a spatial resolution of 12.5 km × 12.5 km and is available for 2013–2020, encompassing the entire sea ice growth season. This new snow depth data show smaller biases with the OIB-derived snow depth, with a mean difference of 2.89 cm on FYI and 1.44 cm on MYI.

- *SnowModel-LG snow density*: SnowModel-LG is a Lagrangian snow-evolution model developed to simulate snow depth and density on a pan-Arctic scale (Liston et al., 2020). The model mainly uses two typical atmospheric reanalysis data (i.e., ERA5 and MERRA2), and simulates full surface, internal energy, and mass balances within a multilayer snowpack evolution system. Stroeve et al. (2020) conducted an assessment analysis of SnowModel-LG and showed that it can well capture the spatial and seasonal variability of Arctic snow depth and snow density.

  The new monthly radar penetration rates at the pan-Arctic scale are shown in Figs. 1-2. Each effective penetration rate was derived by averaging the monthly penetration rates from 2018 to 2020. In particular, non-physical data points were excluded from the calculation, i.e., radar penetration rates above 1 or below 0. Generally, radar penetration rates increase from fall to spring. Note that radar penetration rates tend to be lower in some marginal seas when compared to the central Arctic region.

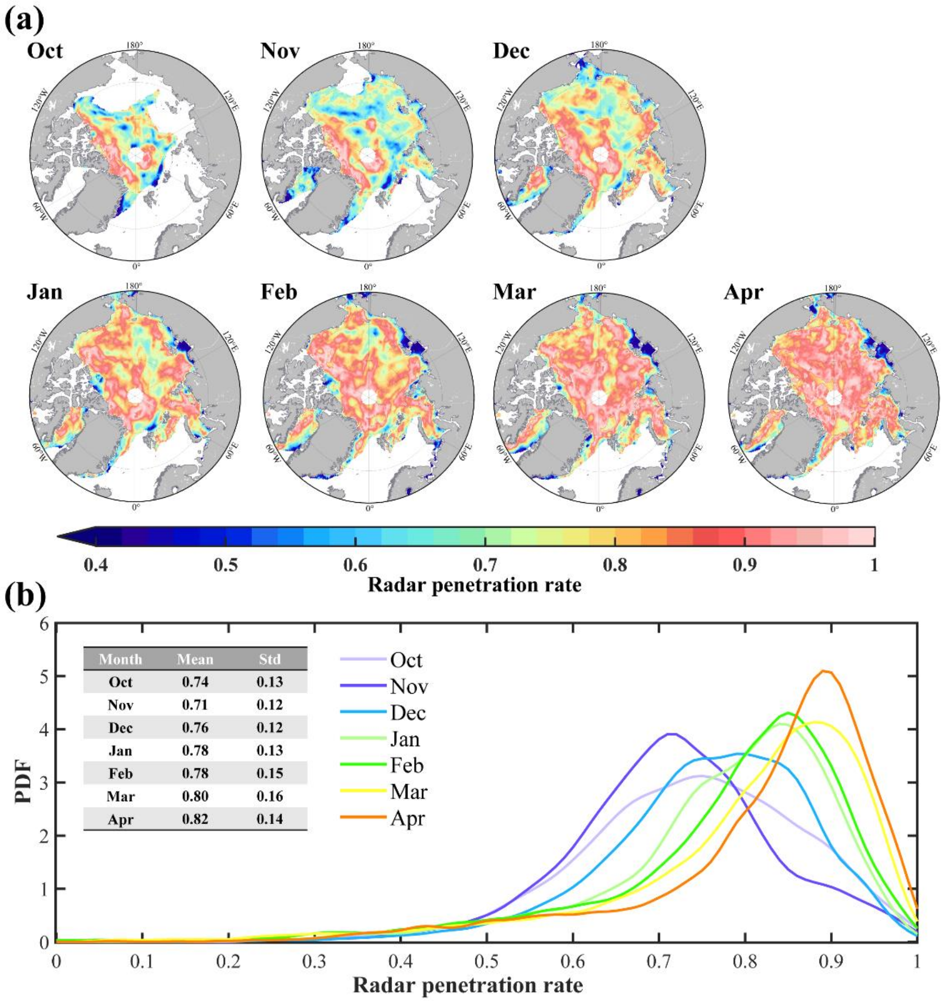

Fig. 1. Monthly mean radar penetration rates. (a) spatial distribution and (b) probability density characteristics

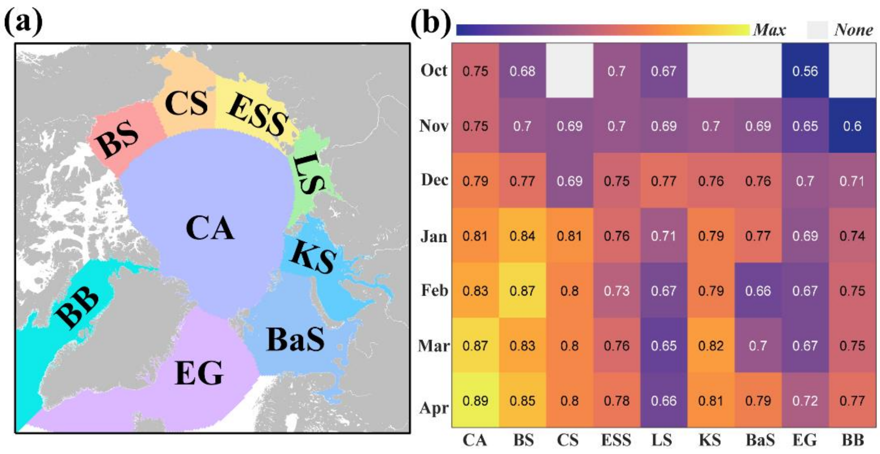

Fig. 2. Regional mean of radar penetration rates. (a) describes the sub-regions of the Arctic Ocean, including the Central Arctic (CA), East Siberian Sea (ESS), Laptev Sea (LS), Kara Sea (KS), Barents Sea (BS), East Greenland (EG), Baffin Bay (BB), Beaufort Sea (BS) and Chukchi Sea (CS). (b) monthly mean radar penetration for each subregion.

Major comment 2. To be honest, I found it difficult to identify any clear conceptual or practical advances in this work. I was also left somewhat doubting whether the claims in the abstract and conclusions were supported by the results. It seems to me that rather than a "comprehensive optimisation", this work represents several experimental recombinations of different existing components in the processing chain. Furthermore, the authors' recombinations are potentially improving one metric of skill (e.g. RMSE) at the expense of another metric (e.g. correlation R value). I also note that not all combination possibilities were explored.

Authors' response: In the revised manuscript, the title has been modified to: "Assessment of radar freeboard, radar penetration rate, and snow depth for potential improvements in CryoSat-2 sea ice thickness retrieval". In this study, we focus on the impacts of radar freeboard, radar penetration rate, and snow depth on retrieving CryoSat-2 sea ice thickness and investigate the potential improvements in sea ice thickness. We don't think our experiments improved one metric of skill (e.g. RMSE) at the expense of another metric (e.g. correlation R value). Based on the new assessment results of sea ice thickness compared with OIB L4 (Fig. 3) and CryoVEX-EM (Fig. 4), the correlation R value can be kept with the similar results with the original AWI CS2, even in some cases, the R values are higher. In addition, the reviewer mentioned that not all combination possibilities were explored. As described before, our purpose is to explore the impacts of radar freeboard, radar penetration rate, and snow depth on retrieving CryoSat-2 sea ice thickness and investigate the potential improvements in sea ice thickness. These three factors are independent. The three individual cases by focusing on the retracking algorithm, radar penetration rate, and snow depth, and one combined case are sufficient to confirm the potential impacts of these factors on the improvements in sea ice thickness. We aim to provide some feasible schemes to the optimizations of sea ice thickness derived from AWI CS2.

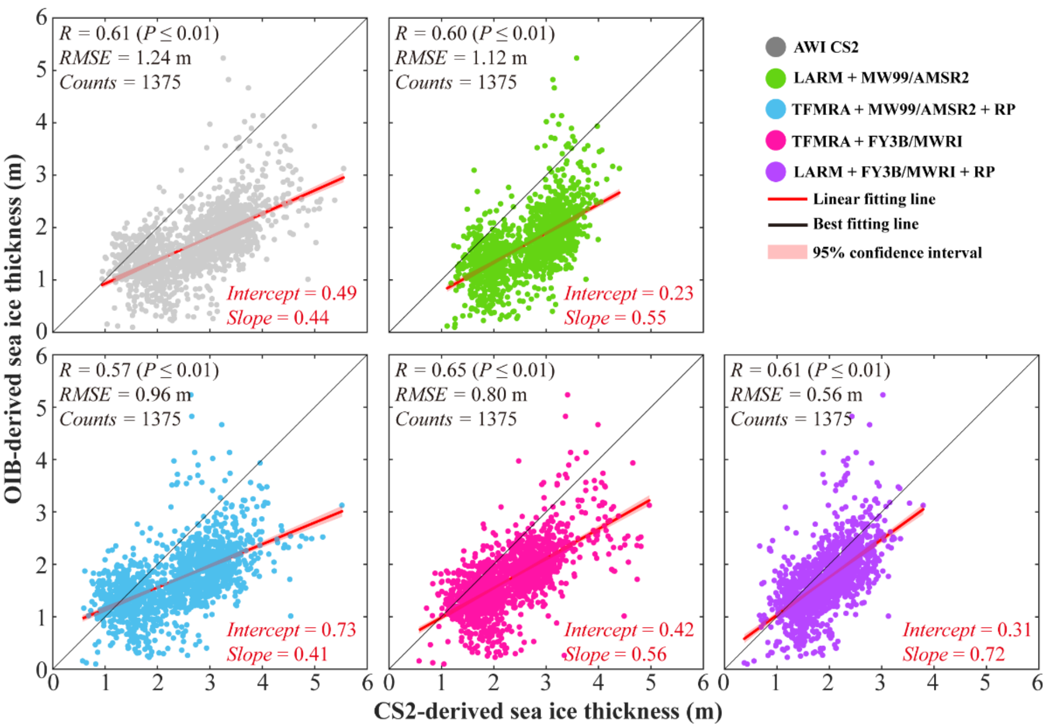

Fig 3. Validation of sea ice thickness improvement with the OIB L4. The correlation coefficient (R), root mean square error (RMSE), and the number of samples (N) are shown in each subfigure. The solid black line indicates the best fitting line, and the solid red line indicates the scatter fitting line (the fitting equation is also shown in each subfigure).

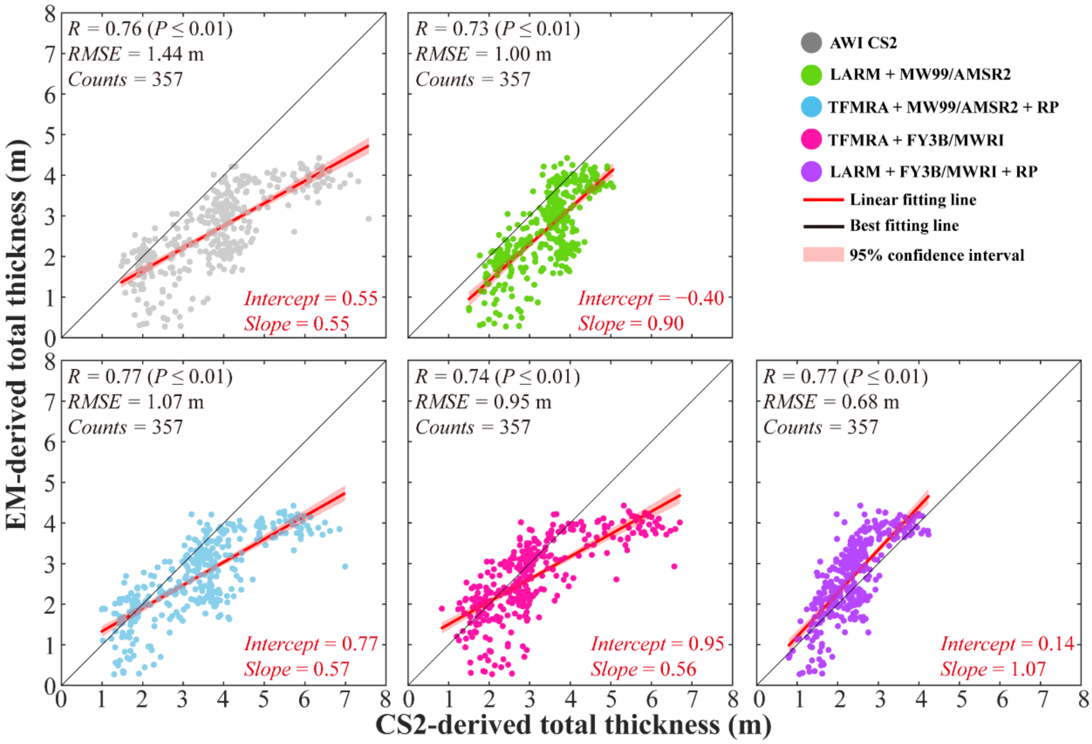

Fig 4. Validation of sea ice thickness improvement with the CryoVEX-EM. The correlation coefficient (R), root mean square error (RMSE), and the number of samples (N) are shown in each subfigure. The solid black line indicates the best fitting line, and the solid red line indicates the scatter fitting line (the fitting equation is also shown in each subfigure).

Major comment 3. I was also concerned about the treatment of OIB QuickLook data as "in situ validation" (Table 2; Figure 5), since that particular data set has several known issues and is of course an airborne remote sensing product (not in-situ). Similarly, AWI IceBird and CryoVEX airborne data are also referred to and treated as "in situ validation", but are of course also the results of airborne remote sensing with their own biases and uncertainties. Particular to CryoVEX, it is not appropriate to use Ku-band ASIRAS data processed with the assumption of full radar penetration to then work out CS2 penetration factors

Authors' response: We agree with the reviewer's suggestion. In the revised manuscript, we no longer describe the airborne measurements as in situ observations. Instead, we describe them as airborne-derived products. In the original manuscript, we did not use CryoVEX data to derive the radar penetration rate; we only used the total thickness measured by CryoVEX-EM to evaluate the sea ice thickness data.

Major comment 4. Relatedly, the issue of how representative an ice-mass-balance buoy's measurement is of the ice sampled by (a) a CryoSat-2 SAR footprint, or (b) the ice in a 25×25 km grid cell such as those analysed here, was mentioned (Sect. 2.4) but not meaningfully tackled. These issues strongly affect the suitability of IMBs for radar-altimetry evaluation. More broadly, the extent to which the authors may be optimising towards an uncertain or even biased truth is not discussed.

Authors' response: The notable difference in spatial resolution between buoys and satellite observations remains a big challenge in contemporary research. Recently, Koo et al. (2021) matched buoy data with modal values of sea ice thickness distribution derived from satellite tracks within a certain radius and found good agreement. However, we cannot access the LARM-derived along-track radar freeboard data to test this approach. Therefore, in the revised manuscript, we no longer use the buoy data for assessing sea ice thickness data.

Major comment 5. Finally, it was not possible for me to reproduce this analysis since it relies substantially on an unpublished snow dataset from the FY3B radar altimeter (which one coauthor has previously led a publication on). Since the manuscript relies so heavily on publicly available data, I thought it was a shame not to act in this spirit. The reason for the data not being made available was not given (despite an explanation being required by the Copernicus data policy). On a related note, no code was made available with this manuscript for review. These factors will limit the impact of any publication, and also limit my confidence as a reviewer.
Authors' response: I am afraid the reviewer has some misunderstandings regarding the data and code availability. In the email communications with the developers from AIW CS2, we have expressed our willingness to share all of the data and codes with the community. The reason why the manuscript did not show the publicly available data and code is that this is an unpublished manuscript. Before accepted, there must be some revisions to do. Therefore, our codes and data probably have to be changed. We prefer to share a final version of codes and data after the manuscript is accepted. In addition, the FY3B/MWRI snow depth data can be accessed at http://coas.ouc.edu.cn/pogoc/2021/0609/c9718a335873/page.htm.

Major comment 6. It is therefore my recommendation that this manuscript should be rejected, and a similar analysis reperformed and resubmitted in service of a more carefully constructed research question. I appreciate this might be received as an overly negative recommendation, but I believe our community has a long way to go until we can meaningfully conduct the comprehensive optimisation marketed in this manuscript. Such an exercise would require reprocessing of several key datasets by many different research groups for a truly "apples to apples" comparison, not to mention a deep new look at the sampling biases, systematic uncertainties and independence of airborne and in-situ evaluation data.
Authors' response: We sincerely appreciate the reviewer's constructive comments. Those comments are all valuable and very helpful for revising and improving our paper. In the revised manuscript, we have followed the reviewer's suggestion to separate the datasets into two parts: one for the calculation of radar penetration rates, and the other for the evaluations of sea ice thickness. In the calculation of radar penetration rates, we no longer used the previous datasets, but used the total freeboard from ICESat-2 (IS2), snow depth from FY-3B, the radar freeboard from LARM, and snow densities from SnowModel-LG driven by ERA5 to replace them. In the evaluations of sea ice thickness, we don't use the data of OIB Quick Look, IMB, and AWI IceBird as the reviewer suggested. We used the data of OIB L4 and CryoVEX-EM. Hopefully the revision could meet the reviewer's requirement.

<u>Major Concerns</u>

<u>Operation IceBridge snow depth, freeboard and thickness data are not "in-situ validation"</u>

Firstly (and this is a somewhat trivial point), they are not “in-situ” because they are taken from an aircraft. They are much better described as data products from airborne radar (and sometimes lidar) remote sensing.

Authors’ response: Same comment as before. In the revised manuscript, we revised it to “airborne-derived product”.

Less trivially, because they’re radar-based products, they’re subject to significant uncertainties involving radar penetration of snow, sea ice roughness, and particularly sidelobes. Much has been written about OIB snow depth retrievals, and the quicklook product that the authors have relied upon in several sections is notoriously poor. In particular, it has relatively poor performance over multiyear ice (King et al., 2015), and the underlying algorithm suffers from a persistent issue of misidentifying range sidelobes from the snow-ice interface as the snow-air interface (Kwok and Maksym, 2014; Kwok and Haas, 2015). Figure 4 of Kwok et al. (2017) illustrates that the GSFC-NK product (which I think corresponds to the OIB QL product?) shows persistently low snow depths. In any case, the figure definitely shows that several very different interpretations of the OIB raw data exist, and therefore any individual snow or ice product cannot represent some kind of “in-situ” truth.

Even in the case where there are no snow-penetration, roughness or sidelobe-related biases, OIB sea ice thickness estimates still rely on exactly the same uncertain conversion of freeboard to thickness as CryoSat2, so cannot form an independent benchmark for optimisation. For instance, I was hoping for some analysis of how the snow and ice densities used the in OIB hydrostatic conversions compared to those being used in the satellite conversions at hand, but that was glossed over. This type of material must be foundational to any activity that aims to “optimise” satellite retrievals to these OIB products. Otherwise the optimised product be biased in all the ways in which the OIB products are biased. Along these lines, describing OIB thickness products as “observations” is also dubious. The confidence with which you can “observe” SIT even with accurate measurements of snow depth and freeboard is nicely documented by Alexandrov et al. (2010; see numbers in second half of abstract).

Authors’ response: We have followed the reviewer’s suggestion and don’t use OIB Quick Look as the evaluation data. We use OIB L4 to replace it.

CryoVEX Data

The authors need to be much more specific and thoughtful about how CryoVEX data on total thickness are generated – it’s not enough to say “airborne electromagnetic sensors”. If the CryoVEX method is similar to CryoSat-2, it will suffer from similar biases as CryoSat-2 and therefore is unsuitable for independent evaluation. Furthermore, it appears that the authors have used a very limited subset of CryoVEX data from 2014. Aside from limiting the applicability of their results, my understanding is that SIT for the CryoVEX’14 campaign is from combination of the Ku-band ASIRAS system and the ALS system (Hvidegaard et al., 2015). Given the study by Willatt et al. (2011) on ASIRAS radar penetration through snow on sea ice, I think a lot more consideration needs to be given to the positioning of this data as some kind of objective truth that can be used to optimise satellite data. It’s also worth pointing out that if you look at Fig. 9 from

Garnier et al. (2021) which analyses CryoVEX 2017 data, it's clear that the difference between the ALS and ASIRAS data is often negative towards the end of the track, further indicating that the "off-the-shelf" approach to CryoVEX ALS/ASIRAS data in this manuscript may need considerable further scrutiny. At an absolute minimum, the authors should understand they cannot get at the CS2 radar penetration factor using Ku-band CryoVEX data that assumes a 100% penetration factor from ASIRAS. When comparing CS2 freeboards to ASIRAS freeboards derived from the assumption of a 100% RP, is it any wonder that we find high CS2 radar penetration factors?
Authors' response: I am afraid the reviewer has some misunderstandings here. Firstly, the CryoVEX data we utilized in our study is the total thickness measurements obtained through airborne electromagnetic detection in 2014, not ALS/ASIRAS data as the reviewer described. Therefore, CryoVEX data was not used to calculate radar penetration rate in our study.

Ice mass balance buoys

There are major issues with using single IMBs to validate measurements by radar altimeters such as CryoSat-2. IMBs are generally deployed on level ice, and are of course point measurements. This is in contrast to as CryoSat-2 footprint which is several hundred metres long and several kilometers wide depending on the ice roughness. And of course this is very different to the 25×25km scale on which this manuscript's analysis is conducted. It's also worth considering that IMBs essentially break as soon as the ice on which they're situated begins to grow dynamically, and dynamic growth can be quite a significant contributor to the thickness in a CS2 footprint and also in a 25 km grid cell. So much more serious consideration needs to be given to how and whether the IMB data correspond to that collected by CryoSat-2, and whether they are suitable for a tuning/optimisation exercise such as this.
Authors' response: As mentioned above, the IMB data is no longer used in the revised manuscript.

Radar Penetration Factors
I assume 0.77, 0.96, and 0.91 for the penetration factors were generated from the means of the histograms in Fig. 3A (not the modes, and not the mean of the Gaussian fits) – this should be clarified. More information is also required on how the "all data" histograms are constructed. This is because there are presumably many more data points from some sources than from others: were their contributions weighted for measurement reliability or quantity? It seems like the only reason that FYI penetration is significantly lower than MYI is because of the contribution of IMB data (panel f), but this is probably related to the fact that IMBs have a distinct sampling bias relative to both the other data and their surrounding environment (discussed above). So is it reasonable to allow the derived penetration factor to be so strongly influenced by the IMB contribution? This again goes back to how different sources of evaluation data should be weighed against each other in terms of reliability and quantity.

How do the authors justify the width of the uncertainty bars in Figure 15? The shaded bars around the line seem surprisingly thin for a quantity that's eluded the CryoSat community so effectively over the last decade. Could it really be the case that the penetration-factor consistently goes up in January and then back down in Feb by a magnitude considerably larger than the uncertainty bars? What is the spatial range of applicability of these statistics? How are the printed numbers of distinct in-situ observations generated? After all, there are hundreds of thousands of SnowRadar/ASIRAS

waveforms and ALS spot heights going into this analysis, So I guess the figures in the thousands correspond to the number of contributing 25 km×25 km grid cells? If more observations go into an aggregated 25x25 km data point, then we should probably weight that data point's reliability as being higher than a grid cell that contains only a few data points. Has this been factored into the uncertainty analysis?
Authors' response: As mentioned above, we have changed the datasets to calculate the radar penetration rates.

I have several further questions about the radar penetration factor analysis, both concerning how it was done and how it is reported, but will leave it here.

Radar freeboard data and retracking
I don't think it's legitimate to claim to have applied a "comprehensive optimisation of an improved retracking algorithm" (from the abstract) when no retracking takes place in the manuscript. This is misleading to the casual reader. A comparison has not been made between retracking algorithms (TFMRA & LARM), but instead between two radar freeboard products (AWI & Bristol/UIT). To imply that differences in the RF products only represents differences in the retracking approach is a risky business. The authors state "although the different classifying waveforms, geophysical corrections, and sea level tie-point interpolation also contribute to a relatively small extent (Landy et al., 2020)." That's not exactly my understanding from reading that paper; Figure S3 shows there definitely are some differences that derive from the "in-house" treatments of AWI vs Bristol/UIT. That's the reason that Landy et al. 2020 emulated the other retrackers with the same processing: to learn about the retrackers in isolation. I think that's the approach that should be taken here if the retrackers are to truly be "optimised" rather than the radar freeboard products
Authors' response: We agree with the reviewer's suggestion. In the revised manuscript, we don't describe the "improved retracking algorithms", and used "LARM radar freeboard product".

Optimisation Metrics
Firstly, the metric referred to as R needs some clarification. Is this the Pearson product-moment correlation coefficient? R is also often used for the coefficient of determination, which essentially captures the data's deviation from the line y=x (i.e. it also captures the bias, where Pearson does not). Using the product-moment correlation coefficient has its pitfalls. i.e. with heavy optimisation for Pearson you end up capturing the variance of your evaluation data, but at the cost of increasing your bias. This issue should be handled explicitly. These concepts are at the core of the "bias-variance tradeoff", and allied concepts in optimisation such as overfitting. While RMSE is sensitive to the bias, it also can be large when spread about the y=x line is large and the bias is small. So I think the metrics against which the processing chain is optimised need much more careful consideration when defining the optimisation exercise
Authors' response: R used in the manuscript is the Pearson correlation coefficient. This is the common correlation coefficient in most studies. As described above, the correlation R value can be kept with the similar results with the original AWI CS2, even in some cases, the R values are higher. In addition, in the revised manuscript, we added the mean error (ME) and mean absolute error (MAE) to evaluate the improvement of sea ice thickness.

Some More Minor Things

The effect of reducing the radar penetration factor from unity to some fractional value is basically to reduce the subsequently calculated ice freeboard and thus the derived thickness. This does the same thing as assuming a deeper snowpack. Since the authors are varying the snow and the RP at the same time, they should grapple with this conflict explicitly: when we observe the sea ice to be "too thick" relative to our evaluation data, we often don't know whether it's because our assumed RP is too high, or whether our assumed snow is too thin. How can we optimise in light of this? One option is to just assume that airborne surveys of snow depth are correct, and use that snow depth to then optimise the RP. The authors haven't done this, but it might be interesting? But it does take a bit of a leap of faith regarding airborne snow depths.
Authors' response: In the individual case with corrected radar penetration rate (TFMRA + MW99/AMSR2 + RP), the radar penetration rate is calculated from airborne data, assuming that total freeboard and snow depth derived from airborne measurements somewhat represent the "true value". This is consistent with the reviewer's description.

Title: I think we should steer clear of subjective adjectives like "comprehensive" in titles. One could easily argue that this analysis is not comprehensive as it uses a fairly limited set of in-situ sea ice thickness measurements relative to those available, and uses limited subsets of CryoVEX and IceBird data. I recommend removal of this word
Authors' response: The original manuscript's title has been modified to: "Assessment of radar freeboard, radar penetration rate, and snow depth for potential improvements in CryoSat-2 sea ice thickness retrieval".

L42: "Recent advances in satellite altimetry began in 2003." – This is a very subjective sentence and I'm not sure what it adds. Envisat began operating in 2002 for instance. What defines "Recent" here?
Authors' response: We have deleted this sentence.

L68: Lower errors, not minimal.
Authors' response: We have revised it.

L140: I think it would be better to cite the official NSIDC source for these data: https://nsidc.org/data/nsidc0758/versions/1
Authors' response: Revised.

L509: Perhaps I've missed this, but I don't think the authors have specified whether they're using the ERA5 or MERRA2 run of SnowModel? When comparing SnowModel and NESOSIM it's of course important to make sure they're forced by similar data, or else you're basically just comparing reanalysis precipitation data rather than the models themselves. Same with TOPAZ – I assume this is driven by some ERA-type product?
Authors' response: The SnowModel-LG data used in this study is driven by ERA5 and TOPAZ4 data is driven by ERA- interim.

Fig. 1: There's no reference to the bathymetry in this paper, so I think shading it into this plot confuses things.

Authors' response: Revised.

Fig. 2: If you label the "Reference ellipsoid" in this figure you should reference/explain it in the text.
Authors' response: We have removed the "Reference ellipsoid" from Fig. 2.

L379: Think this sentence needs rewording for clarity.
Authors' response: Revised.

L447: The authors should discuss their optimised radar penetration factors with reference to Nab et al. (2023), who derived RP factors based on the relationship between radar freeboard and SnowModel-LG depths.
Authors' response: Added.

L594: I was disappointed to see that the snow depth data from Li et al. (2021) are not made publicly available, so many of the results presented in this paper are not replicable by either me as a reviewer, or the community at large. As well as limiting my confidence as a reviewer, this will limit the impact of the paper since the supposedly optimised products are not available or reproducible in future. The Copernicus Publications data policy states "if data are not publicly accessible, a detailed explanation of why this is the case is required", and I can't see why this was ignored. Furthermore, no code was made available with the manuscript, which also limits my confidence in recommending the paper for publication.
Authors' response: Same comment as before. Please check the answer in Major comment 5.

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