Response to Referee comment on "Winter Arctic sea ice thickness from ICESat-2: upgrades to freeboard and snow loading estimates and an assessment of the first three winters of data collection" by Alek Aaron Petty et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-39-RC1, 2022

Original referee comment in black, our responses in blue

General comments

The paper assesses the impacts of a number of changes to ICESat-2 ATL10 processing and to the NESOSIM snow model on estimates of along-track and gridded sea ice freeboard and ice thickness. This assessment is important for users of high level sea ice products such as ATL20 gridded sea ice. Overall the paper is well conceived and written. However, there are a nuber of issues that need to be addressed before the paper is ready for publications. I list these below. I also have a number of specific comments.

We sincerely appreciate the time taken by the referee to provide the review of our paper and the thoughtful suggestions. Please see below for our responses.

Overall, the quality of the figures is good. However, some of them could be improved by adding descriptive titles/labels to each panel. For example, figurure 7 has titles but these appear to be file variable names. Rather than "ice_thickness_unc", it would be more helpful to readers to have "Ice thickness uncertainty" spelled out. Likewise with panel (i) "ice thickness int" would be better as "Interpolated ice thickness". The authors might also want to think about a better layout and if all panels are necessary.

Yes, good points about the panel labels. We have improved and/or added these in the revised manuscript. We have decided to keep the panels as is other than edits/changes made based on other review comments. We have also removed the ice type panels from the time-series plots based on a later suggestion.

The Jupyter notebook is an excellent addition as is making the code available.

Thank you, we are excited about this aspect of our work!

Different releases are used for different evaluations. The authors show that there is little difference between releases 003 through 005 but it would make for a cleaner, and more up to date, analysis to use release 005 throughout. The only exception being to show differences between releases 002 and subsequent releases.

We have now updated the CryoSat-2 comparisons to use rel005 instead of rel003 data (the only place other than the release comparison where rel003 was used). Differences were negligible and we have updated the figures in the text accordingly. Based on a later comment we have also updated the CryoSat-2 comparison figure to improve readability. We still use rel004 in the snow comparison analysis however as that involved various different configurations that were all varied out during rel004 processing. Re-doing this would be very time-consuming and not add much value considering the negligible freeboard differences between rel004 and rel005.

I would like to see a map in the main paper showing the "Inner Arctic Ocean" region as the study region introduced as part of the methods. This would focus readers attention on the analysis region up front.

Agreed, we have now added this to the main manuscript as Figure 5.

Figure 8 is another example of a figure that would benefit from having labels such as a) sea ice freeboard. Parameter names are on the y-axes but they are small. Panels a, b, etc should be referenced in
the text.

We have now added these figure panel labels to Figure 8, and also to Figure 12 and 13. We did not feel it was necessary to provide further labels as these overly cluttered the figures when we tried this.

There are a number of places in the text where important statements are put in parentheses. I think it would improve readability to
rewrite these statements as part of the main text. Some of these parenthetical statements are unnecessary.

Agreed, we have removed several of these parentheses from the manuscript. It is a bad habit of the primary author!

Specific comments

L60. "is *being* developed"

Added, added.

L63. Suggest "collected to estimate sea ice thickness"

Added, changed.

Section 2. I think it would be helpful to summarise upgrades to IS2 processing, NESOSIM and ATL20 gridding in a simple table.

We didn’t feel like a table was the best place to provide all this information. We currently include the Table (Table 1) summarizing the NESOSIM configuration upgrades, but IS2 processing upgrades were more descriptive so we think it’s best to keep this to the description already included in the manuscript.

L111 prefer "km" to be consistent.

We followed the convention of the NSIDC user guides here and feel this is easiest to interpret for the user.

L124 "0-3 cm freeboard changes at basin scales". Does "basin-scales" refer to the Inner Arctic region used in the current paper? Maybe say "an increase in basin average freeboard of up to 3 cm.

Added, changed.

L139 Suggest "New releases of ATL07 and ATL10 also reflect upgrades to the underlying ATL03 processing, such as improvements in geolocation.

Added, changed.

L141 and 110. ATBD for ATL07/10 use "surface reference" rather than "reference sea surface". To avoid confusion it might be better to use the same terminology as the ATBD.

Added. We have used “sea surface reference” to be more consistent with the ATBD which sometimes uses this and sometimes shortens it to just sea surface.

Figure S1. Would it be better to have this figure in the main text? Also, the point here is that the number of reference surfaces is reduced from rel002 to rel003 because dark leads are not used. However, the count difference is positive. It make more sense to me to have this reduction as a negative number.

Agreed, we have moved this to the main manuscript and changed this to show counts relative to rel002. Now Figure 2.

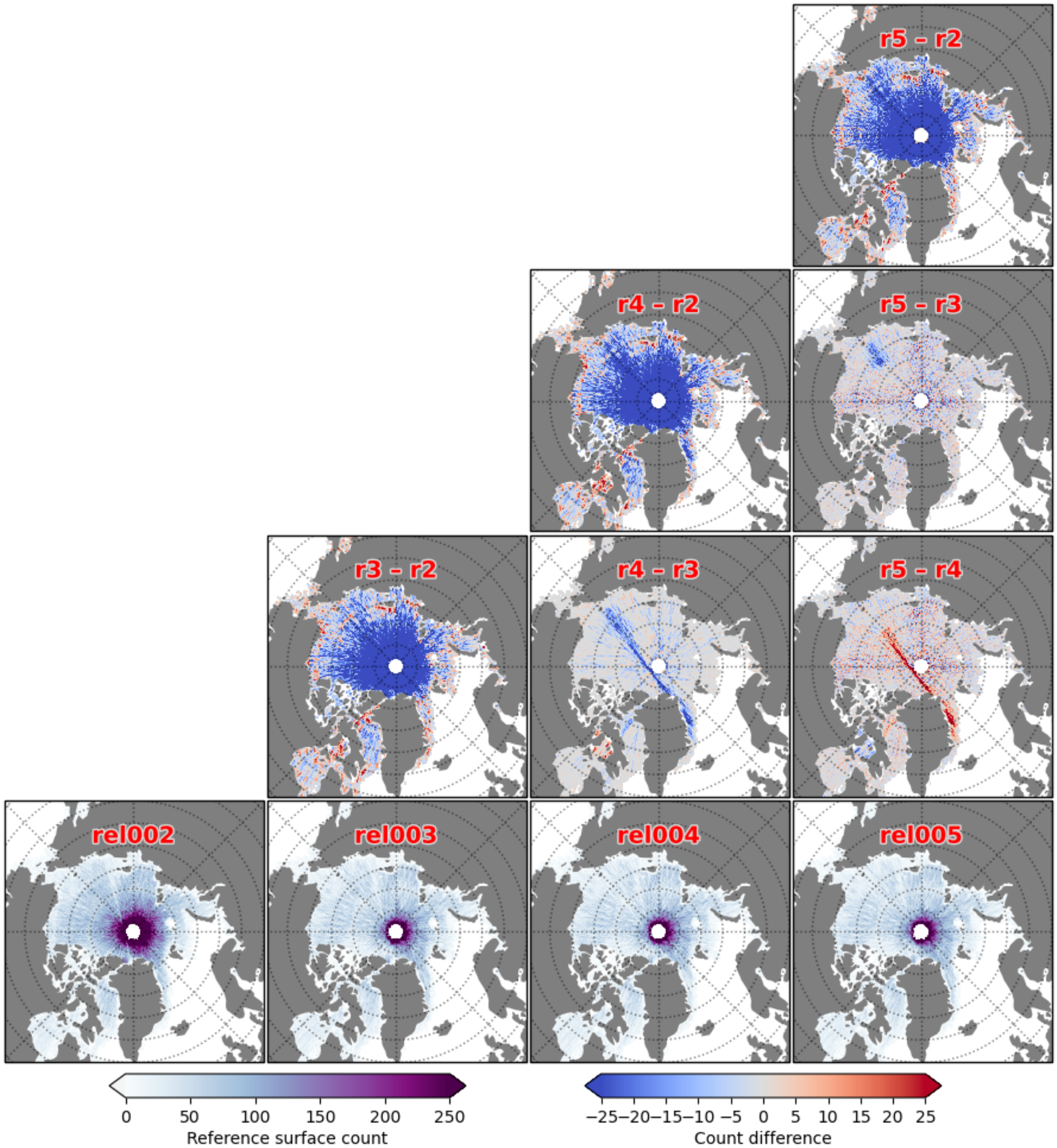


Figure 2: (bottom row) number of 10 km along-track reference surfaces from the three strong beams from November 2018 to April 2019 for Release 002/rel002 (left) to Release 005/rel005 (right). Panels above show the difference in reference surface counts from releases rel003, rel004 and rel005 relative to rel002.

L190 Effectively the /beta and /gamma terms in equation 1 are corrections to solid precipitation. It is not clear to me what the difference is between the two terms. They could be combined into a single loss coefficient.

We introduced the atmosphere snow loss term as an added blowing snow to atmosphere loss term as we expect some fraction of snow that is disturbed during high winds will be sublimated away to the atmosphere, which we developed to be an added function of the new unitless atmosphere loss parameter and not a function of ice concentration.

We agree that for simplicity it makes more sense to combine these coefficients in this equation which we have now done. We now refer to a “blowing snow atmosphere loss coefficient” which has units of s^{-1} as in the now renamed blowing snow open water loss coefficient. The value has been reassigned by combining the two terms into one, now with a value of 2×10^{-8}.

L217 Do you mean "For each OIB snow depth product, snow depths are binned into 100 km grid cells using a drop-in-the-bucket averaging procedure. For each grid cell, the median snow depth of the three products is then assigned as the grid cell snow depth". So in all cases, you are taking the middle value. If the number of products was larger, I can see this as an acceptable approach to avoid outliers but for just three values, you can't really identify an outlier. It would seem that the mean is a better estimator.

Yes, that is the correct interpretation. We decided on using the median and not the mean as we are aware of some odd behavior in some regions and some dates for some of the algorithms. For example, Kwok et al., (2017) shows that the SRLD-derived snow depths exhibit a strong positive trend including very thin snow depths at the start of the OIB time-series, which appears unrealistic (as also noted by Kwok et al., 2017). We admit that three products is not much of a distribution to truly assess outliers, but we were keen for our results to not be consistently skewed by one algorithm being constantly biased compared to the others, hence our approach here.

L 230. "within reason" This needs some clarification. Are there limits you can set on depth or start date?

This is a concept we are currently exploring in the more sophisticated NESOSIM calibration efforts cited in the paper (Cabaj et al., 2021). Due to the lack of modern and reliable late summer/early fall snow depths at basin scales our knowledge is mainly heuristic: Arctic sea ice refreeze generally begins sometime in September following the Arctic sea ice extent minimum.

In response to this comment and a comment from reviewer #2 we have included more discussion on the SnowModel-LG end-of-summer snow depths, which generally show a complete removal of snow in August in those simulations, with snow depth increasing in September onwards.

“as another tuning parameter, constrained mainly by limited evidence in the literature. For example, the Warren et al., (1999) climatology (W99) shows a mean snow depth of 3 cm in August including depths of up to 8 cm near the Greenland/Canadian Arctic coastline based on the quadratic fit to observations. However, output from SnowModel-LG presented in Stroeve et al., (2020) shows zero snow depths in August in the earlier (1985/1986) and later (2015/2016) time periods of that time-series. As NESOSIM includes no snow melt terms, we prefer instead to initialize later in the year (Sep 1st) and prescribe an expected end of August mean snow depth based on our original temperature scaled W99 August climatology.”

Figure 3. The left panel is busy. I suggest having a separate panel for October and April. The horizontal grid-lines should be lighter or removed.

Agreed, we have now split up the panels into October and April which has increased the readability of the figure.

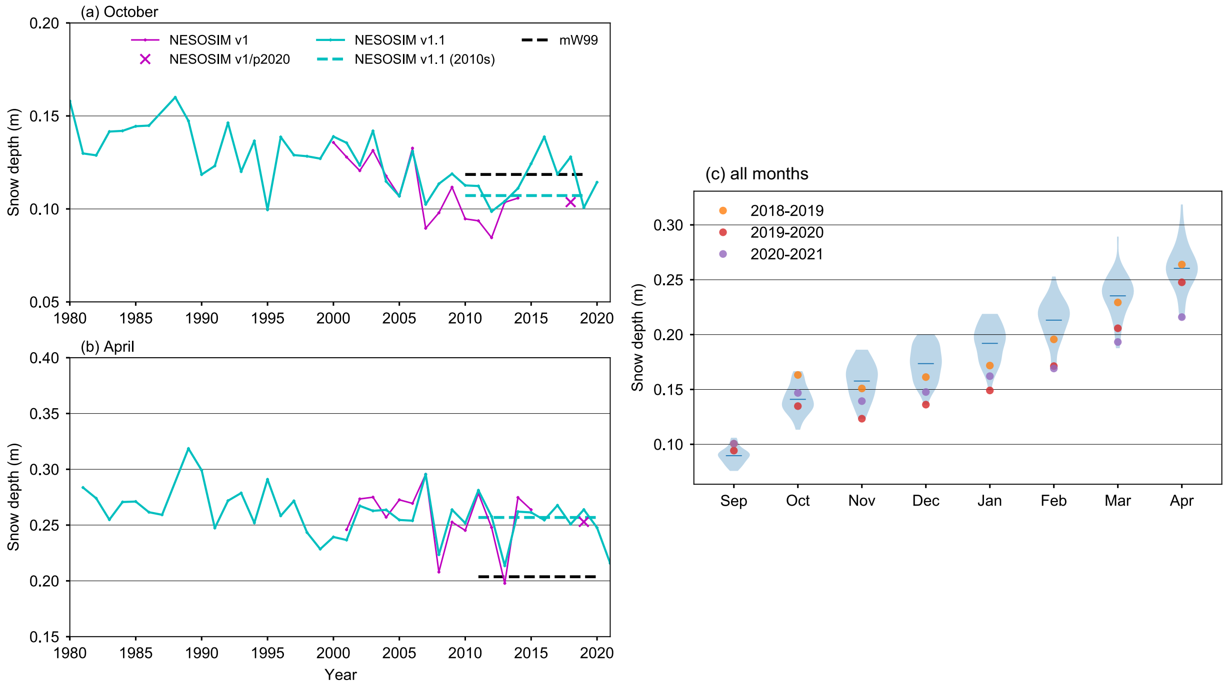


Figure 4: (Mean Arctic snow depths in October (a) and April (b) from NESOSIM v1.0 and v1.1 within an Inner Arctic Ocean domain (Figure 5). NESOSIM now depths are also masked where concentration (from passive microwave) is less than 25%. The cross markers show the extended ICESat-2 NESOSIM v1.0 results used in (Petty et al., 2020). The dashed cyan horizontal lines show the NESOSIMv1.1_2010-2020ave snow depths averaged across the respective month, while the dashed black lines show the modified Warren climatology (mW99) in October and April respectively for regions of coincident NESOSIM v1.1 coverage. (c) violin plots showing interannual distributions of monthly mean snow depths from NESOSIM v1.1 within an Inner Arctic Ocean domain from 1980-2021, colored markers indicate mean monthly snow depths for recent (ICESat-2) years.

Note that there was an error in the original figures in how the Inner Arctic Ocean region mask was applied which = has been fixed for these new plots. The main difference is a small shift in all snow depths and a small relative increase in the April mW99 snow depths, which were lower in the original manuscript, and some slight differences to the violin plot ICESat-2 snow depths.

We have also added new maps to the SI (now Figure S1) showing maps of the differences between the NESOSIM 2010-2020ave and mW99 snow depths for October and April too – highlighting the regional differences including strong differences in the Kara Sea region especially.

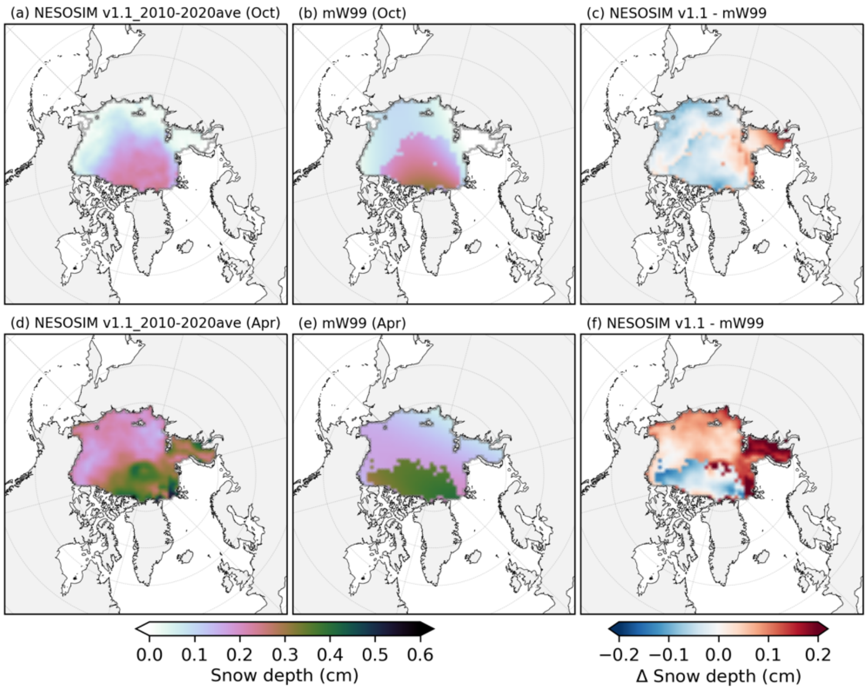


Figure S3: Comparisons between NESOSIM v1.1_2010-2020ave and the modified Warren snow depth climatology (mW99) within our Inner Arctic Ocean domain. The mW99 snow depths are limited to valid NESOSIM grid-cells. Top row shows October (Oct) comparisons while the bottom row shows April (Apr).

L254 One of the arguments for not using the Warren climatology for snow depth is that it is not representative of the present day conditions. The previous paragraph and Figure 3 have been used to argue that recent years snow depth are also lower than average and may be declining. So why would you use a climatology of NESOSIM.

We were wrong to refer to this as a climatology. We now refer to this as a 'modern era representation' and have labelled this dataset NESOSIM_v1.1_2010-2020ave to be clear we only use data from the recent 10-year period to better reflect current conditions.

Wouldn't using output from an operation product or low latency reanalysis be a better option?

It is our understanding that there is a wide-spread in snow depths across available operational products which are generally not calibrated to available observations (e.g. Operation IceBridge). We agree there is potential there but are unaware of a reliable product for this purpose with consistent output.

L266. The redistribution method needs a reference.

Added the Petty et al., (2020) and original Kurtz et al., (2009) citation.

L296 The smoothing/gridding procedure needs more explanation. It would be helpful to say why each of the steps are done. Why use Delaunay triangulation - generally this method is used to interpolate unstructured data? Presumably the KDTree algorithm is to speed up the search for neighboring cells.

We have reproduced all maps and the time-series analysis with the non-interpolated data. This was previously a mix, however our testing has shown the difference in results to be negligible when considering basin-scale aggregates. Our main motivation for introducing the interpolation was to fill in the pole hole and increase spatial coverage and mitigate spatial biases when comparing across months/years, especially considering some of the declines in coverage since the removal of dark leads from the freeboard derivation. However, we admit our method here is crude and do not wish this to distract from the main analysis which is now entirely based on non-interpolated gridded data and we have removed and reordered the discussion of the gridded dataset and the interpolated variables.

L467: "We also include in this Version 2 IS2SITMOGR4 dataset smoothed and interpolated variables of freeboard, snow depth and thickness in an initial attempt to fill in the pole hole and mitigate the spatial sampling biases. These preliminary variables are not used in the subsequent analysis presented here but are available to interested users and shown in the Jupyter Book discussed below. We expect that future work will explore more sophisticated interpolation procedures and blending with other thickness datasets, which we discuss more in the summary section."

Some notes on the method: Delaunay triangulation interpolation benefits from being a flexible interpolation procedure for our needs. The input data is already gridded but the interpolation is able to fill in gaps across variable sizes and directions, e.g. gaps across vertices and more than one grid-cell away. Although we admit simpler algorithms could have been used, this is already a built-in function with the core scientific Python ecosystem making it easy to implement for our needs. Similarly, the KDTree algorithm is perhaps overkill as the problem being solved (finding distance of each grid-cell to the nearest raw valid grid-cell) is a relatively simple one considering the fact the data is already gridded and not too large. We've decided to drop the mention of KDTree as that isn't needed here. Note that the scripts used to generate the entire gridded datasets will be made available so interested readers will be able to reproduce our exact methodology if needed. The interpolation code is already contained in the Jupyter Book which we have now highlighted in the revised paper.

Figure 4, L343. How do these look for other months and for other years? No need to show them but a comment in the text would be helpful.

It was actually challenging to maintain all the needed datasets across different versions and releases so we only have these months on-hand to discuss. We feel they represent the key changes through the winter accumulation season. We look at monthly changes in the gridded comparisons which show no obvious step-change or anomalies in the results.

L354. Significant or major?

Agreed that is better language, made the change.

L356. Prefer peak rather than mode. Mode could be confused with operating mode.

Agreed that is better language, made the change.

Figure 4. How many segments are used to generate these plots? Are dark leads more common in November?

The issue of lead counts and dark vs specular in ATL07 was discussed more in Kwok et al., (2021) and Petty et al., (2021). PDFs of January, June and October 2019 SSH separated by leads counts are shown in Figure 3 of Kwok et al., (2021) showing specular lead counts dominating over dark lead counts. Dark lead counts increased through to January as overall coverage increases also.

In our thickness processing we do not store information regarding the surface type (e.g. specular or dark lead) as we're only using freeboard and associated variables, e.g. segment length, to generate our thickness estimates. We could add the freeboard segment counts to the plots if desired - it is a lot, but drops from rel002 onwards as the ref surf count plots highlight - but we do not believe this would add much value.

L370. The name NESOSIMv1.1clim has not been introduced yet.

Agreed. In Section 2.2.1. we have now referred to this as the modern-era mean and included the relevant label 'NESOSIM v1.1_2010-202ave' here and elsewhere in the manuscript and figures.

L389. Suggest "In Figure 6, we show the correlation coefficients, mean bias and standard deviations of ICESat-2 monthly gridded ice thickness from rel002 and rel003 compared with ESAs CryoSat-2."

Agreed, we have used this and also added a bit more clarity to this and the following sentence.

What are the standard deviations of? Why mask data less than 0.25 m?

Standard deviation of the differences, so the standard deviation between the two after removing the mean bias. We have clarified this based on the previous comment.

Also added "to focus more on the representation of consolidated pack ice between the two sensors, rather than the added complexities of thin and marginal ice."

Figure 6. I suggest removing the shading and, for each month, plot release 002 and release 003 as separate columns. That way you can see the ovelap. The shading suggests the data is continuous rather than discrete monthly data.

Agreed, we have now reproduced these plots as bar plots which we think has improved the readability. Based on the earlier comment these are now also base don rel005 ATL10s.

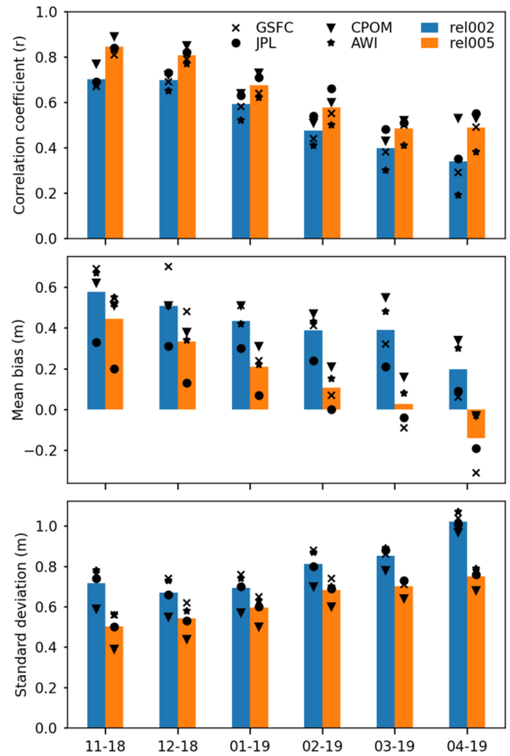


Figure 8: *Comparison statistics of monthly gridded CryoSat-2 thickness for four different CryoSat-2 products (GSFC, JPL, CPOM, AWI) with monthly gridded ICESat-2 sea ice thickness using rel005 ATL10 and the same snow loading and ice density input assumptions from November 2018 (11-18) to April 2019 (04-19). Data are compared within our Inner Arctic Ocean domain and for grid-cells in both datasets that contain thicknesses > 0.25 m.*

L445. NESOSIM presecribes snow density for new and old snow. The bulk density is a weighted average of these two values. How much can be read into variations in density?

This is a fair point, the parameterization is crude and we offered this up more to understand its impact on thickness. We have added the following:

"Due to the crude nature of the NESOSIM density parameterization, we do not view this analysis as a reliable interannual snow density assessment but highlight this more to understand the density variability impact on our ice thickness estimates."

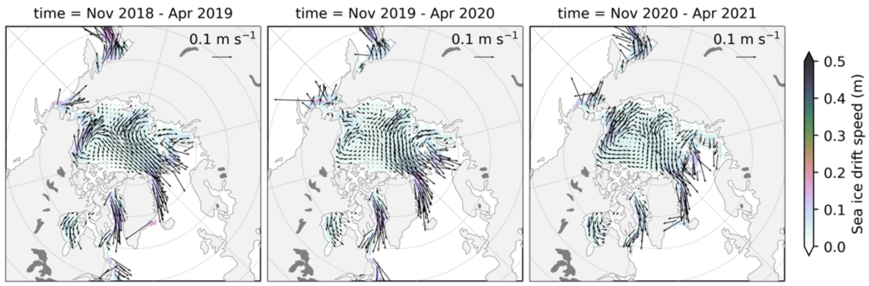
Figure 8. Why is sea ice concentration lowest in October? Is this an artifact of averaging.

This is largely an artifact of coverage changes due to sea ice refreeze and the fact we don't include grid-cells with a mean concentration <50% in ATL10. We have added the following:

"Note that the concentration decline from September to October is due to changes in data coverage as regions with ice concentrations < 50% are not included in ATL10. "

Figure 9. The flow vectors obscure the thickness data. They are not really discussed. Are they necessary? Could they be relegated to supplemental material?

We have now added these overlaid on drift magnitude as a new row in the figure (now Figure 11).



Line 535. Care needs to be taken with ERA5 (or any reanalysis) near-surface variables over snow. ERA5 snow parameterisation is still a single layer, which does not produce realistic surface fluxes (Arduini et al 2019).

Agreed, we did not include surface turbulent fluxes partly for this reason.

L540. Are three years of data enough to make a statement about strength of coupling?

Our point here was that near-surface conditions are generally expected to be strongly coupled with the sea ice state. We did not mean to imply we are discovering this coupling by our analysis here. We have re-worded this line and other elements of this paragraph to make clearer what our limited analysis has shown.

L591. This seems to contradict what is shown in Figure 4.

We state height biases here which are different to the relative measurement of freeboard. We have added 'absolute' to make this clearer.

Figure 12 and 13. The multi-year ice fraction panel is not needed.

Agreed, we have dropped this and the FYI panel in the new figures.

Arduini, G., Balsamo, G., Dutra, E., Day, J. J., Sandu, I., Boussetta, S., & Haiden, T. (2019). Impact of a Multi-Layer Snow Scheme on Near-Surface Weather Forecasts. Journal of Advances in Modeling Earth Systems, 11(12), 4687–4710. https://doi.org/10.1029/2019MS

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Kwok, R., A. A. Petty, M. Bagnardi, N. T. Kurtz, G. F. Cunningham, A. Ivanoff (2021), Refining the sea surface identification approach for determining freeboard in the ICESat-2 sea ice products, The Cryosphere, 15, 821–833, doi:10.5194/tc-15-821-2021

Petty, A. A., M. Bagnardi, N. T. Kurtz, R. Tilling, S. Fons, T. Armitage, C. Horvat, R. Kwok (2021), Assessment of ICESat-2 sea ice surface classification with Sentinel-2 imagery: implications for freeboard and new estimates of lead and floe geometry Earth and Space Science, 8, e2020EA001491. doi:10.1029/2020EA001491.