We would like to thank Dr. Kern for his thoughtful and constructive review; we believe that his feedback will undoubtedly improve the quality of our manuscript.

We have appended an ammended manuscript to this document which illustrates the changes we have made to our submission in response to the feedback. Below we quote Dr. Kern's comments in blue, and our responses follow in red.

We would first like to first bring to the reviewer's attention a mistake made in the original manuscript. Due to a programming error we inadvertantly used radar freeboard data from a different product (that of Landy et al., 2020) in the winter of 2017/18. Because this product generally exhibits higher radar freeboard values than those used in the rest of the study due to a different retracking algorithm, we misidentified this winter at one point as 'a trend bucking year' for radar freeboards. We have now fixed this error and updated our statistics. This has had the following results:

- Regional declines in radar freeboard and resulting sea ice thickness are generally smoother.
- Negative trends in several regions are slightly increased.
- Negative trends are therefore more frequently statistically significant at the 5% level.
- Trends when calculated with SnowModel-LG in the 2002-2018 period are now in better agreement with those calculated from NESOSIM in the 2002-2015 period.

Despite these changes, the central thesis of our paper remains unchanged: the use of a snow product with regional variability and trends propagates into varaiblity and trends in regional sea ice thickness.

Summary: This very interesting paper illustrates the potential improvement in the credibility of trends in and inter-annual / intra-basin variations of Arctic sea-ice thickness estimates from satellite radar altimetry. This is achieved by a comprehensive intercomparison of the contribution of snow on sea ice on the retrieval of sea-ice thickness from radar freeboard when using the Warren et al. (1999) snow climatology on the one hand and a physical model for snow properties driven by atmospheric reanalyses' precipitation and other relevant meteorological parameters on the other hand. As expected, the inter-annual variability of the snow contributions based on the model data is considerably larger than the one based on the Warren et al. (1999) data. The paper further convincingly demonstrates that the more realistic inter-annual variation and trend in the snow parameters obtained with the model yields a new overall picture of the variability and spatio-temporal development of the Arctic Ocean sea-ice thickness.

The paper is generally well written and will have considerable impact on the scientific community. It would benefit from some re-organization (see GC1). It is furthermore quite light when it comes to descriptions of data and methodologies used (GC2). Currently, one would not be able to re-produce the work done. The inclusion of Kara and Barents Sea I find quite a hypothetical move based on the data availability and suggest to consider removing those from the analysis (GC3). Finally, there is a number of open points to discuss when it comes to the illustration and interpretation of the results presented. In the following you will find my list of general comments (GC), specific comments and some suggestions to mitigate typos and editoral issues - all for the main manuscript - followed by a short list of things I found worth to consider in the supplementary material.

Title: While your main conclusion supports the title in general, it is in some way misleading. The main focus of the paper is on the illustration that a snow depth climatology
is not well suited to compute credible trends in sea-ice thickness estimates derived from

satellite altimetry with such a snow depth as input for the freeboard to thickness conversion. In your paper, this is illustrated by usage of data from a numerical model which has experienced limited validation. Hence, albeit the improvement using these model data is obvious it is not necessarily the truth either. Hence, instead of formulating the title as a fact I suggest to include points of the above-stated.

In response to this feedback we would append a clarifying clause, so that the title reads:

> 'Faster decline and higher variability in the sea ice thickness of the marginal Arctic seas *when accounting for dynamic snow cover*'

GC1: I strongly recommend to re-organize the paper. Most of the explanations / motivations given in the subsections 1.1 and 1.2 are tied relatively close to Section 3 and should be combined with that section. In addition, subsections 1.1 and 1.2 refer to data and regions denoted in Section 2. Hence: Remove 1.1 and 1.2 and put it into Section 3. Let Section 2 start right behind the "true" introduction. That way the data sets used in 1.1 and 1.2 would be introduced adequately beforehand which eases reading and which reduces the number of open questions.

We have rearranged these sections accordingly. Section 1.1 (on our method of separating the impacts of snow and radar freeboard data on thickness determination) has been moved to the methods section (Section 3). We agree with the reviewer that our illustration of the limitations of W99 in Sects 1.2.1 & 1.2.2 would have been better placed after the data description (Sect 2). Rather than put this in the Methods section (Sect 3), we have moved it to the beginning of our Results section (Sect 4). We hope this is satisfactory to the reviewer, but if not we will of course reconsider his original suggestion of Sect 3.

GC2: Both, the description of the data used as well as of the methodologies used lack some clarity and/or do not contain all information required. One good example: The ESA-CCI radar freeboard data set used comprises data of two different satellites with some overlap. It is not clear from the description in the data how long the Envisat and how long the Cryosat-2 part of the data used is - plus a motivation of the choice made - plus a discussion about the biases between the radar freeboards of these two satellites, which have a different sign based upon the region. Some of the descriptions also appear to contain errors which ask for re-phrasing.

We agree that not specifying the transition point from Envisat to CryoSat-2 radar freeboard data was an oversight. We would add the following clarifying information into the "Radar Freeboard Data" subsection:

> CS2 carries a delay-Doppler altimeter that significantly enhances along-track resolution by creating a synthetic aperture. For this reason as well as its higher latitudinal limit, we used CS2 radar freeboard measurements over Envisat's during the period when the missions overlapped (November 2010 - March 2012).

We have also made changes to our data description and discussion sections with regard to the potential effect of biases between the radar freeboard measurements of the two satellites. To "Data Description" (Sect 2.2) we have added:

> To create a radar freeboard product that is consistent between the Envisat and CS2 missions, Envisat returns are retracked using a variable threshold retracking algorithm. This variable threshold is calculated from the strength of the surface backscatter and the width of the leading edge of the return waveform such that the inter-mission bias is minimised (Paul et al., 2018). The results are comprehensively analysed in the Product

Validation & Intercomparison Report (ESA, 2018). One key finding of this report is that while Envisat radar freeboards are calculated so as to match CS2 freeboards during the period of overlap over the whole Arctic basin, there are biases over ice types. In particular, Envisat ice freeboards (not radar freeboards) are biased 2-3 cm low (relative to CS2) in areas dominated by MYI, and 2-3 cm high in areas dominated by FYI. We discuss the implications of these biases in Sect. (5.3).

To our Discussion section, we have added a subsection “Inter-Mission Bias between Envisat and CryoSat-2” (5.3). This reads:

An extensive validation exercise for the merged products indicated that although Envisat radar freeboards match well with CS2 freeboards in the Arctic overall, some biases do exist over specific ice types (ESA, 2018). In particular, analysis of the inter-mission overlap period indicates that Envisat freeboards were biased low (relative to CS2) in areas dominated by MYI, and high in areas dominated by FYI.

We first make the point that this will have a relatively minimal effect on our findings regarding interannual variability, as $\overline{\text{Snow}}$ is unaffected by this and $\sigma^2_{\overline{RF}}$ is likely relatively independent of the absolute magnitude of $\overline{RF}$.

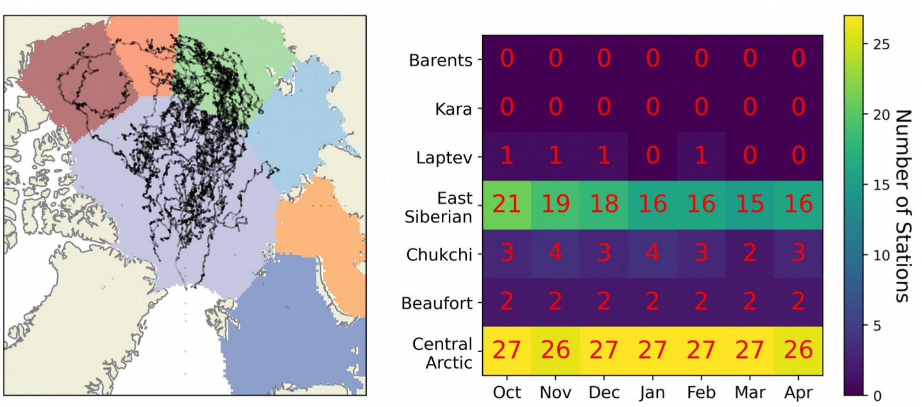
With regard to trends, if Envisat radar freeboards (and thus $\overline{RF}$) are in fact biased high over FYI between 2002-2010 (relative to CS2), then the total trend in many regions dominated by FYI could potentially be smaller than calculated in this manuscript.

We do however add that our findings regarding the impact of declining $\overline{\text{Snow}}$ is unaffected by any inter-mission bias in $\overline{RF}$. Because the trend in $\overline{SIT}$ is determined by both $\overline{\text{Snow}}$ & $\overline{RF}$, the trend in $\overline{SIT}$ will always be more negative when calculated with downward trending data for $\overline{\text{Snow}}$.

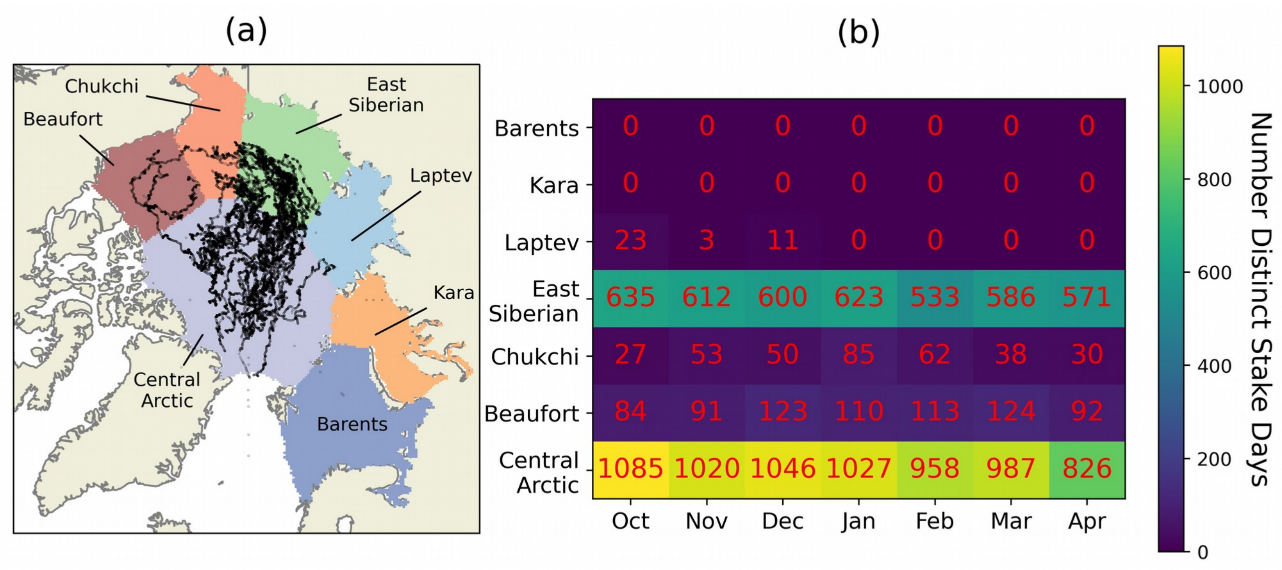
GC3: The overall credibility of the paper would benefit from a more critical consideration of the application area of the Warren et al. (1999) climatology. Sampling density, number of observations, as well as the distribution of the snow depth observations over time combined with the usage of a polynomial fit limits the usefulness of these observations in the regions Kara Sea and Barents Sea. One good solution would be to omit these regions.

Following the suggestion of the reviewer we analysed the original positional data (found within the meteorological observations) from drifting stations NP3 – NP31. This was all the data available to us, and was supplied previously in a personal communication by the NSIDC.

After plotting the tracks of these 27 drifting stations, we counted the number of stations that visited each region in each month:



We note in the above figure that repeat visits by stations on the same or consecutive years do not add to the tally. For example, NP22 lasted for four years and visited the same regions in the same months on consecutive years. However it then became apparent that some stations were not making snow measurements during some regional ‘visits’, and this should be included in the consideration of the sampling density as suggested by the reviewer. Instead we identified all distinct dates on which snow stake data was gathered by each NP station. We then cross-referenced this with the positional data found within the met data, to break down the number of distinct stake-measurement-days in each region by month. We believe this is a suitable metric for the spatial sampling of the drifting stations.



We have now included this figure and a description of the analysis within the W99 component of the Data Description section.

With regard to omitting undersampled marginal seas such as the Kara and Barents Seas from our analysis, we note that poor drifting station coverage has not stopped sevaral authors from using (m)W99 to derive sea ice thickness in these marginal seas (e.g. Sallila et al., 2019; Li et al., 2020; Li et al., 2020b; Belter et al., 2020) particularly in the pursuit of estimating sea ice volume (Tilling et al., 2015; Tilling et al., 2018; Kwok et al., 2018; Laxon et al., 2013). . We therefore would like to consider these regions in this manuscript, but with the clear caveat (which is now made explicit to the reader) that mW99 is likely not representative of the snow conditions. The subsection that we have added reads:

2.3.1 Drifting Station Coverage Illustration

At this point it is instructive to briefly illustrate the coverage of the drifting stations from which W99 was compiled. We analysed position and snow depth data from the twenty-eight drifting stations that contributed to W99 (Fig. 2a). It is clear that the vast majority of these operated in the Central Arctic or in the East Siberian Sea, with very little sampling done in most other marginal seas. But while these tracks illustrate the movements of the drifting stations, it is important to note that the stations were not always collecting snow data which would contribute to the W99 climatology. To assess the spatial distribution of snow sampling, we cross-referenced the position data with days on which the drifting stations recorded the snow depth at their measuring stakes. We then calculated the number of `measurement-days' in each region-month combination (Fig. 2b). We note that when two drifting stations were operating at the same day, we count this as two distinct days (as they were rarely so close together so as to collect redundant data).

This reveals that no snow measurements were taken in the Barents and Kara Seas, and none in the Laptev Sea for four of the seven winter months. While `snow-line' transect data also contributed to W99 (and indeed was used in preference to stake data where possible), we find that snow-line data was overwhelmingly collected on days where stake-data was also collected.

Figure 2 illustrates that the quadratic fits of W99 are not obviously appropriate for use in several of the marginals seas. However we note that a number of authors have still used the climatology for sea ice thickness retrievals in these regions, often in the course of sea ice volume calculations (e.g. Laxon et al., 2003, 2013; Tilling et al., 2015, 2018; Kwok, 2018; Li et al., 2020a; Belter et al., 2020; Li et al., 2020b). We therefore consider these regions in this manuscript, but with the understanding that mW99 is potentially not representative of the snow conditions.

Specific comments

Line 18: " ... it determines whether floes ridge or raft ... " –> My take on this would be that this happens at rather small ice thicknesses, i.e. around 20-30 cm. I am therefore not so sure whether this is such an important physical role of the sea-ice thickness.,

We have removed this line.

Line 21: "... with thin ice favoring melt pond formation ..." Why is that? Because there is little snow, which melts away more quickly than on thick is with a thicker snow cover? Otherwise I don’t see a pressing reason why melt pond formation, which is basically driven by downward short- and longwave radiation should occur more easily on thin than on thick sea ice.

Thin ice favors melt pond formation because it is generally more level. Level ice favors melt pond formation because a given amount of water covers a greater surface area. We have removed this line.

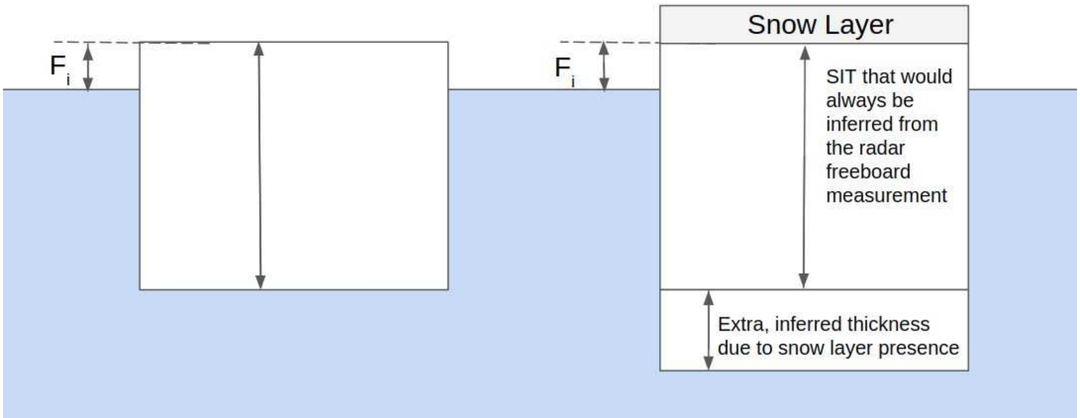
Line 76: "below the waterline" –> Why this part of the total SIT is from below the waterline? I find this addition confusing because it implies that the sea ice is thicker when it is snow covered - which is not necessarily the case. Equation (2) refers to a SIT which is similar in both cases, bare or with snow cover. It could be 1 m, 2 m, whatsoever, with or without snow. The quantities that change are the sea-ice freeboard and the radar freeboard. It seems you want to express that in case of a snow cover the part of the sea ice that is below the waterline is larger than in case of bare ice. Usually this part below the waterline is called draft. It might hence make sense to re-phrase this sentence a bit to avoid confusion.

While the presence of snow on sea ice doesn’t causally make the ice thicker, when we try to estimate the thickness of ice *with a known radar/ice freeboard*, we assume that it is thicker if it has thicker snow on it. That’s because to support the weight of the snow while maintaining radar/ice freeboard, it must be more buoyant and therefore thicker.

For instance an MYI floe with a known radar freeboard of 5cm and no snow cover is likely around 36cm thick. But if it has 6kg/m^2 of snow on it (~2cm @ 300kg/m^3), we estimate it to be around 44cm thick (Eq. 2). If it has 18kg/m^2 of snow (~6cm), we estimate a 60cm thickness. It is in this sense that six centimeters of snow can ‘add’ twenty-four centimeters of sea ice thickness to our estimation.

We can assess exactly how much ‘extra’ sea ice thickness is inferred by a snow thickness by calculating $d(SIT)/d(m_s) = (\rho_w/\rho_w-\rho_i) * 1.81 * 10^{-3}$ = 1.3cm per kg/m^2 of additional snow cover (at MYI ice density).

As for the reviewer’s point about whether this ‘extra’ thickness is added ‘below the waterline’, he is right that this is confusing. This line was originally included with the following schematic in mind:



But there is no reason that the ‘extra’ thickness inferred should be conceptualised as ‘appearing’ at the bottom, as illustrated. We therefore clarify by removing the phrase ‘below the waterline’.

To further reduce confusion surrounding this issue, we have clarified throughout the paper that we are considering the relative contributions of snow and RF data to the thickness *determination*. That is, their contribution is to the calculation, as opposed to their literal contribution in space.

Line 83: "assumes total radar penetration of overlying snow" –> How about penetration of the Ku-Band signal into the sea ice? Given the different near-surface sea-ice salinity and densities of MYI compared to FYI one might need to also make a comment on this issue?

This is an interesting point. In-situ studies by Willatt et al. (2010, 2011) suggest that a very low amount of Ku-band radar energy returns from below the sea ice surface. Further in-situ measurements from the MOSAiC expedition published by Stroeve et al. (2020) are in agreement with this. The physical basis for this is the strong dielectric contrast between the snow and the sea ice.

Regarding the differences between MYI and FYI, it likely that an FYI-snow interface will have a higher normalised radar cross-section (and would therefore be penetrated less). This is because (as alluded to by the reviewer) the density, salinity, and therefore permitivity of the ice is higher relative to that of snow or air. We do however note that due to the process of upward brine migration into the snow from the FYI surface, an ‘impedance matching’ effect can be produced (Perovich et al., 1998) which lowers the dielectric contrast at the ice-snow interface. However the strength and prevalence of this effect are not well understood.

Lines 109/110: "as quadratic fits ... without corresponding fits of density." –> I don’t agree. Warren et al. (1999), page 8, writes about 2-dimensional density fits. Yes, only a May map is shown but maps are derived for all months. And it is the SWE which is computed, not the other way round. Please rewrite this paragraph accordingly.

The reviewer is right to highlight that a quadratic fit for May snow density is shown in Fig. 10 of Warren et al. (1999), and it was density that was measured in-situ, not SWE (as shown in Fig. 2 of Warren et al., 1999). We would like to state that we have conducted an extensive search for the original density fits (one of which is displayed in Fig. 10 of Warren et al.), but have not been able to find them. This search included contacting the first two authors of Warren et al. 1999 and going through the Fetterer and Radionov (2000) data hosted publicly by the NSIDC (which only contain fits for SWE and depth). We also received additional, raw data on snow line and stake data from the NSIDC in a private communication which did not include the quadratic density fits. But it is clearly the case that the fit for May is published in Warren et al., so we revise our wording to: “quadratic fits of density … are not publicly available for all months”.

Lines 121-124: I suggest to provide information whether and to which extend this modification of W99 has been implemented by follow-on studies, e.g. Tilling et al. ? Kwok and Cunningham 2015 ...

We have elaborated on this in the text:

> Mainstream CS2 thickness retrieval products have generally used this approach since (e.g. Tilling et al., 2018; Kurtz et al., 2014; Hendricks et al., 2018). Kwok and Cunningham (2015) also investigated the effect of multiplying by a factor of 0.7 (rather than 0.5) with some success.

Lines 126-127: "are not currently used in sea ice thickness retrievals" –> I am wondering whether these variabilities are input into the uncertainty estimates provided alongwith the ESA-CCI SIT products? It might be worth to check.

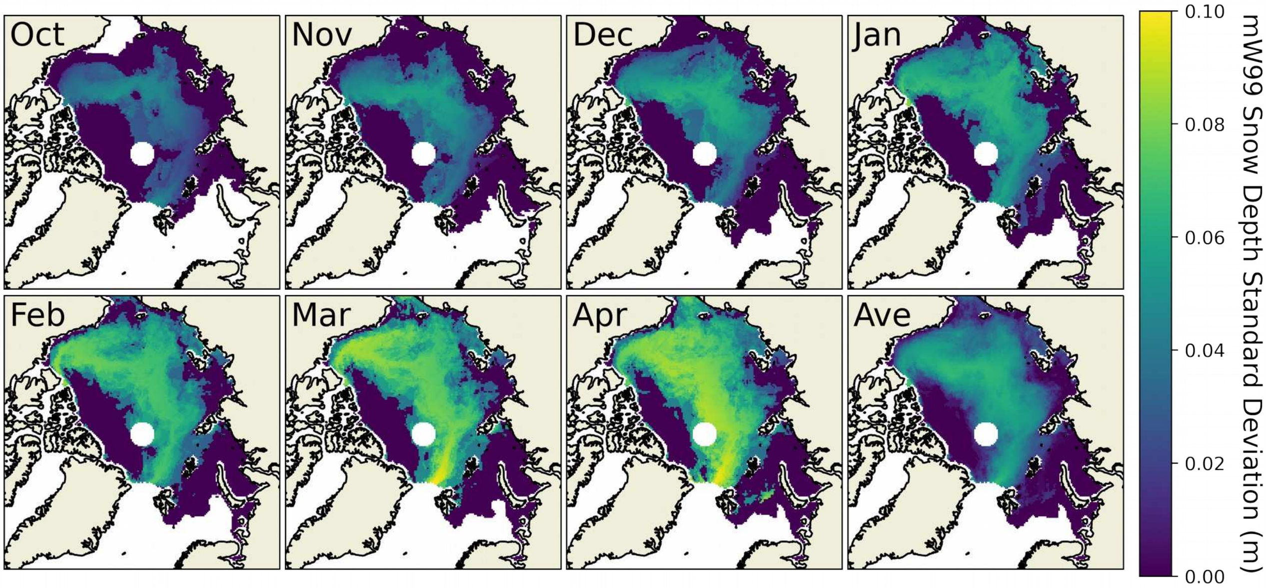
This information is contained in the D2.1 Algorithm Theoretical Basis Docuement for the ESA-CCI freeboards (Sect. 2.9.4). The IAV values do indeed contribute to the uncertainty estimates provided with the ESA-CCI product and we have added this information to our manuscript.

Lines 130-133: "As such, ... values." –> Will this described in more detail in section 2? No it will not. Are positions of real drift stations used? If yes, which? If not: Isn’t taking into account ALL ice covered grid cells of a 25 km grid providing a substantially different statistics - compared to the few drift stations used in the W99 climatology (their Fig. 1)? What is the time period considered? The description of this analysis step is lacking key details and should be re-written.

We have comprehensively rewritten this section to clarify. We now stress that the IAV values presented in W99 represent **the mean of many (positive) IAV values from individual, point-like drifting stations**. To compare mW99 and SnowModel-LG values to these in a rigorous way, we therefore must similarly take **the mean of many (positive) IAV values from individual points**. It would be wrong to take the IAV of the mean value of many points (which may each have cancelling positive or negative anomalies). This is a nuanced distinction that was not fully spelled out, but we believe to now be remedied. We also note that the logic above does not hold regarding the trends observed at drifting stations (because individual drifting station trends are not always positive), but because none the trends reported in Warren et al. (1999) are statistically significant we have not reported or visualised them in our manuscript.

Figure 2: Fig. 2 could be connected more easily to Fig. 1 if you’d use snow depth instead of SWE. It is not entirely clear how these maps are derived.

We have now changed this figure to snow depth and added clarifying information in the caption and the main text (see below):



Caption: “Snow depth variability at each EASE grid point over the 2002-2018 period. This is calculated by generating a timeseries of snow depth at each point and then calculating the standard deviation of that timeseries. High variability is displayed in a band where sea ice type typically fluctuates from year to year. IAV is zero in areas that do not exhibit sea ice type variability, introducing unphysically low variability in SIT in these areas.”

I note that the ice edge in the Kara / Barents Seas looks a bit weird for months DEC and JAN.

These sharp edges were formed by an unexpected bug involving interaction between the “zero-line” of the W99 quadratic fits and our masking technique in these two months. A small change of a boolean ($x<0 \rightarrow x<=0$) has fixed this.

Line 136: "this results" –> Which? Not clear to what this refers.

We have changed this to:

> “We present this analysis of the point-like snow variability to illustrate that mW99...”

Line 138: "where the ice type typically varies from year to year" –> How did you define this?

We have clarified this with the following sentence:

> “This band represents areas where the ice-type is not typically either FYI or MYI. Instead it is either switching between the two, or it is an area where FYI has replaced MYI during the period of analysis.”

Lines 157-162: "We find ... (S3)." –> One question upfront: What is exactly the region you are considering here? The central Arctic Ocean? Laptev Sea included? Kara Sea?

In the manuscript we stated (L155):

> “We instead compare the trends in basin-wide snow depth...”

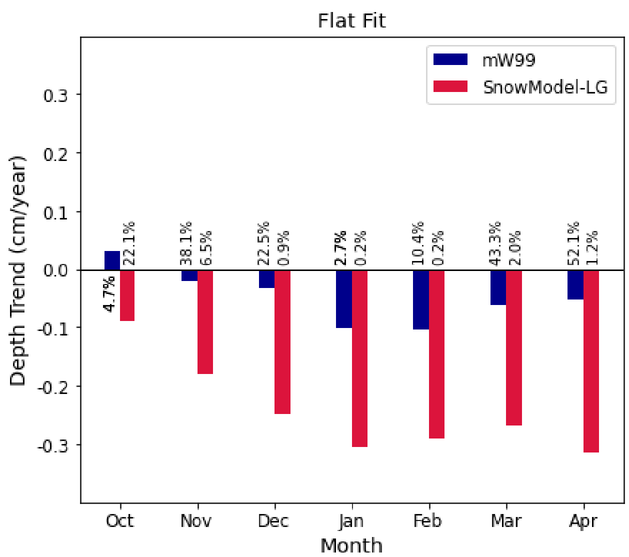
But we appreciate it may have been unclear what we meant by “basin-wide”. We have therefore added a set of clarifying parentheses so that the sentence now reads:

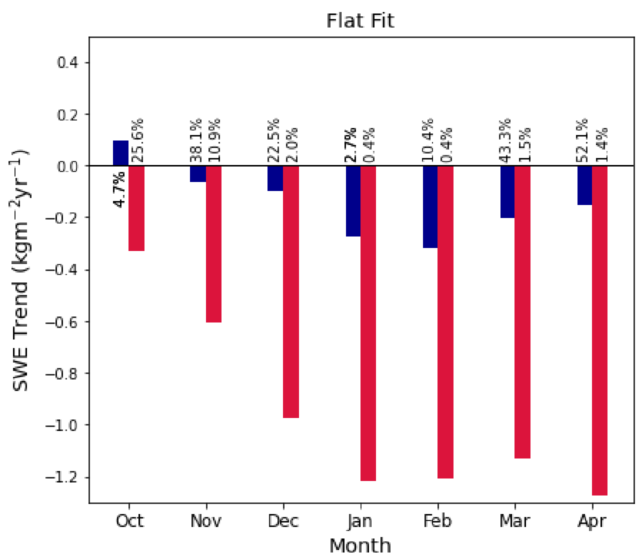
> “We instead compare the trends in basin-wide (all shaded regions of Fig. 1) snow depth...”

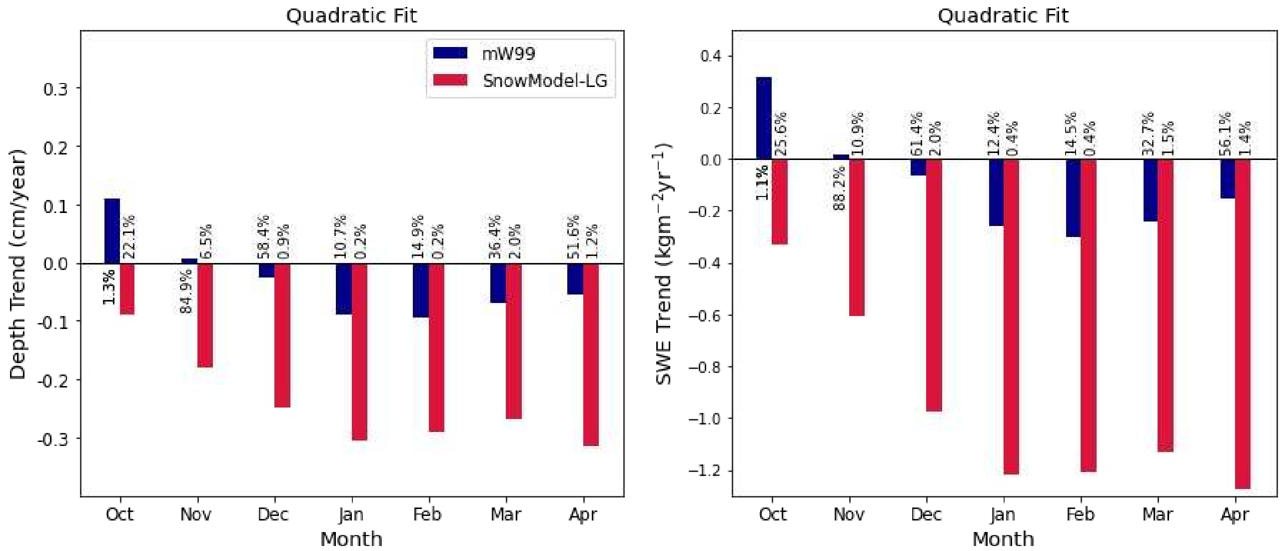
I am just asking because at a certain point in winter the entire region considered should be ice covered and the FYI fraction hence be only a function of the MYI extent. I buy that there is a the decreasing fraction of FYI relative to the total ice extent in October due to later freeze-up. I don’t agree, however, that this is the sole reason for your observation with respect to the trend mW99 snow depth and SWE. I believe an issue to consider is that the MYI coverage retreats more and more to those regions where W99 has maximum snow depth. Hence the relative fraction of MYI grid cells with comparably thick snow is increasing which to my opinion can result in a higher mean W99 snow depth (and SWE) for the MYI part of the sea ice in October.

This is an interesting point that we had not considered. To investigate the effect of the ice retretating into a region of high W99 snow depth, we repeated our analysis with a ‘flattened’ W99 climatology. To do this we first calculated monthly basin-wide SWE and depth averages from the W99 climatology. We then assigned those values to every point in the basin, rather than using the quadratic fits. This has the effect of removing the areas of high snow depth/SWE. We then halved the snow depth/ SWE over FYI as before to produce an mW99 product without the quadratic fitting.

We then performed our trend analysis again and found that the October trends were lower in the ‘flattened’ case, indicating that retreat into a higher-depth region of W99 is indeed playing a role in the positive snow trends. We do however note that the October trends in SWE and snow depth are both still significant at the 5% level in the ‘flattened’ case.







We therefore modify our manuscript to say:

> "This increasing trend in snow depth is in part due to the diminishing area of October FYI relative to that of MYI (Fig. S4), and in part due to the retreat of the October ice into the Central Arctic where W99 exhibits higher snow depths and SWE."

Lines 164-166: "Several ... year to year. –> I don't find this formulation particularly clear. I find the "cannot accumulate snow from year to year" not to well chosen in the light of mostly complete snow melt during summer - also on multiyear ice. I'd state that there are two reasons for the observations with SnowModel-LG: 1) The MYI area shrinks. Hence your sentence about "a lower [smaller] ice area is exposed to snowfall in September/October fits well. 2) Freeze-up commences later, hence new seasonal ice either has not yet formed or is too thin to carry / accumulate snow resulting in a substantial amount of the precipitation falling as snow being dumped into open water. In short also here your sentence about "a lower ice areas ..." applies, meaning you can, to my opinion, delete that extension "also the later ... year."

We agree with this observation and have changed the manuscript to the following:

> "We identify two processes as responsible for this decreasing trend: the MYI ice area is shrinking, so a smaller MYI sea ice area is present during during the high snowfall months of September and October (Boisvert et al., 2018); also freeze-up commences later, so a lower FYI area is available in these months and more precipitation falls directly into the ocean"

Line 167: "Webster et al. ..." –> I am wondering whether the "in situ sources" mentioned in the context of Webster et al. (2014) are i) also representing FYI and ii) aren't complemented with information from airborne operation ice bridge data [in which case these are not "in situ" anymore]. Please check! If their data indeed represent FYI and MYI then it might be worth to mention that explicitly in your manuscript.

This comment has led to our revision of this sentence. In particular, we highlight that the figure cited from Webster et al. (2014) includes raw data from the NP stations, so cannot be seen as independent of W99. We have also ammended the sentence to highlight that airborne measurements contributed significantly to the figure.

> "Webster et al. (2014) observed a -0.29cm/yr trend in Western Arctic spring snow depths using both airborne and *in situ* sources. This airborne contributions to this figure included data over both ice types, and the in-situ contributions included data from individual Soviet drifting stations from the Western Arctic."

Lines 187-189: "Where sea ice ..." –> Could you please comment on whether this second data set is similar to / consistent with the OSI-SAF one? What is the basis? Given the fact that you investigate quite short time series in your paper and put quite some weight on different ice types it is important that thes two data sets are consistent to each other, i.e. provide a seamless continuous spatial FYI/MYI fraction distribution without a jump in total regional FYI and MYI extent from, e.g., Feb 2005 to March 2005.

The OSISAF-403c Global Sea Ice Type data is an operational product that is only available since 2005. Unfortunately the NSIDC product (which spans a longer time period) is not suitable because of its weekly time resolution (see next point), so we wanted to use this product as our primary source of ice-type data.

To extend our ice-type data for three years to reach the beginning of the radar freeboard timeseries we used the Copernicus Sea Ice Type Climate Data Record. Since our original submission this data has been relocated, the documentation has improved and it has been assigned a DOI (10.24381/cds.29c46d83). This documentation (here) includes a dedicated appendix (App. B) summarising the main differences between the OSI SAF operational product and the C3S product. The C3S product's underlying algorithm is adopted from the OSISAF operational processing chain, but then modified to produce a consistent record compatable with reanalysis.

The other key difference is that the OSISAF record uses active sensors such as the ASCAT scatterometer whereas the C3S product is confined to 'passive' satellite radiometers. This allows the OSISAF product to be delivered at a higher spatial resolution. This advantage is generally lost in our analysis as we would downsample to a 25km grid for compatability with our radar freeboard and SnowModel-LG data. But the reviewer is absolutely right to ask (a) whether these differences affect our study and (b) is it appropriate to switch from C3S to OSISAF when the latter becomes available for the winter of 2005?

We compared the two ice-type datasets over the period 2005-2019 and found the differences to be very small (once we'd classified the ambiguous pixels). Nonetheless we now opt to now use the C3S dataset over the entire period of our study. This is because our study is based around trends and variability, so a consistent record is of particular importance. We found after switching that this had a minimal impact on our results, and removes the need for a detailed assessment of (a) the abrupt transition from the CDS to the OSISAF product and (b) the impact on trends from the transition over the period.

We have rewritten the description of our ice type data to reflect this information.

I note that you also use NSIDC ice-age data and one could ask the question: why didn't you use ice-age data throughout the entire study?

We do not use the NSIDC ice-age data in this study. The only NSIDC product cited here is the concetration data that is used to drive the NESOSIM model. Although the NSIDC product has a long time span (1984-2019), it is only available at a weekly temporal resolution. This makes it unsuitable for use in conjunction with the monthly data from the ESA-CCI product. This unsuitability stems from ambiguity about how to split certain weeks of data that span two months.

Section 2.3: Please comment on two issues.

1) The sampling on which W99 is based has large regional variations with substantial differences between marginal seas such as the Kara or Barents Seas compared to the central Arctic. How does the lack of reliability of W99 in these partly undersampled regions influence your results - particularly in the two regions mentioned above?

Our response above to GC2 highlights the exceptionally poor (and sometimes non-existant) sampling of the Kara, Barents and Laptev seas by Russian drifting stations. This uncertainty undoubtedly propagates into W99 and mW99. However, this has not stopped sevaral groups from using W99 to derive sea ice thickness in these marginal seas (e.g. Li et al., 2020a,b; Belter et al., 2020) particularly in the pursuit of estimating sea ice volume (Tilling et al., 2015; Laxon et al., 2013).

We do not argue here that using (m)W99 in these regions is an appropriate approach, but simply point out the difference in trends and variability of SIT when calculated with the two different methods. We also point out that even if the climatology has an absolute error in these regions, it does not significantly affect our findings regarding variability and trends (which are both low or effectively zero in (m)W99 regardless of the snow depth value itself). We have now included this argument in our manuscript (see GC2 response).

2) Snow depths / SWE in the Kara / Barents Sea do - for the same reason - depend a lot on the extrapolation / fit function used. Howdoes this influence your results?Aren't the snow depth values in these regions too hypothetical to be adequately used in your study? Wouldn't it make sense to exclude the Kara and Barents Seas? To my opinion it would make your study considerably more credible. And it would potentially safe some space.

The extrapolation/fit function is indeed highly uncertain in these marginal seas, although this does not affect our central arguments concerning trend and variability. This is because the trends and variability in the implementation of (m)W99 are not sensitive to the extrapolation/fit functions, but rather the lack of trends that stem from the climatological approach.

Section 2.4: Isn't the EASE grid a polar aspect of the Lambert Azimuthal Equal area grid? I'd suspect no re-gridding is required.

The reviewer is correct that EASE is a polar aspect of the Lambert Azimuthal Equal Area projection, and our description of the data was confusing in this respect. Both the ESA-CCI freeboards and the SnowModel-LG data are supplied on a 25 km EASE grid. The ESA-CCI data were previously being subjected to a "regridding" process that did not change the positions of the relevant coordinates at all (but did remove low-latitude 'nan' datapoints to make the grids the same dimension). This regridding process has now been removed from our processing chain (with no impact on the analysis).

What is the grid resolution of the radar freeboard data?

As mentioned above, it supplied on a 25x25 km EASE grid, and this is now mentioned in our data description section.

What is the time period (years, months of the year) for which these data are available and used by you?

The radar freeboard data is available from the CCI website from October in the winter of 2002/03 until then end of winter 2016/17. However the CS2 radar freeboard component of the CCI product is functionally identical to the radar freeboard product made available by the Alfred Wegener Institue (Hendricks and Ricker, 2019). We have checked this manually for the period of overlap. We were therefore able to extend the timeseries of radar freeboards for one year until the end of winter 2017/2018, which is the end of our SnowModel-LG data series. All of these details have now been added to the Data Description of our revised manuscript.

How did you treat the overlap of Envisat and Cryosat-2?

A description of this was originally omitted in our manuscript. We direct the reviewer to our response above to GC2 where we have answered this question.

Key information is lacking here. It might also be worthwhile to take a look into the validation report of the SIT / freeboard data set used (see e.g.: https://icdc.cen.uni-hamburg.de/fileadmin/user_upload/ESA_Sea-IceECV_Phase2/SICCI_P2_PVIR-SIT_D4.1_Issue_1.1.pdf). It provides some information about how "consistent" the two "merged" data sets are. Taking this information into account and discussing the potential biases (which still exist) I rate mandatory for a paper which so much relies on the analysis of this 17-year long time-series with a change of sensor right in the middle of the time series.

We note first for readers of this review that this document is now found here. We have now added relevant information to Section 2 on how the Envisat retracker threshold is varied to match CS2 in the period of overlap, as well as a citation to the PVIR document and some information on the relevant biases.

We have also added the following subsection to our Discussion:

> "**Inter-Mission Sea Ice Type Bias between Envisat and CryoSat-2**
>
> An extensive validation exercise for the merged products indicated that although EnviSat radar freeboards match well with CS2 freeboards in the Arctic overall, some biases do exist over specific ice types (ESA, 2018). In particular, analysis of the inter-mission overlap period indicates that Envisat freeboards were biased low (relative to CS2) in areas dominated by MYI, and high in areas dominated by FYI.
>
> We first make the point that this will have a relatively minimal effect on our findings regarding interannual variability, as $\overline{Snow}$ is unaffected by this and σ^2_{RF} is likely relatively independent of the absolute magnitude of $\overline{RF}$.
>
> With regard to trends, if Envisat radar freeboards (and thus $\overline{RF}$) are in fact biased high over FYI between 2002-2010 (relative to CS2), then the total trends in many regions dominated by FYI could potentially be smaller than calculated in this manuscript.
>
> We do however add that our central findings regarding the impact of declining $\overline{Snow}$ is unaffected by any inter-mission bias in $\overline{RF}$. Because the trend in $\overline{SIT}$ is determined by both $\overline{Snow}$ & $\overline{RF}$, the trend in $\overline{SIT}$ will always be more negative when calculated with downward trending data for $\overline{Snow}$."

Line 206: "ice motion vectors" –> Which ice motion vectors? Please provide this information - including the temporal resolution and the version of that ice motion data set used - in your manuscript.

The ice motion vectors used were v4 of the polar pathfinder ice motion vectors (Tschudi et al., 2020). These are supplied on the 25x25 km EASE grid and were analysed at weekly time resolution. This information has been included in our revised manuscript.

In addition: "pan-Arctic snow depth and density distributions" –> Please provide a spatial and a temporal resolution as well as the domain. While the paper focuses a lot on snow depth I am wondering how snow densities obtained with SnowModel-LG compare to W99 ones?

The SnowModel-LG output is supplied on the 25 km EASE grid at daily time resolution. From this we produced monthly gridded fields for combination with the monthly radar freeboard data. This information is now included in our revised manuscript.

Line 210: "snow-ice accumulation" –> Please explain what "snow-ice accumulation" is. Do you refer to snow-ice formation at the basal snow layer?

We were actually referring to superimposed ice and used the wrong term. Snow-ice in its conventional meaning results from flooding of the base of the snow by depression of the ice surface below the waterline. This is not modelled in SnowModel-LG, as the model does not model or assimilate ice freeboard. We have removed this reference to snow-ice from our revised manuscript.

Lines 216/217: "snow depth differences ... than 5 cm" –> This is a quite global statement. Is this an Arctic mean value? Is this the mean difference in SnowModel-LS realizations just for the grid cells co-located with the OIB data? What is the standarddeviation of this difference?

In our submission we stated:

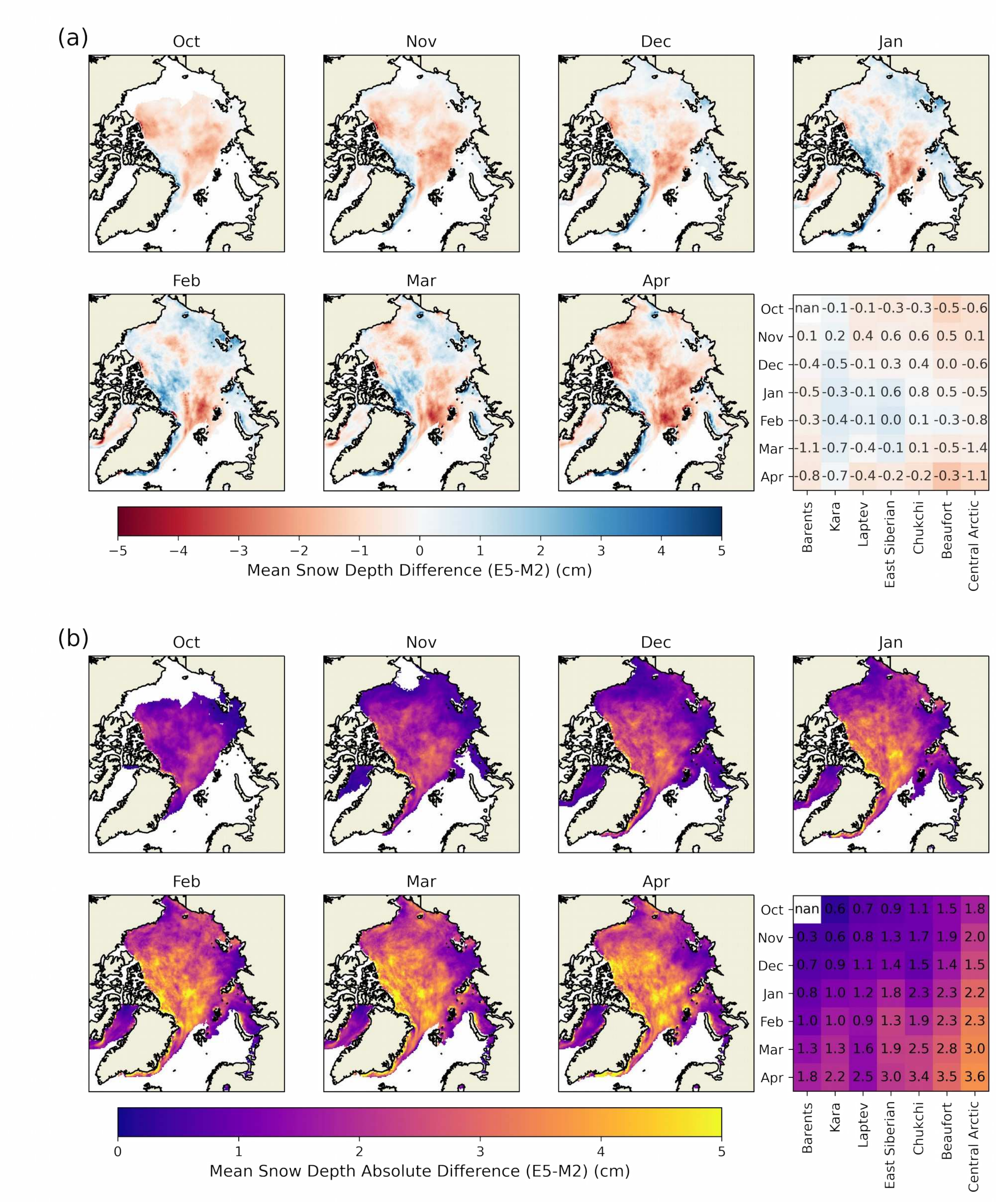
> “snow depth differences between the reanalysis products were found to be less than 5 cm”

By this we were referring to the difference in SnowModel-LG snow depths when forced with MERRA-2 and ERA5 reanalysis data. We have clarified this in our revised manuscript to:

> “snow depth differences between the ERA5 and MERRA2 reanalysis products were found to be less than 5 cm”

The 5 cm figure represents the Arctic as a whole and is illustrated by Figure 4 of Stroeve et al. (2020). The standard deviation of this value was not reported by the authors. We investigate this here.

We first visualise the “mean bias”, which we calculate as the difference in the snow depths calculated when SnowModel-LG is run with ERA5 and MERRA2, averaged over several years. In this scheme, if the ERA5 run is larger in one year and smaller in another, this cancels out. This indicates the degree of *bias*. We also calculate the average absolute difference. For this, we subtract MERRA2 SnowModel-LG output from that of ERA5, and then take the absolute value. We then plot the time-mean of this distribution. We also summarise these data by region and by month in the lower right hand panels of the below figures:



We place this figure in the supplement and reference it from the main mauscript.

Do the OIB data used to tune SnowModel-LG represent FYI conditions adequately?

Figure 7 of Stroeve et al. (2020) illustrates the locations of the OIB flights used to tune SnowModel-LG. The vast majority are over the Beaufort Sea and the Greenlandic side of the Central Arctic, which is generally consituted by multiyear ice. It is therefore conceivable that the scaling factor would be different if FYI were better sampled by OIB. We have raised this point in our revised manuscript.

We do however believe that the scaling factor is not paticularly relevant to our central observations concerning trends and variability. It is certainly the case that if the scaling factor is off then we are under/overestimating snow and sea ice thickness. However the impact on this on trends and variabiliy is potentially very small.

Section 2.6: For a better understanding it might make sense to explicitly state whether precipitation and/or snow fall are assimilated into NESOSIM as well.

It does assimilate reanalysis precipitation directly and we have now explicitly stated this.

The snow pack initialisation, is this covering both snow depth and density? As W99 data are monthly values, is this initialisation only done monthly, or are monthly values interpolated to daily values with which the model is initialised henceforth?

In this study we use data from a NESOSIM run initialised on the 15^{th} August for each year (Sect 2.1 Petty et al., 2018). The initial depth was produced by a “near-surface air-temperature-based scaling of the August W99 snow depth climatology”. This is a linear scaling based on the duration of the preceeding summer melt season. Snow density was initialised using the August snow-line observations of Soviet NP drifting stations 25, 26, 30 and 31. Data from the most recent publicly available drifting stations were chosen to maximise their relevance in a changing climate. We have ammended our draft manuscript to include this information.

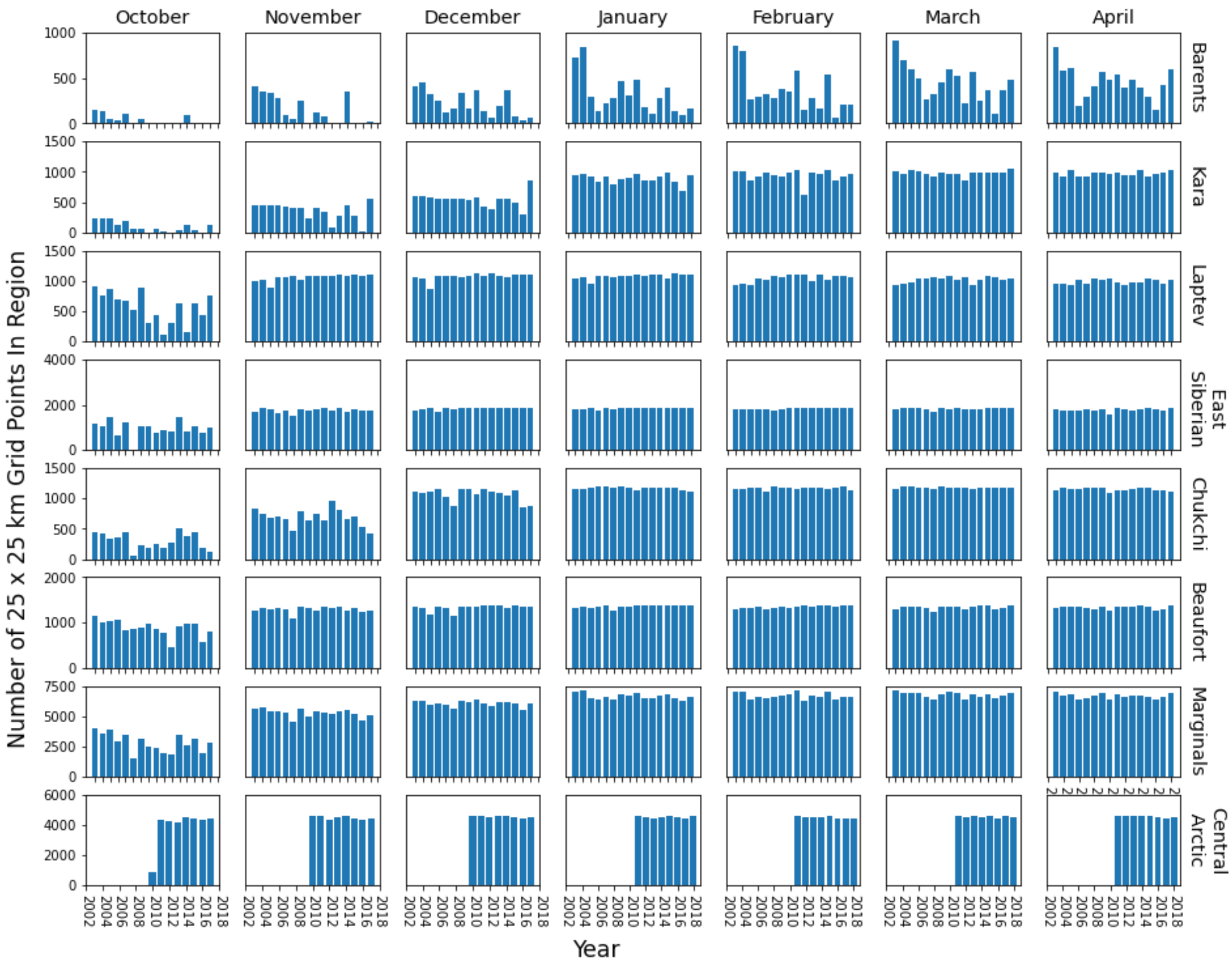
You explicitly mention depth-hoar and wind-packed layers in the context of NESOSIM. Does this imply that SnowModelLG does not represent such features? If not, then I suggest to be more specific in the description of what SnowModel-LG can do and what not.

SnowModel-LG does include these aspects of snowpack evolution in a multi-layered scheme. We have added a line in the SnowModel-LG descrition to reflect this, and also modified our subsequent description of NESOSIM to draw a direct comparison.

Section 3.2: Please provide more details. How many grid cells with valid SIT measurements are requied to compute a regional mean SIT value?

(related comment) How many valid observations are required for the results broken down into ice types (see Figures S4 and S5)?

$\overline{\text{Snow}}$, $\overline{\text{RF}}$ & $\overline{\text{SIT}}$ were calculated when any grid cells were present in a region, and we have now added this information to our manuscript. We have also now conducted analysis on the number of measurements that feed into our analysis. Although the number of snow and radar freeboard data points in a region were closely related (as both are tightly coupled to the ice area in the region), we found that there were generally fewer radar freeboard measurements. We have visualised this below:



We add to our manuscript:

> “$\overline{\text{Snow}}$, $\overline{\text{RF}}$ & $\overline{\text{SIT}}$ were calculated where any valid grid points existed on the 25x25 km EASE grid. Because of this, no average values were computed in the Kara Sea in October 2009 or 2012. Furthermore, no October values were generally available in the Barents Sea after 2008 (with the exception of 2011 and 2014). The impact of this on our analysis is clearly visible in Fig. (10).We do not exclude the Barents Sea in October from our analysis because of the low number of valid points, but we do highlight the undersampling issue here. We continue to consider it because we do not find statistically significant declining trends with the data we have, so essentially we are reporting a null result. Our calculations of interannual variability in this month is inherently adjusted for the small sample size, but we nonetheless urge caution in interpretation of the value.”

How about regional means of radar freeboard and snow depth / SWE? Did you compute these as well?

These were calculated during our analysis and plotted alongside each other as a figure in the supplement. We have now added an explicit reference to this figure to this section.

Please provide a reference for the "Wald test".

Although we were correct to use this term, its use (and technical definition) are not particularly enlightening for the reader. Instead we have opted now to just describe the test as a hypothesis test (and state the null-hypothesis).

Line 249: I suggest to stress here once more what "Snow_overbar" is, that it is not the snow depth but the snow-depth contribution to the SIT retrieved from altimetry

We have reiterated the definition here.

Line 259: "individual years, regions and months" –> Not clear what you did. You used detrended time-series of monthly, region-mean values of RF and snow and computed the correlation between these time series separately for every month and every region?

Yes, this is correct. We have added the following text to clarify this:

> To do this we calculated a monthly timeseries of $\overline{RF}$ and $\overline{Snow}$ for each region over the time-periods (2002-2018, with the Central Arctic being 2010-2018). Because we considered eight regions and seven months, this led to to 56 pairs of timeseries for $\overline{RF}$ and $\overline{Snow}$. We then detrended each of them. We then calculated the correlation between each of the pairs of detrended timeseries.

Lines 263/264: "The Barents Sea ... correlation." –> I suspect this observation is based on two completely different causes. For the Central Arctic the time series is just 9 years long. For the Barents Sea, neither is mW99 overly reliable nor are RF values overly reliable - especially during the Envisat period.

> We have added a brief clause to this line: "- the reasons for this are discussed in Sect. (5.4)".

Another, more general comment: The RF data for region Central Arctic are considerably more robust in terms of the number of valid observations contributing to the RF values used.

Figure 6: Not clear what is shown magnitude-wise on x- and y-axes. The same applies to Figures S6 and S7.

Because correlation statistics are not sensitive to the choice of axes, units or linear scalings of the values, we decided to not display axes ticks or labels and scale the axes to fit the rectangular panels of the figure. However we clearly should have stated this in our submission and we now have added the following text:

> "We note here that the correlation between the timeseries is dependent on their relative position to a linear regression. These correlation statistics are thus independent of the absolute magnitude of the values, their units, or any linear scaling of the axes. We therefore choose to present the correlations in Fig (7) without axes and scaled to the rectangular panels, so as to best show the relative positions of the points without extraneous numerical information."

Line 286: "but analysis ... regions." –> Not clear what you mean here.

We have reworded this sentence for clarification:

> "but analysis of this grouping conceals more significant variation at the scale of the individual group members"

Line 287: "The covariability ... contribution" –> This discussion focuses on radar freeboard. It does not comment on the observation that at the beginning of winter (Oct.) the fraction of SIT IAV that is explained by RF-Snow covariance is larger than snow IAV.

We have added the following text to include this information:

> We note that in contrast to $\sigma^2_{\overline{RF}}$, $\sigma^2_{\overline{Snow}}$ is almost always larger than the covariability component. A noticeable exception to this generalisation is in October for the Marginal Seas grouping, where the covariability contribution to $\sigma^2_{\overline{SIT}}$ is around twice as large as the contribution from $\sigma^2_{\overline{Snow}}$

Figure 7: Can you please check whether your representation of "Fraction of Total Variance (%)" is correct? I mean, ok, if the dimensionless factor rho is negative then the covariance term in Eq (5) gets a negative sign. Therefore you plot negative bars in panels (b).

We gave the visualisation of the data in Figure 7 considerable thought, but agree that a succinct characterisation of what is actually being plotted on the y axis is challenging. It is possible that "Fraction" is not the best word, as it carries connotations of being less than a whole. Given that the quantity on the y axis is the normalised contribution to $\sigma^2_{\overline{SIT}}$, then we propose that this is put as the axis label to minimise confusion.

However, looking at the Central Arctic, November, this results in a fraction of radar freeboard IAV of about 110%, also the one for October is larger than 100%. I get a headache with this because a fraction cannot be negative (have you ever had a negative piece of cake?) and it can also not be larger than 100%, i.e. larger than the total (only if you order a medium size Pizza and get a large one instead). This applies then also to Figure S15 where the deviations from 100% are even larger. Again, I can see from Eq. 5 that it is mathematically correct. However, a positive covariability means that sigma_RF and sigma_snow are positively correlated while a negative one means that these quantities are anti-correlated. If we assume a very strong negative covariability of, say -90%, does that mean that the IAVs of RF and snow need to sum up to a fraction of the total of 190%?

We consider the more intuitive case of apples and oranges being delivered in a truck. If the number of apples is determined randomly and truck space is limited, it is the case that when more apples are delivered then less oranges are delivered. This corresponds to a strong negative covariability between the n_apples and n_oranges – they are random, dependent variables just like RF and Snow. For the case that 100 extra apples delivered means that you lose the space for 90 oranges, then the variability in n_apples or n_oranges individually is many times larger than the variability in the total number of pieces of fruit. If your standard deviation in n_apples = 500, then your standard deviation in n_oranges is around 450, but the standard deviation in total pieces of fruit is just 50. This case illustrates that the $\sigma^2_{\overline{RF}}$ and $\sigma^2_{\overline{Snow}}$ can logically be much larger than the standard deviation of the sum of RF and Snow (so long as their covariability is negative).

Lines 327/328: "Perhaps more significantly, ..." –> This I don't find too convincing - also given the unknown uncertainty of these regional mean SIT values. I suggest to only mention these three new trends but do not hypothesize about the main reason.

We have removed our hypothesis about this being driven by the years 2003 and 2004.

Line 350: "... truncated SIT distribution ... thicker ice." –> This relatively global statement is not supported by Fig. S10 for all months.

We have now qualified this statement by adding “in the months January – April”.

Particularly, I would not use the word "truncated". Truncated means that below or above a certain SIT values the area occupied by these SIT bins is abruptly zero

We have now removed all uses of the word “truncated” from our manuscript. In the case of the CS2 observational period, we have used the word “shorter”. In this case of the distribution of sea ice thickness distributions, we have used the word “narrower”.

Lines 351/352: Please see my comment at Figure S10: You need to provide more details about how you derived this Figure. What is missing are binsizes and borders as well as the time-period for which the Figure is valid (2010-2018 I assume) as well as a statement here that this Figure is now showing a classical pdf but expresses the distribution in form of sea-ice area. In order to avoid confusion with the classical definition of sea-ice area which is sum of the area of ice covered grid cells weighed with the actual sea-ice concentration, you might want to rename your y-variable.

Line 353: "The regional, seasonal growth rate ..." –> What is the period considered?

We’ve added the following sentence to clarify this:

> “These rates were calculated over the period 2002-2018 with the exception of the Central Arctic which was restricted to the period 2010-2018.”

Lines 372-374: I suggest to refer to the Boisvert et al. (2018) paper here (about the difference between Merra-2 and ERA-Interim) and in addition take into acount this paper: https://doi.org/10.1029/2019GL086426.

We have now added the following text:

> “Boisvert et al. (2018) conducted a similar analysis with drifting ice mass balance buoys, and found the interannual variability of the data sets to also be similar (although the authors found larger discrepancies in magnitude). These differences in magnitude however cannot be physical (as there is only one Arctic), and Cabaj et al. (2020) were able to bring precipitation estimates into better alignment using CloudSat data with a scaling approach. However this scaling approach preserved the interannual variability of the data sets, which Barrett et al. (2020) and Boisvert et al. (2018) found to be in comparatively good agreement.

I am wondering whether Figure S12 is required. After all it confirms that your choice combining SnowModel-LG runs of ERA5 and Merra-2 is a good one.

We agree that Fig. S12 is not strictly required, although we would prefer to keep it in the supplement (as were the reader to not see it, they may doubt our choice).

Line 387: "replicates the higher contribution ..." –> Even though for the Central Arctic the data are just based on the period 2010-2015 = 6 years.

The word “replicates” does perhaps imply that the data are more directly comparable than they are. We therefore explicitly note the timeframe difference and change “replicate” to “also exhibits” as such:

> “Despite the shorter timeframe, NESOSIM also exhibits an increasingly … and also a higher...”

Lines 390/391: "underlying trends ... period" –> Ok ... but at the same time you use a shorter time period for the Central Arctic anyways. So this argument is not conclusive.

In response we add:

> “(by comparison to regions where all relevant data is available from 2002-2018)”

Lines 394-399: "As such ... 2018)." –> I suggest to shorten this part and perhaps either delete Figure 11 or move it as well to the Appendix. - which however, already contains a lot of additional material. But I feel that the paper would be still understandable with a few sentences highlighting the common findings between NESOSIM and SnowModelLG

> We have removed this paragraph and the figure.

Line 427-428: "... these investigations ... warmer temperatures ..." –> I do not quite agree to this statement because, according to my knowledge, among the cases investigated and presented in Nandan et al. (2017) as well as in the later paper by Nandan etal. (2020) is a sufficiently high number of cases with cold snow; hence the observation of a rising scattering horizon is not uniquely tied to warmer temperatures.

In response to this comment we have modified this statement:

> “However, these investigations were **often (but not exclusively)** carried out at the end of the winter season or in the Sub-Arctic, when warmer temperatures **may have increased** the snow's brine volume fraction...”

Lines 434-440: "Knowledge ... depth." –> I can follow the physical reasoning here and also how it is connected with Equation (2). However, it might not be as straightforward as it is formulated, given the fact that equation (2) transforms into equation (4) in Kwok and Cunningham (2008, https://doi.org/10.1029/2008JC004753) for a lidar - hence introducing a different factor in front of the snow depth. Perhaps formulating these lines more like being your own hypothesis than being fact might be more appropriate?

To improve the rigor of this section we have elected to remove the following sentence:

> “This incidentally raises the possibility that radar waves with a certain relative penetration depth may allow the estimation of SIT without requiring any knowledge of the snow depth.”

We hope that by doing this we remove the hypothesis-based element of this part of the discussion.

Line 443: In the context of the paragraph ending here you could also comment on the impact of i) the increased likelihood of a flooded basal snow layer in regions of comparably thin sea ice / thick snow / high SWE as, e.g. the Barents Sea and part of the region Central Arctic facing the Atlantic and ii) of a potentially increased likelihood for enhanced snow metamorphism, like is typical for the Antarctic, even in the middle of winter due to intrusions of marine air-masses into the Arctic, e.g. via the Fram Strait.

We have addressed these issues with the following paragraph:

> “We finally note the potentially confounding influence of negative freeboard in regions such as the Atlantic sector of the Central Arctic region and the Barents Sea. In the case of high

snowfall and low ice thickness, the ice surface can be depressed to the waterline or below. Beyond this point Eq. (5) no longer function. The prevalence of negative freeboards has been studied by Rosel et al. (2018) and Merkouriadi et al. (2020), but has yet to be incorporated into any radar-altimetry based sea ice thickness retrievals. This situation can be driven by storm tracks entering the Arctic from the Atlantic (but also the Bering Strait). These intrusions of warm air can also drive snow grain metamorphism, which may well affect radar penetration through the snowpack."

Line 469: "in the high precipitation months ..." –> One could argue, though, that thanks to later freeze-up and the concomitant change in atmospheric moisture content (and perhaps also circulation) also shifts the maximum of the precipitation to a later time - and with that the seasonal sea ice would still be able to accumulate a fair amount of snow - particularly supported by the fact that a warmer atmosphere can hold more moisture and with that can lead to increased precipitation rates. This is hypothetical of course but among the possible scenarios.

Lines 504/505: "negative ... this is not seen." –> I thought the negative covariances between RF and Snow shown in Figures 7 and S15 are an indication of exactly this observation?! The fact that these only occur in early winter makes a lot of sense as well.

Yes, "this is not seen" was an oversimplification. We have ammended this to read:

"This corresponds to a negative covariability term in Eq. (5) and is represented by purple bars in Fig. (8). Negative values are generally not seen, with the exception of October and November in the Central Arctic, November in the Barents Sea and December in the Chukchi Sea."

Lines 505-507: "... snow is a highly ... weeks." –> I am wondering whether it wouldmake sense to to have a thought experiment to check whether the magnitudes of the the changes involved fit this hypothesis. While Snow_overbar can be easily computed based on the snow properties and is independent of the sea-ice thickness and its radar freeboard, RF_overbar is not. It is a function of both, ice growth and snow load. An experiment one could think of is, e.g. an initially 80 cm thick ice floe with i) 5 cm snow depth and ii) 20 cm snow depth (and similar snow densities) grows at -30 degC over a month. What is the RF for both cases at the beginning and what is the change in RF over the month? Without further snow accumulation Snow_overbar remains constant. How would the change in RF be modified if one would add another 5 cm of snow in the middle of the month and again at the end of the month? My hypothesis is that RF_overbar and Ssnow_overbar are correlated well in case a thin to moderately deep, slowly increasing snow cover allows adequate ice thickening so that Snow_overbar increases over time but RF increases as well over time. Further, my hypothesis is that RF_overbar and Snow_overbar are not well correlated in case an already thick snow cover hampers adequate ice thickening while it further deepens over time. In that case Snow_overbar increase over time like in the above-mentioned example, but the RF increase by increasing SIT and hence sea-ice freeboard is counterbalanced by an increasing radar range such that the increase is considerably slower than in the first example or even zero.

We found the reviewers comments of great interest, and they have clear bearing on the seasonal correlations between $\overline{RF}$ and $\overline{Snow}$ We point out that while thereis an immediate (theoretical) negative impact between snow accumulation and radar freeboard, this relationship is weakened by a negative feedback in which snow insulates theice and reduces subsequent growth. We believe that that this monthly-to-seasonal interplayof snow and radar freeboard is fertile ground for further study, and draw the reader's attention recent conference presentations by Lawrence et al on this topic, as well as previous modelling work by Stroeve et al. (2018) and Petty et al. (2018).

Lines 513-517: "Freeze-up ... further study" –> I doubt that for the months you consider this is an issue. Melt ponds in the Central Arctic begin to freeze over in mid August, latest in September; hence any snow falling in October falls on solid ice. I suggest to delete this part.

In response we have removed this section.

Typos / Editoral remarks

Lines 5/6 " ... with the conventional method ... " –> I suggest to tie this better the usage of a snow climatology.

We have ammended this line to do this

Line 91: I suggest to use a different letter for the dimensionless factor than rhoto avoid confusion with a density.

We have ammended this line to do this

Also "sigma_SIT_overbar" should possibly read "sigma_Snow_overbar"

Yes, this was a typo and has now been fixed.

Line 110: "radar speed" –> "radar wave speed" or "speed of the radar waves"

This line has now been restructured in response to another comment and “radar speed” no longer appears.

Line 118: If I am not mistaken, then Giles et al. (2008b) is purely dealing with the Antarctic, hence application of the W99 climatology appears to be unlikely.

Yes, we have removed this reference.

Line 119: I agree that Eq. (2) contains the snow contribution to the sea-ice thickness; however, the term Snow_overbar is used in Eq. (4).

Yes, we have now changed this.

Line 134: "average half of the value" –> In order to allow a more fluent reading I suggest to write "about 50%" instead of "average half of the value" in Line 134. That way it will be easier to connect this finding with the SnowModel findings.

We have now changed this.

Line 139 and 140: "consistent" –> What is a consistent ice type in this context? Could it be that you wanted say "constant" or "unchanged"?

We have changed “temporally consistent” to “temporally unchanging”

Line 187: "2005" –> "March 2005"

We’ve added this as suggested.

Lines 205: "assimilates reanalysis weather data" –> "is capable to assimilate meteorological data from different atmospheric reanalyses (see below)"

We’ve changed this as suggested.

Lines 214: "results of reanalysis" –> "representation of the actual distribution of relevant meteorological parameters by atmospheric reanalyses"

We’ve changed this as suggested (and added a citation to Boisvert et al., 2018)

Line 228: "Eq. (2)" –> Same issue as for Line 119 (see above)

We’ve changed this as suggested.

Line 238: "The Central Arctic region exists above the latitudinal limit of the Envisat orbit" –> "The Central Arctic region is not sufficiently well observed by the Envisat radar altimeter (see Fig. 4)"

We’ve changed this as suggested.

Line 255: "SnowModel’s" –> I suggest to always keep the full name.

We’ve changed this as suggested.

Line 257: "Having calculated ..." –> I suggest to refer to Figure 5 and the standard deviation shown therein.

We’ve added a reference to Figure 5 (detrended timeseries of $\sigma^2_{\overline{Snow}}$)

Line 260: "between" –> "between detrended"

We’ve added this as suggested.

Line 278: "in for each" –> "for each"

We’ve changed this as suggested.

Lines 286/287: "grouping by comparison to" –> "grouping in comparison to"

We’ve changed this as suggested.

Lines 287-289: I suggest to add "radar" to all mentioning of "freeboard"

We have been through each usage of the word “freeboard” and added “radar” where appropriate.

Figure 7 caption: For better readability of the figure and the text refering to it I suggest to somewhere re-introduce IAV here as the inter-annual variability. The last time IAV was used was in Figure 2.

We have reintroduced the term on L293.

I cannot see (a) and (b) in the Figure.

We've now added these annotations.

I suggest to write "panel" instead of "figure in Line 293.

We've made this change as suggested.

Line 294: "continuous" –> "contiguous"

We've made this change as suggested.

Line 296: "was the" –> "was in the"

We've made this change as suggested.

Line 306 "2002 - 2018": I suggest to add something like: "except for the Central Arctic: 2010-2018" in this caption as well as in all other figure captions where the Central Arctic region is shown along with results of the other regions.

We've made this change as suggested.

Line 315: "Fig. 9" –> I suggest to refer to the panels highlighted in green.

We've made this change as suggested.

Line 320: "declining" –> "negative"

We've made this change as suggested.

Line 324: "mW99" –> Suggest to refer to Fig. 9 and the red panels

We've made this change as suggested.

Line 325: "declining months to four" –> "months with a decline in SIT to four"

We've made this change as suggested.

Line 326: "after 2006" –> I suggest to write "after 2003, except 2006"

We've made this change as suggested.

Lines 333/334: "are only ones" –> "are the only ones"

We've made this change as suggested.

Line 337: "Laptev Sea." –> I suggest to add "when using SnowModel-LG instead of mW99."

We've made this change as suggested.

Line 340: "statistically significant months" –> When SIT is computed using SnowModelLG?

We’ve made this change as suggested.

Line 344: "snow" –> "show"

We’ve made this change as suggested.

Figure 9, caption: I suggest to add the notion that y-axes of Central Arctic and East Siberian Sea differs from all other regions.

We’ve made this change as suggested.

I suggest to make "Where trends are ... superimposed" the second sentence of the caption.

We’ve made this change as suggested.

I suggest to add a note that unlike all other regions Central Arctic data are based solely on Cryosat-2 data.

We’ve made this change as suggested.

Line 356: "Siberian there" –> "Siberian Sea there"

We’ve made this change as suggested.

Line 404: "Chukchi" –> "Chukchi seas"

We’ve made this change as suggested.

Line 405: "one month" –> "only one month"

We’ve made this change as suggested.

Line 427: "raise" –> "rise"

We believe that we have used the word ‘raise’ correctly here (with the difference being that *raise* is a transitive verb while *rise* is not – the object of the sentence being “the scattering horizon”).

Line 442: "diminishing show cover" –> I suggest to add: ", i.e. actual values of snow depth and SWE that are smaller than the climatological values"

In this paper we do not argue that snow depth or SWE are underestimated in an absolute sense by comparison to W99. Instead we argue that the rate of decline (which is important for trends) is not properly represented by W99. As such we do not feel that we can add this line.

Line 457: "EM" –> Has EM been explained already ...?

This abbreviation had not been previously defined and we have replaced it with “electromagnetic”

Line 489: "... truncated" –> Please re-phrase. It is not the radar altimetry time series that is truncated. There is a region where simply no measurements could be taken.

We have reworded this to:

“at best limited to the CryoSat-2 era”

Line 511: "has longer to" –> "has longer time to"

We’ve made this change as suggested.

Comments for the supplementary material:

Line 19: Please refer to Figure S1 here. Otherwise it is completely unclear how you ended up with the expression in Eq. (S8). You might want to replace the "=" by an "is approximated by ... as illustrated in Figure S1."

We have rewritten L21 to read:

> “This linearity is visualised in Fig. (S1) and allows the second term...”

Line 23: "reformulated as" –> Which value is used for rho_water?

We have rewritten L23 as follows:

> “This can be reformulated by setting ρ_w = 1023 kgm^{-3} as follows:”

Figure S6, caption, line 3: "five" –> "four":

We wrote:

> “A persistent, positive correlation exists in the Central Arctic and the East Siberian Sea in the last five months of winter.”

It’s possible that we’re misinterpreting the reviewer’s comment here, but we believe this should read “five”. Here we reproduce the two regions in question. The five months of December, January, February, March and April all exhibit positive correlations.

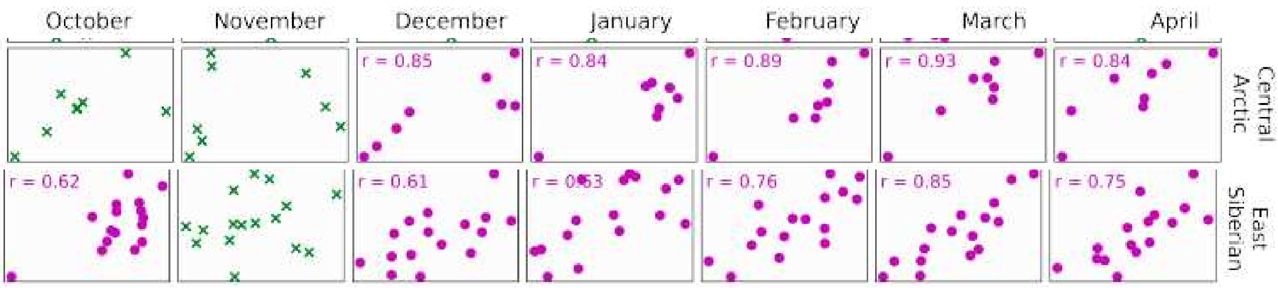


Figure S10, caption "... SnowModel-LG data." –> I suggest to add something like ... "expressed as total sea ice area of all grid cells falling into a specific SIT bin." In addition: What is the bin size? What the bin borders? Since it includes the region "Central Arctic" this figure is based on years 2010-2018 only, correct?

We have added the suggested clarification and have included the bin size (which is 5 cm). The reviewer is correct that this is carried out in the CS2 period (2010-2018) and we have also added this.

Figure S11: There are two identical panels denoted (b).

This has now been fixed.

Please make a note in the caption about the differences in the y-axis range.

We have rescaled the plots so all regions have the same y-axis scales with the exception of the Central Arctic. We have noted this exception in the caption.

Please make a note about the error bars. i.e. what these represent.

The error bars represent 1 standard deviation either side of the mean of the timeseries. We have now added this information to the caption.

Figure S11, caption: "The SnowModel-LG contribution ..." –> While this statement is undoubtly correct also the aggregated marginal seas start low and end high. So why not also commenting on those regions?

We have now commented on this.

Again, I note that you need to provide the information whether all regions but the Central Arctic are based on the full period 2002-2018 or whether indeed all regions only used data from 2010-2018.

We have now included this in the caption.

Figure S13: Again the notion about the smaller time period covered for region Central Arctic is missing.

We have now noted this in the caption.

We thank the anonymous referee for their useful comments and believe we have been able to address each of them.

Below we have copied their comments in blue and responded to each in red.

We would first like to first bring to the reviewer's attention a mistake made in the original manuscript. Due to a programming error we inadvertantly used radar freeboard data from a different product (that of Landy et al., 2020) in the winter of 2017/18. Because this product generally exhibits higher radar freeboard values than those used in the rest of the study due to a different retracking algorithm, we misidentified this winter at one point as 'a trend bucking year' for radar freeboards. We have now fixed this error and updated our statistics. This has had the following results:

- Regional declines in radar freeboard and resulting sea ice thickness are generally smoother.
- Negative trends in several regions are slightly increased.
- Negative trends are therefore more frequently statistically significant at the 5% level.
- Trends when calculated with SnowModel-LG in the 2002-2018 period are now in better agreement with those calculated from NESOSIM in the 2002-2015 period.

Despite these changes, the central thesis of our paper remains unchanged: the use of a snow product with regional variability and trends propagates into varaiblity and trends in regional sea ice thickness.

The manuscript argues that the snow climatology normally used when retrieving sea ice thickness from altimeter is missing trends and interannual variability This results in a statistical significant faster decline of Arctic sea ice in the Arctic marginal seas.

The link between the snow cover and the retrieved ice thickness is not new but the quantification is interesting and any progress towards understanding the snow cover is of importance. The use of Warren and modified Warren climatologies has been a issue for a while but there has not been any obvious alternative. I find this paper of interest to the community.

General notes

Please be consistent and call sea ice the same. First example is on line 19 where it is mentioned as both sea ice and ice. I would prefer the first.

We searched through the document and changed every mention of "ice" to "sea ice" where relevant. The only places where we have not done this is where the phrases "first year ice" and "multi-year ice" have been used, as we view these as standardised expressions.

I would reconsider whether it is necessary to plot all panels for all areas and month in different figures. These become very small. Maybe it is better to show a few representative panels and put the rest in the supplementary material.

We have removed three rows from Figure 6 in response to the specific comment concerning this issue. We have put a version of the figure with all regions in the supplement as suggested (and referenced it from the main text figure).

Minor Comments

Line 10 and 11. It is true that knowledge about the polar climate is important for the polar climate, but I think that for this abstract it is a bit out of context to include stakeholders from Arctic shipping in the description. I would stop the sentence with mentioning the polar climate system (line 10).

We have removed the words:

> "as well as for stakeholders involved in Arctic shipping and natural resource extraction"

Line 23 Agreed that thick ice has some of the properties mentioned, however thick ice do not make it easier to predict the ice cover. Assimilation of a correct ice thickness as oppose to a correct ice concentration has more memory and therefore the predictions are improved. Please rephrase.

We have rephrased this section as follows:

> "thick sea ice is far more likely to survive the melt season, increasing the average age of Arctic sea ice. Correct assimilation of ice thickness into models therefore offers opportunities for prediction of the sea ice state on seasonal timescales"

Line 34 ERS is mentioned here but its full name is mentioned on line 118. Please state full name here including the abbrivation and use the abbriviation in the rest of the text

We have now defined the acronym in the first instance and used just the acronym subsequently.

Line 107 There is an issue with the reference. Henceforth W99?

We have removed the abbreviation from this line and opted to define it when W99 is formally introduced in the Data Description section. The relevant part now reads:

> All four groups utilize modified forms of the snow climatology assembled by Warren et al. (1999) from the observations of Soviet drifting stations between 1954 and 1991 (henceforth referred to as W99).

Line 136: More a comment. It would be surprising if the variability of snow only depended on where the first year ice and the multi year ice was located.

Yes, we agree! But the sea ice type distribution is the dominant determinant of snow variability in mW99.

Line 154 I would not start by describing why W99 is not mentioned.

We have since rearranged this section (on the advice of the other reviewer) and believe this to no-longer be an issue.

Line 155: We instead compare. . . should include a reference to figure 3.

We have added this reference.

Line 168 remove one of "of". Typo.

We have removed this typo

Line 184: I would replace shaded with color coded.

We have made this change.

Line 249 – 254 I think that the readability of this section can be improved if the flow of this section is improved.

We have comprehensively reworded this and added clarifying details concerning our approach. We note that on the advice of the other reviewer we have also repositioned this section.

Line 272. Is the Central Arctic for all ice types already mentioned in line 263. If this is not the same point then please clarify.

We were referring to the MYI fraction of the Central Arctic, and have added this detail to the sentence.

Line 278 "in for each region in for each month." Should it be "one for each region and for each month. Please modify

Yes this was a typo and we have changed it. The sentence now reads:

> "the three components of $\sigma^2_{\overline{SIT}}$ for each region in each winter month"

Line 497. The last sentence should be reformulated. I think that a word is missing after positive in line 499.

We have reformulated it to read:

> "The enhancement of declining trends where they exist is perhaps *of benefit* for these industries."

Figure 1 W99 IAV values should be mentioned in figure text. If they are not used then remove the green bars

We have reworded this part of section 4.1.1 to read:

> " we find the snow variability introduced at a given point for mW99 (Fig. 3 blue bars) was on average about 50% of the value presented in W99 (Fig. 3, green bars)."

We note that this figure has been significantly repositioned on the advice of the other reviewer.

Figure 2 "Variability is displayed in a band where ice types typically fluctuates". Should this be High variability . . ..

Yes, we have made this change.

Figure 5: I would reduce the number of panels and only show the ones that are commented on. The rest can go to the supplementary material. Details are very hard to see in these small panels.

We have reduced the number of rows of this figure from eight to five by removing the rows corresponding to the Barents, Laptev and Kara Seas. The original figure has been moved to the supplement.

Figure 6: The axis labels says meters but there are no ticks on the axis. I think that it should be added

Because correlation statistics are not sensitive to the choice of axes, units or linear scalings of the values, we decided to not display axes ticks or labels and scale the axes to fit the rectangular panels of the figure. This decision was related to the issue highlighted in a previous comment about our figures being crowded. However we clearly should have stated this in our submission and we now have added the following text:

> "We note here that the correlation between the timeseries is dependent on their relative position to a linear regression. These correlation statistics are thus independent of the absolute magnitude of the values and any linear scaling of the axes. We therefore choose to present the correlations in Fig (7) without axes and scaled to the rectangular panels, so as to best show the relative positions of the points without extraneous numerical information."

The reviewer is correct also to point out that it is jarring to specify "(m)" as units without any axis ticks or tick-labels – we have therefore removed this from the axis labels (for reasons of unit-independence discussed above).

Figure 7 a and b labels should be added. I can guess which are a and b but it should not be left to the reader to guess.

We have now added these annotations

In addition I would like to move the colorbar outside of the figures and enlarge it a bit.

We have enlarged it and moved it up and outside the panels

It should be commented why the fraction of total variance can extend beyond 0 and 100. For instance November central Arctic figure b (I suppose) exend from -18 to 118 (or something like that.

We have now added some clarifying text:

> "We note that in the case of negative covariability between $\overline{\text{Snow}}$ and $\overline{\text{RF}}$, it is possible for $\sigma^2_{\overline{\text{Snow}}} + \sigma^2_{\overline{\text{RF}}}$ to be larger than $\sigma^2_{\overline{\text{SIT}}}$. This is not problematic because $\sigma^2_{\overline{\text{Snow}}} + \sigma^2_{\overline{\text{RF}}}$ does not represent a real quantity when the variables are not independent.

Figure 8 Should It be modified Warren? Labels say w99

Yes, we have now fixed this

TCD Discussion References

Barrett, A. P., Stroeve, J. C., and Serreze, M. C.: Arctic Ocean Precipitation From Atmospheric Reanalyses and Comparisons With North Pole Drifting Station Records, Journal of Geophysical Research: Oceans, 125, https://doi.org/10.1029/2019JC015415, URL https://onlinelibrary.wiley.com/doi/abs/10.1029/2019JC015415, 2020.

Belter, H. J., Krumpen, T., Hendricks, S., Hoelemann, J., Janout, M. A., Ricker, R., and Haas, C.: Satellite-based sea ice thickness changes in the Laptev Sea from 2002 to 2017: comparison to mooring observations, The Cryosphere, 14, 2189–2203, https://doi.org/10.5194/tc-14-2189-2020, URL https://tc.copernicus.org/articles/14/2189/2020/, 2020.

Boisvert, L. N., Webster, M. A., Petty, A. A., Markus, T., Bromwich, D. H., and Cullather, R. I.: Intercomparison of precipitation estimates over the Arctic ocean and its peripheral seas from reanalyses, Journal of Climate, 31, 8441–8462, https://doi.org/10.1175/JCLI-D-18-0125.1, URL http://journals.ametsoc.org/doi/10.1175/JCLI-D-18-0125.1, 2018.

Cabaj, A., Kushner, P., Fletcher, C., Howell, S., and Petty, A.: Constraining Reanalysis Snowfall Over the Arctic Ocean Using CloudSat Observations, Geophysical Research Letters, 47, https://doi.org/10.1029/2019GL086426, URL https://onlinelibrary.wiley.com/doi/abs/10.1029/2019GL086426, 2020.

ESA: Sea Ice Climate Change Initiative: Phase 2 D4.1 Product Validation & Intercomparison Report (PVIR), Tech. rep., 2018.

Giles, K. A., Laxon, S. W., and Worby, A. P.: Antarctic sea ice elevation from satellite radar altimetry, Geophysical Research Letters, 35, L03 503, https://doi.org/10.1029/2007GL031572, URL http://doi.wiley.com/10.1029/2007GL031572, 2008.

Hendricks, S. and Ricker, R.: Product User Guide & Algorithm Specification: AWI CryoSat-2 Sea Ice Thickness (version 2.2), 2019.

Kwok, R. and Cunningham, G. F.: Variability of arctic sea ice thickness and volume from CryoSat-2, Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 373, https://doi.org/10.1098/rsta.2014.0157, 2015.

Landy, J. C., Petty, A. A., Tsamados, M., and Stroeve, J. C.: Sea ice roughness overlooked as a key source of uncertainty in CryoSat-2 ice freeboard retrievals, Journal of Geophysical Research: Oceans, 44, 1–36, https://doi.org/10.1029/2019jc015820, 2020.

Laxon, S. W., Giles, K. A., Ridout, A. L., Wingham, D. J., Willatt, R., Cullen, R., Kwok, R., Schweiger, A., Zhang, J., Haas, C., Hendricks, S., Krishfield, R., Kurtz, N., Farrell, S., and Davidson, M.: CryoSat-2 estimates of Arctic sea ice thickness and volume, Geophysical Research Letters, 40, 732–737, https://doi.org/10.1002/grl.50193, 2013.

Li, M., Ke, C., Shen, X., Cheng, B., and Li, H.: Investigation of the Arctic Sea ice volume from 2002 to 2018 using multi-source data, International Journal of Climatology, p. joc.6972, https://doi.org/10.1002/joc.6972, URL https://onlinelibrary.wiley.com/doi/10.1002/joc.6972, 2020a.

Li, Z., Zhao, J., Su, J., Li, C., Cheng, B., Hui, F., Yang, Q., and Shi, L.: Spatial and temporal variations in the extent and thickness of arctic landfast ice, Remote Sensing, 12, 64, https://doi.org/10.3390/RS12010064, URL www.mdpi.com/journal/remotesensing, 2020b.

Merkouriadi, I., Liston, G. E., Graham, R. M., and Granskog, M. A.: Quantifying the Potential for Snow-Ice Formation in the Arctic Ocean, Geophysical Research Letters, 47, no, https://doi.org/10.1029/2019GL085020, URL https://onlinelibrary.wiley.com/doi/abs/10.1029/2019GL085020, 2020.

Nandan, V., Geldsetzer, T., Yackel, J., Mahmud, M., Scharien, R., Howell, S., King, J., Ricker, R., and Else, B.: Effect of Snow Salinity on CryoSat-2 Arctic First-Year Sea Ice Freeboard Measurements, Geophysical Research Letters, 44, 419–10, https://doi.org/10.1002/2017GL074506, URL http://doi.wiley.com/10.1002/2017GL074506, 2017.

Nandan, V., Scharien, R. K., Geldsetzer, T., Kwok, R., Yackel, J. J., Mahmud, M. S., Rosel, A., Tonboe, R., Granskog, M., Willatt, R., Stroeve, J., Nomura, D., and Frey, M.: Snow Property Controls on Modeled Ku-Band Altimeter Estimates of First-Year Sea Ice Thickness: Case Studies from the Canadian and Norwegian

Arctic, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 13, 1082–1096, https://doi.org/10.1109/JSTARS.2020.2966432, 2020.

Perovich, D. K., Longacre, J., Barber, D. G., Maffione, R. A., Cota, G. F., Mobley, C. D., Gow, A. J., Onstott, R. G., Grenfell, T. C., Scott Pegau, W., Landry, M., and Roesler, C. S.: Field observations of the electromagnetic properties of first-year sea ice, IEEE Transactions on Geoscience and Remote Sensing, 36, 1705–1715, https://doi.org/10.1109/36.718639, 1998.

Petty, A. A., Holland, M. M., Bailey, D. A., and Kurtz, N. T.: Warm Arctic, Increased Winter Sea Ice Growth?, Geophysical Research Letters, 45, 922–12, https://doi.org/10.1029/2018GL079223, URL https://onlinelibrary.wiley.com/doi/abs/10.1029/2018GL079223, 2018a.

Petty, A. A., Webster, M., Boisvert, L., and Markus, T.: The NASA Eulerian Snow on Sea Ice Model (NESOSIM) v1.0: Initial model development and analysis, Geoscientific Model Development, 11, 4577–4602, https://doi.org/10.5194/gmd-11-4577-2018, 2018b.

Radionov, F. and Fetterer, V. F.: Environmental Working Group Arctic Meteorology and Climate Atlas, Version 1, https://doi.org/10.7265/N5MS3QNJ, 2000.

Rösel, A., Itkin, P., King, J., Divine, D., Wang, C., Granskog, M. A., Krumpen, T., and Gerland, S.: Thin Sea Ice, Thick Snow, and Widespread Negative Freeboard Observed During N-ICE2015 North of Svalbard, Journal of Geophysical Research: Oceans, 123, 1156–1176, https://doi.org/10.1002/2017JC012865, 2018.

Sallila, H., Farrell, S. L., McCurry, J., and Rinne, E.: Assessment of contemporary satellite sea ice thickness products for Arctic sea ice, The Cryosphere, 13, 1187–1213, https://doi.org/10.5194/tc-13-1187-2019, URL https://www.the-cryosphere.net/13/1187/2019/, 2019.

Stroeve, J. and Notz, D.: Changing state of Arctic sea ice across all seasons, Environmental Research Letters, 13, 103 001, https://doi.org/10.1088/1748-9326/aade56, URL https://doi.org/10.1088/1748-9326/aade56, 2018.

Stroeve, J., Nandan, V., Willatt, R., Tonboe, R., Hendricks, S., Ricker, R., Mead, J., Mallett, R., Huntemann, M., Itkin, P., Schneebeli, M., Krampe, D., Spreen, G., Wilkinson, J., Matero, I., Hoppmann, M., and Tsamados, M.: Surface-based Ku- and Ka-band polarimetric radar for sea ice studies, The Cryosphere, 14, 4405–4426, https://doi.org/10.5194/tc-14-4405-2020, URL https://tc.copernicus.org/articles/14/4405/2020/, 2020.

Tilling, R. L., Ridout, A., Shepherd, A., and Wingham, D. J.: Increased Arctic sea ice volume after anomalously low melting in 2013, Nature Geoscience, 8, 643–646, https://doi.org/10.1038/ngeo2489, 2015.

Tschudi, M. A., Meier, W. N., and Scott Stewart, J.: An enhancement to sea ice motion and age products at the National Snow and Ice Data Center (NSIDC), Cryosphere, 14, 1519–1536, https://doi.org/10.5194/tc-14-1519-2020, 2020.

Warren, S. G., Rigor, I. G., Untersteiner, N., Radionov, V. F., Bryazgin, N. N., Aleksandrov, Y. I., and Colony, R.: Snow depth on Arctic sea ice, Journal of Climate, 12, 1814–1829, https://doi.org/10.1175/1520-0442(1999)012¡1814:SDOASI¿2.0.CO;2, 1999.

Webster, M. A., Rigor, I. G., Nghiem, S. V., Kurtz, N. T., Farrell, S. L., Perovich, D. K., and Sturm, M.: Interdecadal changes in snow depth on Arctic sea ice, Journal of Geophysical Research: Oceans, 119, 5395–5406, URL http://doi.wiley.com/10.1002/2014JC009985, 2014.

Willatt, R., Laxon, S., Giles, K., Cullen, R., Haas, C., and Helm, V.: Ku-band radar penetration into snow cover on Arctic sea ice using airborne data, Annals of Glaciology, 52, 197–205, https://doi.org/10.3189/172756411795931589, 2011.

Willatt, R. C., Giles, K. A., Laxon, S. W., Stone-Drake, L., and Worby, A. P.: Field investigations of Ku-band radar penetration into snow cover on antarctic sea ice, IEEE Transactions on Geoscience and Remote Sensing, 48, 365–372, https://doi.org/10.1109/TGRS.2009.2028237, URL http://ieeexplore.ieee.org/document/5282596/, 2010.