Author Response

Anheuser et al., 2021

tc-2021-333

RC 1

1. Assessing the performance of a new sea ice thickness product

Two key benchmarks for a new sea ice thickness product are (a) the degree to which the product outperforms a climatology, and (b) whether and in what ways the product outperforms comparable products - in this case probably PIOMAS.

To address point (a), I think the authors could do some fairly straightforward things. The first is to take the first two columns of Table 2 and correlate the anomalies. This would reveal whether SLICE is thicker when the sea ice is observed to be thicker. To put this another way - to what degree does SLICE capture the sign and size of volume anomalies from the mean state/ climatology. You could even break it down regionally into where it does & doesn't show skill.

We have completed this analysis and found that the p-value of the correlation between the SLICE volume growth and the CryoSat-2/SMOS volume growth is too high to report the results as significant. We have added Figure 8 which shows volume growth of SLICE compared to PIOMAS and AWI CS2SMOS.

To address (b) I think the users need to find a unique selling point over PIOMAS. To have impact, this product/method will need to be preferable in at least one way, either in availability or skill. The way I see it, PIOMAS has clear availability advantages: it is open-access so anybody can use it, and it stretches back to 1979. As for skill, it has the advantage of including both the dynamical and thermodynamical components of thickening. On the other hand, SLICE is based on fairly direct observations of the snow-ice interface temperature, whereas PIOMAS has to work this out by first modelling the snow and then calculating the temperature gradient across the snow and ice. To show that this translates into an actual skill advantage (and so to get people to use the method/product), I think the authors need to show that SLICE outperforms PIOMAS in some way, place or time. This is particularly the case because SLICE needs to be intialised by a sea ice thickness data set anyway, which may well be PIOMAS. The best way to assess skill would be by benchmarking against (i.e. assuming as truth) some satellite-altimeter derived ice thickness product, or by combining the work they've done with other in-situ products like Operation Ice Bridge or ULS buoys. Without doing this, I don't think the SLICE-derived ice thickness will be greatly used by the community over PIOMAS.

We have added a comparison of SLICE initialized with CS2SMOS, PIOMAS and the model described by Kang et al. 2021 (hereafter K21) to Operation Ice Bridge (OIB) data (Kurtz, 2015). This comparison has been added to Sect. 4.2 (Ln 377-384). Additionally, we've added a comparison between SLICE and PIOMAS by comparing both to AWI CS2SMOS (Ln 385-403).

These comparisons show that all three models have similar performance when modeling sea ice thickness, even without SLICE including a deformation component and including a number of assumptions that allow SLICE to retrieve instantaneous thermodynamic thickness growth rate. This has become a prominent point of the paper (Ln 9-11, 438-447, 477-478).

2. Retrieval vs modelling

The term 'retrieval' touches on an emerging issue in the sea ice community concerning what properties we model, and what properties we observe/retrieve. For variables such as sea ice height, there is clearly a spectrum from direct observations (e.g. spot heights from a satellite-mounted laser altimeter) to highly-modelled (e.g. sea ice height output from a CMIP6-class model). Other quantities (e.g. radar-derived sea ice thickness from CryoSat-2, as used in this paper) are synthesised from observations (the timing and waveforms of scattered radar energy) and simple models (hydrostatic equilibrium, radar pulse propagation through snow, etc.). The case in this paper is similarly subjective: on one hand the authors are using observations of brightness temperatures, and what I would say is an observation of the snow-ice temperature. But then they're using a highly-idealised model of latent-heat release and heat flow known as Stefan's Law (which is in many ways is not a law but a series of combined thermodynamic assumptions, which are arguably outdated - see below).

Although I am certain this is not the intention of the authors, I fear that describing these sea ice thickness data as 'retrievals' implies a degree of direct observation that is too strong. I think that this implication may, at worst, lead to users (i.e. those wishing to initialise models) thinking that these data are more certain than they are, and more directly observed than they are. We regularly see this phenomenon with PIOMAS data for instance, which is very much a model but is treated by some as if it were observed because it is fed by reanalysis products. I therefore suggest that the authors be more explicit that they are modelling ice growth, and accumulating the results of that modelling exercise to model total ice thickness. On this basis I also urge them to remove the term 'retrieval' from their title.

The authors broadly agree with the points made here. Certainly, all retrievals rely on a model at some level. For instance, even the spot heights from a laser altimeter referred to by the reviewer are based on a very simple model converting signal response times to spot heights. We believe SLICE sits in between these types of methods that are accepted as retrievals and results from models such as PIOMAS and that this is indeed the strength of SLICE.

As stated by the reviewer, the most direct output from SLICE is a thermodynamic growth rate (and conducted flux through the ice). Much like many accepted retrievals, this output relies upon a priori information--sea ice thickness, freezing point temperature, etc. We believe this step of the process can be considered a retrieval based on a simple model. We concede that accumulating the sea ice growth into an absolute sea ice thickness is more of a modeling exercise, albeit one that is heavily observationally constrained. We will make this clear in the next revision.

We have re-framed SLICE as a thermodynamic sea ice growth retrieval rather than a sea ice thickness product and propose the following title:

A simple model for daily basin-wide thermodynamic sea ice thickness growth retrieval

3. Long Term Applications

The authors state that this method could be deployed several decades into the past. For instance, they do this in both their abstract and penultimate sentence. Their justification for this is that the snow-ice temperature is retrievable back to 1987, but I think that reconstruction back to this date is not usefully possible because initialisation is not available. It seems to me that the only way of doing this would be to initialise the product with an already existing and probably more accurate pan-Arctic sea ice thickness product. If this already exists, what would be the benefit of having this product, that would be dependent on (i.e. initialised by) the superior product? As a side point, I also fear that the authors' 5 K bias correction may not be relevant pre-2000, given than the roughness and snow depth of sea ice has declined, among other geophysical changes.

We will remove allusions to the long term application of SLICE and instead leave that for future investigation.

4. 5 K bias Correction

On L82 the authors mention that they have performed a 5K bias correction on the Snow-Ice temperature data to make it match the buoy data. It's possible that they didn't do this themselves, but took it from a paper - but if so they should cite it. They certainly need to say whether they've added or subtracted the value. But this seems to be a pretty critical point that is not explored nearly enough. How did they get to this number? How sensitive is it to the data from individual buoys? How much did it improve the match between S-I temperature and the buoys? It also concerns me that they say they've done this 'to produce the best sea ice thickness retrievals'. Evaluated against what? If SIT data at the buoys has been used to tune or train the method, it casts doubt on the whole buoy-based evaluation exercise. The veracity and role of this correction must be quantified prior to publication, and its impact on the validity of the evaluation must be assessed.

The “bias correction” is added to the satellite observed brightness temperature and is due to the slight absorption of 6.9 GHz radiation by the polar atmosphere. In order to better account for this, we have chosen to use a radiative transfer model. Details can be found in new section Sect. 3.1.1.

Whereas we previously had used a static 5 K correction, the resulting change to 6.9 GHz brightness temperatures affected by the modeled transmission term is very consistently near 5 K. Figure 2 shows mean and standard deviation atmospheric correction from atmospheric transmission to a 250 K brightness temperature during December, January and February (DJF) across the years 2003-2019. The Arctic basin shows a very spatially consistent roughly 4.5 K mean with standard deviations less than 0.1 K. These results are very similar to those reported by Burgard et al. (2020) who used a geophysical model to simulate 6.9 GHz brightness temperature at TOA using MPI-ESM output data. They report a difference of 4.49 K between the model ice surface temperature and the simulated 6.9 GHz brightness temperature at TOA for pixels with 99% or greater sea ice concentration during the summer season when accounting for columnar water vapor and columnar cloud liquid water. Though we’ve reported our DJF results here, our summer results are very similar. These results will not be shown in the manuscript but are relevant to this review response.

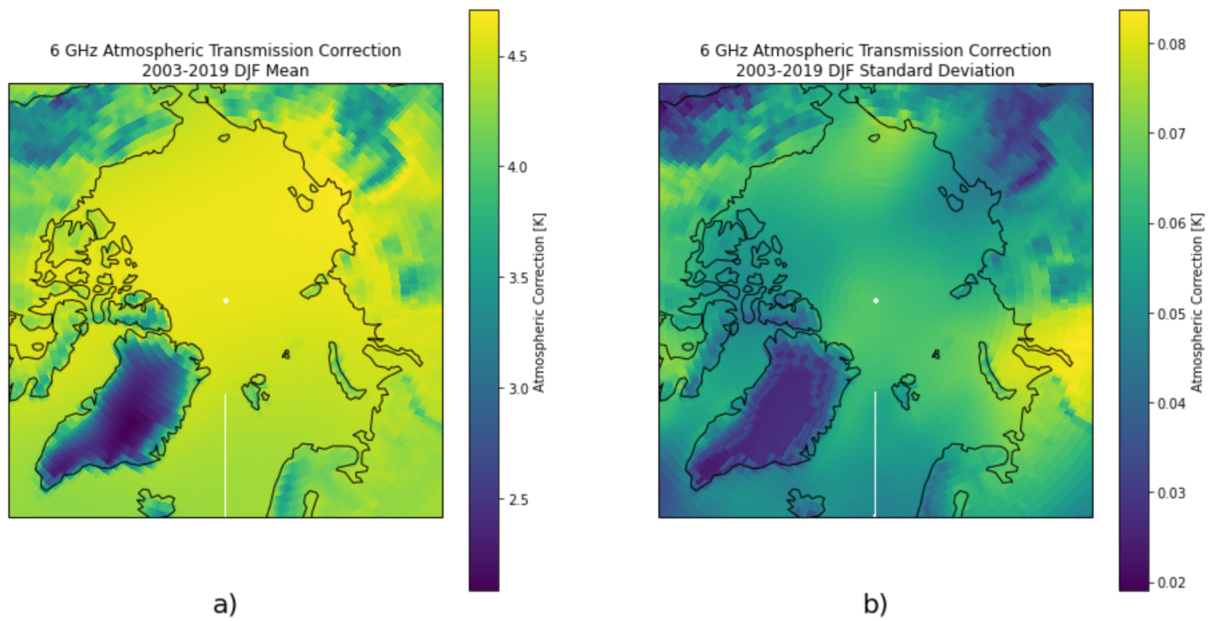

Figure 1: AMSR-E and AMSR2 6.9 GHz channels brightness temperature correction at 250 K in the 2003-2019 DJF (a) mean and (b) standard deviation calculated using a radiative transfer model and ERA5 reanalysis data.The correction is consistently near 4.5 K.

5. Ice-Ocean Boundary Conditions

Seawater is not always in local thermal equilibrium with the sea ice interface (Schmidt et al., 2004; Mcphee, 2016). I’m not an expert on this, but it’s relevant because this paper assumes equilibrium. For instance (as reported in McPhee), Maykut et al. (1971) found that without a steady basal flux of about $2Wm^{-1}$, ice continued to grow unrealistically large in their model. Indeed Parkinson and Washington (1979) had to use a flux of an order of magnitude higher than this in their model. McPhee reports that observations from Sheba and Aidjex back these model fluxes up. This is clearly something the authors should address, perhaps with a sensitivity analysis to ocean-ice heat flux (which they say they’ve set to zero). If the 5 K offset discussed earlier was deployed to reduce modelled ice growth, perhaps the authors should consider that it is not the snow-ice temperatures being too low that are causing it, but an underestimation of ocean-ice heat flux?

We have removed the assumption of no flux from the liquid sea water to the solid sea ice and added a term for this flux (F_w) to the SLICE model (Eqs. (8), (9), and (10)). A detailed investigation of basal flux and its implications has been added with new section Sect. 3.2.2.

6. Sea ice is a mushy layer

Sea ice is a mushy layer (Feltham et al., 2006) and this should be addressed when discussing heat flow through sea ice and accretion of new ice. Recently formed sea ice has brine inclusions, the phase equilibrium of which alters the bulk thermodynamic properties of the ice even well below the freezing temperature of seawater. Just stating what values you're using for the sea ice geophysical properties (L219) is insufficient. At minimum the values should be cited, and ideally they should be justified based on other previous modelling applications of the values. The constancy (as a function of temperature) of these values should also be considered. I'm not suggesting a multi-phase model of ice as I see that would make the whole situation very complicated and probably non-analytically soluble - the strength of SLICE is its simplicity. However, when presenting a model for ice growth based on heat flow through and phase change in ice near the freezing point, the mushy, mixed-phase characteristics of sea ice should be at least mentioned, and probably discussed.

We agree both that the strength of SLICE is indeed its simplicity and that further discussion of the choice of constants and their relationship to the multi-phase properties of sea ice is warranted. We have added a parameterization of the multi-phase effects on thermal conductivity per Feltham, et l. (2006). New section Sect. 3.2.3. describes this parametrization in detail and includes a discussion of sea ice density.

7. Data and code availability

I was disappointed that the code and data used in this project were not made available to either the reviewers or the sea ice community. This is particularly the case given how much the authors have used other open data such as the CS2-SMOS and PIOMAS sea ice thickness data sets. To support this view, It's perhaps useful to refer to the data policy of this journal:

The output of research is not only journal articles but also data sets, model code, samples, etc. Only the entire network of interconnected information can guarantee integrity, transparency, reuse, and reproducibility of scientific findings. Moreover, all of these resources provide great additional value in their own right. Hence, it is particularly important that data and other information underpinning the research findings are "findable, accessible, interoperable, and reusable" (FAIR) not only for humans but also for machines.

I would recommend that upon resubmission they make their code available on a site such as GitHub, and produce a persistent identifier such as a DOI. I also suggest they place their data product in a persistent archive such as that run by Zenodo, for which they will receive a DOI and the opportunity to reversion the data upon article acceptance. In taking the above steps, I believe the authors will significantly increase the impact of their research.

Thank you for this suggestion. Data used in creation of all figures is now available at https://doi.org/10.5281/zenodo.6554832. Code for creation of data and figures is available at https://doi.org/10.5281/zenodo.6561431 and https://github.com/janheuser/SLICE/releases/tag/1.0.0.

8. Other Comments

L2: 'Coupling'. I feel that 'coupled' systems/equations generally exchange information with and influence each other. However it seems that in this case you're feeding satellite information on the snow-ice interface temperature to an equation which tells you the growth rate. The satellite algorithm is not dependent on Eq. 7. So I think you should avoid portraying this as a coupled system; perhaps something like 'linking', or 'feeding' ?

We changed "coupled" to "linking".

L34: I think “is also effective” is subjective and should be changed. Perhaps “is also popular”?.

We have replaced that sentence with the following:

A coupled ocean--sea ice model with assimilated observational data is also commonly referenced. (Ln 30)

L46: I think the word ‘promising’ is subjective and should be removed.

We have removed “promising”.

L63: Should be polarization, not polarity I think?

You are correct, we have made this change. (Ln 68)

L113: “Obvious dynamic effects” - what does this mean? I think you need to be clearer in this paper between dynamic thickening in a Lagrangian sense (i.e. convergence driven ridging and rafting of ice to make it thicker), and dynamical thickening in an Eulerian sense (advection of thicker ice into and thinner ice out of a grid cell).

We agree, we need to be clearer about advection vs. deformation. In this case, we mean “obvious deformed ice”. We have added a distinction between deformation and advection throughout the paper (e.g., 173; 336).

L127: The snow loading is used before the hydrostatic conversion, in the calculation of the height of the ice surface above the waterline to account for the delay in radar propagation through the snow (e.g. Mallett et al., 2020).

We have updated this description. (Ln 91-92)

L129 CPOM is not affiliated with ESA

We have removed “ESA”. (Ln 94)

L142: Complementing, not complimenting

This has been updated. (Ln 107)

L150: It’s noticeable that the grid on which data are supplied and applied is consistently described up until the PIOMAS description. This is perhaps the most important data set for which to mention this, because the native grid is very unusual. Worth describing or not describing the grids consistently.

We have added a description of the PIOMAS grid. (Ln 150-154)

L360: Antarctic sea ice floes often have negative freeboards so you probably won’t retrieve get the snow-ice interface temperature. Some floes have had them in the past leading to the formation of snow-ice, and ice lenses also exist in the snow, which I imagine will significantly complicate the retrieval of the snow-ice interface temperature. Indeed the potential for negative freeboards in the Arctic (Merkouriadi et al., 2020) should perhaps be mentioned at some point.

We have removed the allusion to Antarctic sea ice.

L374: It’s my opinion that you’ll only be ‘retrieving’ sea ice thickness when you do actually account for both thermodynamic and dynamic/advective contributions to sea ice thickness at a point. Right now I’d say you’re modelling one part of it.

See comments above.

9. Figures and Tables

The map projections used in this paper were unusual and not well-suited to the data being displayed. They look a bit like a Near-Sided Perspective projection? In any case, I think a more traditional North-Polar- Stereographic or Lambert Azimuthal-Equal-Area projection would be better. It looks in this case like data nearer the pole is being over-represented in area, and it’s concerning that Hudson and Baffin Bay are hidden and highly distorted respectively.

We have updated all applicable figures to the Stereographic projection.

I also think a figure should be displayed complementing Table 1 (perhaps put in a supplement?) with the tracks of the buoys used to evaluate SLICE. This would give the reader a better sense of the geographic/spatial validity of the buoy-based evaluation presented.

We have added this, along with a plot of the OIB tracks.

Figure 1: The colorbar should be labelled with the variable (S-I Temp), and units (Kelvin) should be stated.

We have updated this.

Figure 5: The blue/white plots aren't providing much narrative value here. They're similar in appear- ance and concept to Fig 4, and the panels often look very similar to each other; I would suggest putting them in a supplement and increasing the size of the difference plots, which are much more relevant and important.

Table 2: I think put this in a supplement and display the data as a timeseries. You could put the Vol. Growth in first two columns on the Y axis and the relative difference in % on a secondary Y axis. I'm not convinced the column with absolute differences adds much value. I think displaying this data as a graph would give the reader a much better feel for what's going on.

Figure 6: Again, enlarge and focus on the difference plots and put the blue/white plots in a supplement. Table 3: Same comment as Table 2, and you could probably merge the resulting figures.

We have removed SLICE initialized with PIOMAS from the manuscript and instead focused on comparing SLICE (initialized with AWI CS2SMOS) to PIOMAS, per comments from both referees. As such, we have removed previous Fig. 6 and previous Table 3. We have separated out end of season results from end of season difference plots as well. In an updated version of previous Fig. 5, new Fig. 6 shows end of season SLICE minus AWI CS2SMOS and end of season PIOMAS minus AWI CS2SMOS in order to compare SLICE and PIOMAS using AWI CS2SMOS as the reference dataset. End of season results for SLICE, PIOMAS and AWI CS2SMOS are shown in new Fig. A1. Additionally, previous Table 3 has been replaced by new Fig. 8 showing volume growth for SLICE, PIOMAS and CS2SMOS in graphic form rather than tabular form.

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RC 2

Major comments

1. Assumptions in the SLICE method

From L202 to L208, the authors listed the four assumptions used in the SLICE retrieval method. I have concerns about the second and the third assumptions. In my opinion, the second assumption is equivalent to the statement that the temperature profile of sea ice is linear. But if you see the buoy measured temperature profiles, you will find this assumption is not always valid. Such linear profile assumption is generally valid during wintertime. Moreover, even during wintertime, sudden change in air temperature due to warm/cold advection or radiative forcing due to cloud cover can rapidly change surface temperature which makes curves in the temperature profile. The good thing is time-averaged temperature profile during wintertime is close to linear (Shi et al., 2020). The authors would consider shortening of retrieval period of the SLICE method.

The third assumption tells that there is no internal heat source associated with shortwave radiation. In other words, this assumption is valid for the regions where the solar zenith angle is maintained less than zero. The authors should check the validity of this assumption regarding the seasonal variability of the solar zenith angle. There can be sunlight in lower latitude regions during fall and spring. Otherwise, please consider the shortwave radiation effects or justify that the shortwave radiation effect is negligible for the lower latitude regions during the fall and spring seasons.

We agree that the assumptions of linear temperature profile and negligible shortwave radiation are valid during the winter time only and that SLICE is not valid outside of the sea ice growth season. As such, we have shortened the SLICE one-dimensional and basin-wide outputs to be from November 1 to March 31 only.

The other point is that the authors mentioned that the retrieval method should be applied in a Lagrangian sense in L224 but they neglected sea ice motion in the actual calculation (L262). What are the reasons for this? There must be justification for the neglect of sea ice motion. Each sea ice parcel should be tracked and matched with the nearest satellite Tsi because the equation used in this study is a time-dependent equation. Meanwhile, the neglect of sea ice motion is not the same as focusing on thermodynamic growth. Thermodynamical growth, sea ice motion, and dynamical growth (deformation due to convergence and divergence) should be addressed separately. Consideration of sea ice motion without dynamical growth is possible.

The authors agree that sea ice motion and deformed sea ice due to convergence or divergence should be treated separately. While we are unable to include the effects of deformed ice, we have added a sea ice motion component to the basin-wide SLICE results. New section Sect. 3.3 describes this component.

2. Effectiveness of self-correcting characteristic

It was interesting to read the statement in L225 regarding the self-correcting characteristic of the SLICE method. Thicker sea ice indeed grows slower than thinner sea ice with a given Tsi and vice versa according to equation (7). Therefore, the error in sea ice thickness can be relaxed by the modulation of sea ice growth speed.

However, the relaxation speed of error is important as well. If the speed of relaxation is slow, the effectiveness of self-correcting characteristics will be minor and the initial condition will be the major factor that determines the accuracy of sea ice thickness estimation. In L249-250 and Figure 2, the authors tried to show the effect of the self-correcting characteristic. Although it seems that 0.25 m deviations in the initial condition are decreasing with time, it will be better to specify the improvement quantitatively to know how fast the errors are relaxed. In addition, I suggest conducting a sensitivity test and including the result as an appendix.

I found some doubtful points on the self-correcting characteristic of the SLICE method. In my opinion, if the method is self-correcting, the retrieval result should fluctuate around the true state. Why is the SLICE retrieval (red solid line) the center of red shade instead of the buoy (blue solid line) which is the true state? In addition, I think the sentence “The bias grows with time as the SLICE profile moves away from its initialized thickness” makes a contradiction with the self-correcting characteristic of SLICE.

The significance of self-correcting characteristic is important for the algorithm extension to the past because such characteristic makes the retrieval method relatively independent from the accurate initial condition. If the self-correction is significant, SLICE sea ice thickness records initialized with PIOMAS can be constructed, and it will be more accurate than PIOMAS. To examine this, I suggest comparing the accuracy of the sea ice thickness from the PIOMAS and that from the SLICE initialized with the PIOMAS. There are some widely used independent datasets for validation such as Operation IceBridge (OIB), buoy, upward-looking sonar (ULS), and submarine observations.

The self-correcting characteristics of SLICE will not be significant enough to remove any dependence upon its initial condition. An accurate initial condition is important for SLICE’s results. With regard to long term studies, we will remove allusions to the long-term application of SLICE and instead leave that for future investigation. There are other advantages of SLICE over PIOMAS that are more significant than the theoretical error reduction discussed here. SLICE is thermodynamically forced by satellite observations of snow-ice interface temperature rather than an atmospheric reanalysis and is a much simpler model. Additionally, it is likely that PIOMAS exhibits similar behavior in that thinner ice grows faster than thicker. We have removed any discussion of self-correction other than:

In Eq. 9, thicker sea ice grows slower than thinner sea ice and thinner sea ice grows faster than thicker ice with a given snow--ice interface temperature. This means that in the presence of only thermodynamic effects, sea ice that is too thick or too thin will correct towards the unbiased SLICE thickness profile. (Ln 268-271)

and

Additionally, SLICE has a self-correcting quality by nature of Eq. 9 whereby sea ice thicknesses that are biased in either direction approach the unbiased SLICE sea ice thickness over time. (Ln 419-420)

3. Retrieval or modeling (significance of this study)

In some sense, the SLICE retrieval method seems to be a thermodynamic sea ice model. The reason is that it simulates sea ice thickness evolution with time, and the result of SLICE retrieval is highly dependent on initial conditions rather than observed data. I think that the SLICE method is a simplified version of the thermodynamic sea ice model introduced by Maykut and Unterstiener (1971) or the PIOMAS. It will be nice for the authors to explain why the SLICE method is satellite retrieval.

The most direct output from SLICE is a thermodynamic growth rate (and conducted flux through the ice). Much like many accepted retrievals, this output relies upon a priori information--sea ice thickness, freezing point temperature, etc. We believe this step of the process can be considered a retrieval based on a simple model. We concede that accumulating the sea ice growth into an absolute sea ice thickness is more of a modeling exercise, albeit one that is heavily observationally constrained.

We have re-framed SLICE as a thermodynamic sea ice growth retrieval rather than a sea ice thickness product and propose the following title:

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Nonetheless, the novel point of this study is SLICE method is independent of the atmospheric reanalysis generally used as the forcing to sea ice model. The most relevant study to the SLICE method will be Kang et al. (2021), which simulates the physical state of a snow- ice system by using a thermodynamic equation set forced by atmospheric reanalysis and nudged by satellite Tsi. This study has significance in terms of constructing an independent sea ice thickness record, while the physics of SLICE is very simplified compared to Kang et al. (2021) or other thermodynamic sea ice models. I recommend including an ice thickness comparison with the results of Kang et al. (2021). Their results are open to the public, and the authors can find the data repository in their paper. It is worth comparing the performance of the SLICE method with other sea ice models with more sophisticated physics and forced by reanalysis data.

We have added a comparison of SLICE initialized with CS2SMOS, PIOMAS and the model described by Kang et al. 2021 (hereafter K21) to Operation Ice Bridge (OIB) data (Kurtz, 2015). This comparison has been added to Sect. 4.2 (Ln 377-384). Additionally, we've added a comparison between SLICE and PIOMAS by comparing both to AWI CS2SMOS (Ln 385-403).

These comparisons show that all three models have similar performance when modeling sea ice thickness, even without SLICE including a deformation component and including a number of assumptions that allow SLICE to retrieve instantaneous thermodynamic thickness growth rate. This has become a prominent point of the paper (Ln 9-11, 438-447, 477-478).

4. Bias correction for satellite Tsi

The authors mentioned that "The resultant snow-ice interface temperatures were found to require a bias correction of 5 K in order to match buoy snow-ice interface temperatures..."). I have read Lee and Sohn (2015) and remember that the snow-ice interface derived from AMSR- E 6.9 GHz brightness temperatures are validated with buoy measured temperature. The validation result showed that the bias was less than 1 K, which is a very different result from the 5 K bias in the manuscript. Lee and Sohn (2015) also neglected atmospheric/snow absorption.

Regarding this situation, first I thought that it is possibly due to the bias within AMSR-E and AMSR2 measurements. However, the authors stated that the AMSR2 data has been intercalibrated with the AMSR-E data so this may not be the issue. Then, may the version of L3 brightness temperature be a problem? Or simply authors failed to reproduce the T_{si} retrieval algorithm.

It is unclear why the results from Lee and Sohn (2015) seem to have not required a correction for atmospheric absorption. The physics described in that paper are valid at the surface but the brightness temperatures viewed by the satellite at 6.9 GHz will be affected by the atmosphere, which we are accounting for.

This is a very critical issue because sea ice thickness is determined by Tsi, which is the only real observation used for the sea ice thickness retrieval. The mentioned comparison result between buoy data and Tsi calculated by the authors showing 5 K bias must be presented (as an appendix) to justify the bias correction procedure. It will be worth reproducing figure 6 in Lee and Sohn (2015).

The "bias correction" is added to the satellite observed brightness temperature and is due to the slight absorption of 6.9 GHz radiation by the polar atmosphere. In order to better account for this, we have chosen to use a radiative transfer model. Details can be found in new section Sect. 3.1.1.

Whereas we previously had used a static 5 K correction, the resulting change to 6.9 GHz brightness temperatures affected by the modeled transmission term is very consistently near 5 K. The below figure shows mean and standard deviation atmospheric correction from atmospheric transmission to a 250 K brightness temperature during December, January and February (DJF) across the years 2003-2019. The Arctic basin shows a very spatially consistent roughly 4.5 K mean with standard deviations less than 0.1 K. These results are very similar to those reported by Burgard, et al., 2020 who used a geophysical model to simulate 6.9 GHz brightness temperature at TOA using MPI-ESM output data. They report a difference of 4.49 K between the model ice surface temperature and the simulated 6.9 GHz brightness temperature at TOA for pixels with 99% or greater sea ice concentration during the summer season when accounting for columnar water vapor and columnar cloud liquid water. Though we've reported our DJF results here, our summer results are very similar. These results will not be shown in the manuscript but are relevant to this review response.

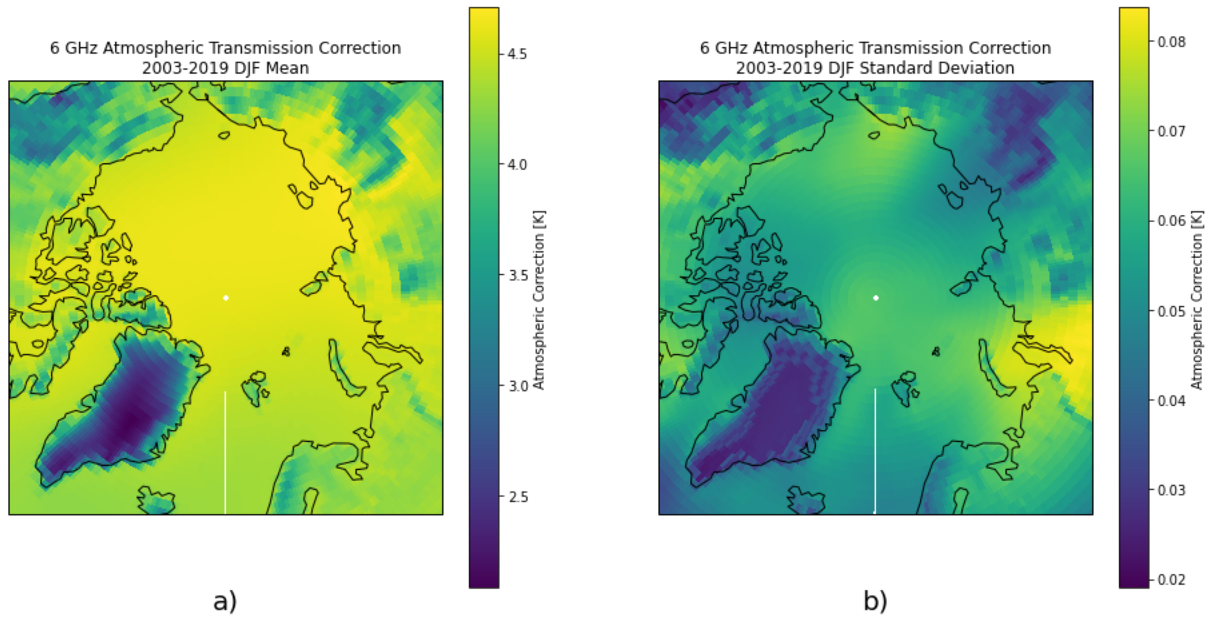

Figure 3: AMSR-E and AMSR2 6.9 GHz channels brightness temperature correction at 250 K in the 2003-2019 DJF (a) mean and (b) standard deviation calculated using a radiative transfer model and ERA5 reanalysis data.The correction is consistently near 4.5 K.

5. Minor comments

L29-L37: Please provide more details for relevant studies on sea ice thickness retrieval in order to emphasize the novelty or necessity of SLICE. How are the satellite altimetry methods limited in spatial coverage and temporal resolution (I think the resolution of ICESat-2 is better than passive microwave sensors such as AMSR2 6.9 GHz)? What are the limitations of the other methods? How is this study related to the existing studies?

We have added more quantitative information to this passage:

Though the instruments aboard these satellites have relatively high spatial resolutions, it takes 28 days and 91 days, respectively, for CryoSat-2 and ICESat-2 to cover the entire Arctic due to their relatively low spatial coverage (Ln 26-28)

L63: horizontally and vertically polarized...

You are correct, we have made this change. (Ln 68)

L215: Please define negative degree-days in the manuscript and provide what happens if the temperature is positive (melting?).

The negative degree-days term is defined in L216. SLICE is not capable of capturing melt.

L221: It is hard to know which equation was used for sea ice thickness calculation. Equation (4) is too general. Did you use equation (8) which is an analytic solution for sea ice thickness, or equation (7) for change in sea ice thickness per unit time and accumulate the thickness changes?

We have added Ln 265-266, clearly stating which equation is used.

L235-237: Why the retrieval method was initialized with such condition (the day when the 14 d rolling average sea ice growth exceeded 1mm d-1)? Please provide the reason.

We have updated the one-dimensional results to begin with the buoy initial condition on November 1 rather than the previous definition of a start time based on ice growth exceeding a threshold. This also reflects how the basin-wide methodology works. (Ln 370)

L400: I think uploading the data produced in this study to the public data repository more fits the data policy of TC journal.

Thank you for this suggestion. Data used in creation of all figures is now available at https://doi.org/10.5281/zenodo.6554832. Code for creation of data and figures is available at https://doi.org/10.5281/zenodo.6561431 and https://github.com/janheuser/SLICE/releases/tag/1.0.0.

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