Review of: A daily basin-wide sea ice thickness retrieval methodology: Stefan's Law Integrated Conducted Energy (SLICE)

The authors wish to thank the reviewer for the very thoughtful and helpful review. We have responded in red font to individual comments below where necessary.

1 Synopsis

The authors present a method of modelling thermodynamic sea ice growth based on the temperature of the snow-ice interface and assumptions involving the latent heat of fusion (SLICE). This was an original and interesting project, but I question whether it has reached the necessary level of completion to be published in The Cryosphere. In particular, I don't think the authors showed that their 'retrievals' (which are not made available) outperform a popular model (for which the data is publicly available): I believe this would affect the impact of the paper were it to be published.

I also take issue with the framing of SLICE as a 'retrieval', when I would argue it is more a model-output. Furthermore, I question the assertion that this exercise can be extended back in time in a useful sense, given that it requires initialisation with a separate product in a way that may not be practically possible pre-2000 as suggested. With regard to this, I also found it slightly strange that the authors stressed the near-term potential to improve the product by extending it back in time and including dynamical/advection based thickening, but didn't present either. Finally, I was concerned with the relatively undocumented application of a 5 K offset to the snow-ice interface temperatures, which are the fundamental data set underpinning the exercise.

Based on the above comments, I believe that the manuscript requires some major revisions and additions prior to any publication. Given the salience of the PIOMAS model within the paper, I strongly suggest the revisions include a comparison of the performance of SLICE against PIOMAS, explicitly evaluated against observations from a satellite product (or OIB). I believe the sea ice thickness community will only use this method where they can be clear about whether, when and where SLICE out-/under-performs PIOMAS. Should the editor move forward after receiving major revisions, I would like to review the manuscript again prior to publication.

2 Significant Comments

2.1 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.

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 will add a comparison to the paper 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).

The SLICE basin-wide component now includes advection of sea ice parcels using the NSIDC Polar Pathfinder daily sea ice drift product (Tschudi et al., 2019). We had planned to include this element in a future study but at the request of Referee #2, we have chosen to include advection in this paper. SLICE is initialized with the CS2SMOS data or PIOMAS (interpolated to the 25 km EASE grid 2.0) from the first week of November and each 25 km x 25 km grid cell is divided into 5 km x 5km parcels, which are advected daily using the motion vectors interpolated to their position and who add sea ice thickness thermodynamically using the SLICE thermodynamic model. As before, new ice per a sea ice concentration product is initialized at 0.01 m. At any given time step, the parcels can be gridded back to the EASE grid 2.0 grid by taking the mean of parcels within each EASE grid. This process will be included in all basin-wide results shown in the revised manuscript.

OIB data from the month of March for the years 2013 through 2018 (including NSIDC OIB quick looks data) was first binned by SLICE grid cell and averaged across each bin to create an OIB dataset collocated with SLICE. Both PIOMAS and the Kang et al. 2021 data were also interpolated to the SLICE grid. Using only SLICE grid cells containing 100 or more individual OIB sea ice thickness data points, a comparison between SLICE initialized CS2SMOS, PIOMAS and K21 was created and shown in Figure 1.

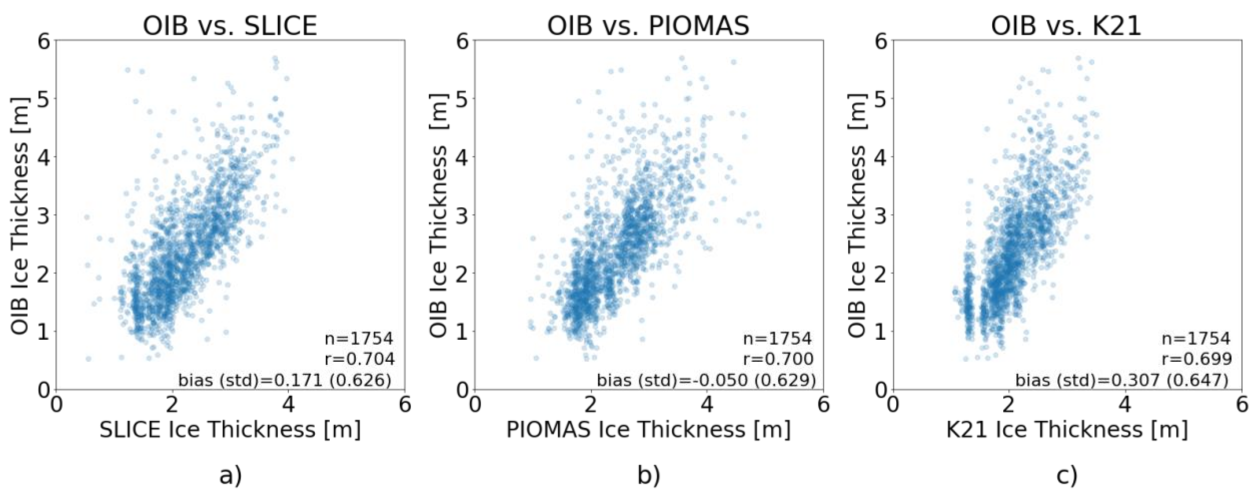

Figure 1: OIB thickness versus (a) SLICE intilialized with CryoSat-2/SMOS, (b) PIOMAS and (c) Kang et al., 2021 data including number of data points, linear correlations and bias (standard deviation). SLICE has the highest linear correlation though all three are virtually equal.

The highest linear correlation value belongs to SLICE at 0.704, however the linear correlation for PIOMAS and K21 are very near that value at 0.700 and 0.699 respectively. The smallest mean (standard deviation) bias is exhibited by PIOMAS at -0.050 m (0.629 m) followed by SLICE with 0.171 m (0.628 m) and K21 with 0.307 m (0.647 m). This analysis will be included in the revised manuscript.

These statistics show that all three models have similar performance when modeling sea ice thickness, even without SLICE including a deformation component. The differences are related to complexity of the model and reliance upon model reanalysis data. Whereas both PIOMAS and K21 require snow information and must calculate the temperature profile in the snow in order to determine the temperature profile in the ice from a reanalysis product, SLICE uses direct retrieval of the snow—ice interface temperature in order to calculate the heat flux through the ice and therefore thermodynamic sea ice growth. By assuming a linear temperature profile in the sea ice, SLICE also removes the requirement for multiple ice layers to be tracked by the model.

We don't believe SLICE to be a replacement for existing sea ice thickness retrievals, rather an additional independent dataset created using an observationally constrained very simple model that may be more applicable in certain situations.

2.2 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 propose the following title:

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

2.3 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.

Lee and Sohn (2015) investigated the impact of surface roughness on their snow—ice interface temperature retrieval. They report typical sea ice surface roughness figures of at maximum 2.5 mm. With this maximum in mind, they applied a Bragg scattering model to find the sensitivity of the snow—ice interface emissivity to surface roughness values of 0 to 3 mm. In the case of 3 mm surface roughness, the impact to horizontal polarization emissivity is 3.5% while the impact to vertical polarization emissivity is nearly zero. For long term applications, we will use the vertical emissivity and vertical brightness temperatures. Additionally, they note that snow in the Arctic region is transparent to the low frequency channels of the AMSR-E and AMSR2 instrument (Matthew, et al., 2009). Lastly, atmospheric changes over time will be accounted for with the inclusion of a radiative transfer model based replacement for the 5 K bias correction (more info in the following section).

2.4 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 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 radiation transfer model (RTTOV; Saunders et al., 2018) and pressure, temperature and humidity profiles along with skin temperature, surface pressure, 2 m temperature, 2 m humidity and 10 m winds from ECMWF ERA5 reanalysis monthly data (Hersbach et al., 2018) to model the effect of the atmosphere on the 6.9 GHz AMSR2 channels. For every month since 2003 and for the entirety of the Arctic basin, we have used the model to estimate the atmospheric transmission at 6.9 GHz and applied a location and time specific transmission factor to each AMSR2 radiance used in the calculation of snow—ice interface temperature. We will change equation 1 to reflect this by inserting a transmission t to the right side of the equation and remove the statement that absorption at 6.9 GHz by the atmosphere is assumed negligible. The phrase "bias correction" will be removed from the manuscript as it doesn't accurately describe this methodology. Rather, we will add a description of the new methodology described here.

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.

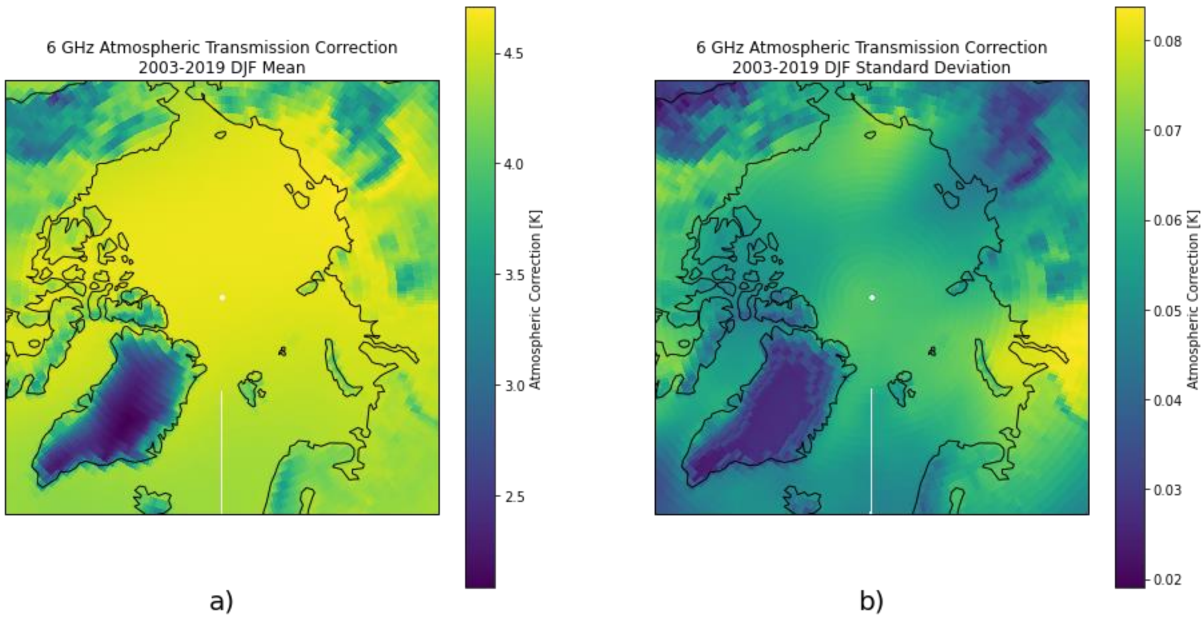

Figure 2: AMSR-E and AMSR2 6.9 GHz channels brightness temperature correction at 250 K in the 2003-2019 DJF (a) mean and (b) standard deviation calculated using a radiative transfer model and ERA5 reanalysis data.The correction is consistently near 4.5 K.

2.5 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?

First, we will remove the assumption of no flux from the liquid sea water to the solid sea ice and add a term for this flux (F_w) to equation 7:

$$\frac{\partial H}{\partial t} = \frac{\kappa_i}{\rho_i L H}(T_f - T_{si}) - \frac{F_w}{\rho_i L}$$

The addition of this term makes equation 7 difficult to solve analytically. However, the reduction in thickness growth due to flux from the liquid sea water is not dependent upon thickness and constant for a given flux value:

$$\frac{\partial H}{\partial t} = -\frac{F_w}{\rho_i L}$$

Multiplying this by a time interval will yield the total effect of the flux from the liquid sea water over that time interval. Equation 8 can be updated to include this effect:

$$H_t = \sqrt{H_{t-1}^2 + a^2 S} - \Delta t \frac{F_w}{\rho_i L}$$

We like the suggestion to apply a sensitivity analysis to the ocean—ice heat flux. With the new form of equation 8, it is straight forward to determine how much a change in basal flux will affect thickness growth. For a given snow—ice interface temperature, the reduction of sea ice thickness growth by inclusion of a basal flux is linearly related to the basal flux value by a factor of $1/\rho_i L$. Each 1 W m^{-2} of basal flux decreases sea ice thickness growth by 2.84×10^{-4} m d^{-1}. As such, the inclusion of 2 W m^{-2} per Maykut and Untersteiner (1971) will reduce sea ice growth by 5.67×10^{-4} m d^{-1}. We have updated SLICE to include a 2 W m^{-2} flux from the liquid sea water.

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 don't think that including the effects of temperature and phase change within internal brine pockets on the thermal conductivity of the sea ice is too complicated for SLICE and will include this effect. We begin by assigning an ocean salinity value of S = 33 ppt. We will assume that the ice is in thermal equilibrium relative to phase change within the ice between liquid brine and solid ice and calculate freezing point temperature (in °C) from S (in ppt) per Notz (2005) with the following polynomial fit to the liquidis curve for sea ice:

$$T_{f,NaCl} = -0.0592S - 9.37x10^{-6}S^2 - 5.33x10^{-7}S^3.$$

The latent heat of fusion of liquid to solid phase change the difference between the enthalpies of the two states if they are at the same temperature. In this case, we will use a latent heat of fusion as calculated by Notz (2005) using his empirical relationship for latent heat of fusion as a function of temperature (in °C):

$$L(T) = 333700 + 762.7T - 7.929T^2.$$

We then use equation 10 from Feltham (2006) to calculate effective conductivity at each time step:

$$k_{eff} = k_i - (k_i - k_b)\,(T_l(S_i) - T_l(S_{bulk}))/(T_l(S_i) - T),$$

with k_i and k_b from Batrak et al. (2018), Bailey (2010) and Schwertfeger (1963):

$$k_{bi} = k_i(2k_i + k_a - 2V_a(k_i - k_a))/(2k_i + k_a + 2V_a(k_i - k_a))$$

$$k_i = 1.162(1.905 - 8.66x10^{-3}T + 2.97x10^{-5}T^2)$$

$$k_b = 1.162(0.45 - 1.08x10^{-2}T + 5.04x10^{-5}T^2)$$

$$k_a = 0.03$$

$$V_a = 0.025.$$

The question becomes what temperature and ice salinity, S_{bulk}, would be used when calculating the effective conductivity since SLICE treats the sea ice layer as a single layer. Two options are to use snow—ice interface temperature and a surface salinity of 0 or to use a temperature mean over the depth of the ice and a salinity value from a parameterization such as Cox and Weeks (1974). We've tried both approaches and while the results are slightly different from one another, the difference is not significant. We will use effective conductivity calculated with surface conditions for SLICE which is similar to the approach adopted by Cox and Weeks (1988) who also used conductivity calculated from the surface to determine conductive flux through the ice layer.

Additionally, we will have further investigated our choices of density and latent heat of fusion. A first-year sea ice (FYI) density of 917 kg m^{-3} and multi-year sea ice (MYI) density of 882 kg m^{-3} was reported by Alexandrov et al. (2010) and these values have seen use in the sea ice thickness calculations from CryoSat-2 data (Laxon et al., 2013; Tilling et al., 2018; Hendricks and Ricker, 2016; Kwok and Cunningham, 2015). A sea ice density of 915 kg m^{-3} is also in use with altimeter data (Kurtz et al., 2014; Petty et al., 2020) and a sea ice density of 925 kg m^{-3} has been used with IceSat data (Kwok et al., 2009). Choice of sea ice density is a significant source of uncertainty in altimeter-based estimates of sea ice thickness. Kurtz et al. (2014) report that the range of densities from 882 kg m^{-3} to 925 kg m^{-3} yields a 1.1 m range in sea ice thickness estimates from a 60 cm snow—ice freeboard with 35 cm of snow. All ice formed by the SLICE model is FYI, leaving us with the choice of 915 kg m^{-3}, 917 kg m^{-3} or 925 kg m^{-3}. We have moved forward with 917 kg m^{-3}. For a given snow—ice interface temperature, basal flux and sea ice thickness, a change to 915 kg m^{-3} would increase sea ice thickness growth rate by only 0.2% and a change to 925 kg m^{-3} would decrease sea ice thickness growth by only 0.8%.

2.7 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 repro- ducibility 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.

While this step is not required for publication, we would like to increase the impact of this research in any way we can and will work to post both code and data on a publicly available repository. We will aim to provide more details along with the next revision of the article.

3 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 will use the word “linking”.

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

We will replace that sentence with the following:

“A global coupled ocean sea ice model with assimilated observational data is also commonly referenced.”

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

We will remove “promising”.

L63: Should be polarization, not polarity I think?

You are correct, we will make this change.

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 will also be sure to differentiate between the two effects throughout the paper.

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 will update this description.

L129 CPOM is not affiliated with ESA

We will remove “ESA”.

L142: Complementing, not complimenting

This will be updated.

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 will add a description of the PIOMAS grid, it is an important detail since the PIOMAS data is later interpolated to the EASE grid.

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 will remove 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 after section 2.2.

3.1 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 will update 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 geo- graphic/spatial validity of the buoy-based evaluation presented.

We will add 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 will update 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 agree that these suggestions will improve the narrative of the manuscript will update accordingly.

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