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

1 Synopsis

Firstly, congratulations to the authors on a strong set of responses to my last review. A lot of effort has clearly gone into this, and it has visibly improved the manuscript. I believe that the write up to this project can and should ultimately be published; driving a model with the snow-ice interface temperature is an original and interesting idea, and a lot of work has gone in to implementing the method.

However I have some remaining concerns about the paper presented here. These chiefly involve the design of the line-plot figures and the consideration of the contribution of dynamical processes to sea ice thickness. I'd also like to see an additional investigation involving the snow-ice temperatures actually measured at the buoys.

I would like to review the revised manuscript again before publication.

2 Significant Comments

2.1 Figure 3

This plot is not publication quality due to its design. The subplots do not succeed in fully displaying their data and conveying the information within. The subplots are too narrow, and the lines are so similar that they are almost indistinguishable in some cases. You also don't need to put 'Date' under every x axis. You could also probably move the panel-letter annotations so that the plots are less letterbox-like.

The plot also raises the question of how skillful your model actually is. It looks like the SLICE output is pretty linear - from this it's hard to see how much the slope actually responds to atmospheric conditions?

I think you should also critically consider the use of the r-value here. Specifically, if your model output just linearly and monotonically increased in value but had completely the wrong gradient, and your buoy-data also monotonically and linearly increased, the r value would be 1. Even though the rate of growth in your model is completely wrong. I think you need to come up with a better or additional statistic for evaluating your model. For instance 2013F shows a pretty clear divergence in the growth rates, but r=0.99. Whereas 2012L seems to show a very similar divergence, but it occurs later and delivers r=0.58. What justifies the r value being so much lower for these two model-runs when they're so visually similar?

It's also clear that your r-value is lower when the buoy-data exhibits thinning. This is confusing, as it often happens in the cold-season when I doubt that the ice is thermodynamically melting or thinning? Or perhaps it is? I think you should offer a (perhaps speculative) explanation about what's driving this thinning as it's clearly not captured by your model.

Finally your x-axis ticks presumably indicate the first of the month. If so, say this in the caption.

2.2 Retrieving the Thermodynamic Component of the Thickness

The authors now explicitly claim to be 'retrieving' the thermodynamic component of sea ice thickness (so do not consider the contribution of dynamical processes). I think it is theoretically possible to do this, but the validation of such a retrieval beyond 1D is nuanced. Specifically, actually observing the thermodynamic component of mean sea ice thickness in an area is difficult and this quantity is not necessarily represented by the mean thickness of a CS2/PIOMAS grid cell.

The sea ice cover in a specified area has a well-defined mean thickness, part of which is the result of thermodynamic growth (e.g. congelation/accretion), and part of which is the result of dynamical processes (thin ice formation in leads, ridging, rafting etc). We can conceptually break these components up into two additive parts (see von Albedyll et al. (2022)). But actually observing these components separately in reality is hard work: The 2022 paper by von Albedyll et al. using airborne data indicates that dynamical processes contributed 30% to the mean ice thickness during MOSAiC, with Koo et al. (2021) getting similar numbers for the same period using ICESat-2. Work previous to that (von Albedyll et al., 2021) indicates 50 %. Kwok and Cunningham (2016) have similarly high numbers for the contribution of dynamics to sea ice thickness.

So for a given area (like a pixel of CS2SMOS or PIOMAS), the thermodynamic component of the sea ice thickness is potentially quite variable, and is potentially much less than the total thickness. The authors should therefore not expect their 'retrievals' of thermodynamic-SIT to match these total-SIT products. If they do match, then the authors are implying that *there is no dynamical contribution* to the sea ice thickness in the area of comparison. This must be addressed before publication.

However, for the 1D case of the ice-mass-balance buoys, I think the validation is immediately useful. The buoys measure thermodynamic growth at a point, which is what is being modelled by SLICE. I'm not sure what the situation would be for OIB, but this also needs to be considered in these respects. I think the case remains to be made about why SLICE would reflect the basin-wide sea ice volume evolution (Figure 8 & L448), given that it doesn't capture the dynamical contributions to sea ice thickness. If the authors are of the mind that it matches well because the dynamical effects are negligible, then they should say so. This should specifically be done with reference to the teardrop rheology/ mechanical redistribution effects that are incorporated within PIOMAS (which I am not an expert in).

2.3 Use buoy snow-ice temperature to separate error in model from error in forcing

In exploring when/why the buoy data diverges from your modelled SIT, it would make sense to check if some of the divergence is caused by inaccurate retrievals of the snow-ice temperature by the radiometers. It seems that this could be done very straightforwardly as the buoys presumably give you the whole temperature profile of the ice and snow. I think doing this would provide a valuable insight into the true performance of the model itself (rather than the integrated performance of the model and its forcing). It may also provide valuable support for your model's efficacy, if you can shift some of 'the blame' (as it were) onto your forcing.

3 Specific Comments

L26: I don't think it's right that ICESat-2 takes 91 days to 'cover the entire Arctic' - I think its orbit repeats every month or so? Have you just taken the ICESat-column from Table 1 of Wang et al. (2016)? But I'm also not really sure what it means for a laser altimeter (which generates spot heights) to 'cover' the Arctic Ocean. I guess you'd have to define some time for every location to fall within some max-distance of a spot height - but I don't think Wang et al. do this. That's not even getting into the pole-hole issues. My advice is to avoid getting into this territory, and to ditch the specific numbers and reword. The point is a good one - altimeters have small footprints and only measure a line of points/footprints (depending on whether they're lidar/radar) underneath them, so they provide incomplete sampling.

L26: 'relatively high' relative to what? A lot of people out there would say that CS2 has a 'relatively poor' spatial resolution, relative to the typical length-scales of sea ice floes/ridges/leads, or a SAR-image pixel, or the resolution of a visible image. I guess you mean it's good relative to radiometers - so say this.

L30: I clicked on the Mecklenburg ref and it didn't take me to any literature that supports our ability to estimate SIT? It just says there's an L3 product in development...

L31: Say that this is PIOMAS, as it will come up again later and you're introducing it now.

L48: Retrieve daily rates, or retrieve *the* daily rate.

L50: I would add 'provided initialisation data is available'

L65: ‘as an’ should be ‘is an’

L75: Lagrangian should be capitalised here and elsewhere

L115: My understanding is that OIB had its final flight in 2019

L119: On L89 you describe the freeboard as the ”thickness of the sea ice above sea level”. But here you describe the ATM as retrieving sea ice freeboard, when it retrieves the ‘total freeboard’ (ice + snow). Needs a bit of clarification.

L120-123: I think you’re using the GSFC-NK product as described by Kwok et al. (2017). This is the quicklook product available from NSIDC (do correct me if I’m wrong). It’s pretty clear from Fig. 4 of that paper (Kwok et al., 2017) that the GSFC-NK method underestimates the BROMEX snow depths, probably due to the sidelobe issues explained in Kwok and Haas (2015). I think you need to point out that you’re probably using the most unreliable OIB snow depth product in terms of sidelobes, and that may introduce biases in your snow depths and SIT. See also Webster et al. (2014) who state:

> *However, the IceBridge product appears to underestimate thin snow depths in comparison to the in situ averages, and a clear discrepancy can be seen around 50 m along Transect 2 (Figure 4); this discrepancy is also apparent in the scatter plot (Figure 5) where the IceBridge quick-look and standard products estimate a snow thickness of ∼5-8 cm while the in situ mean is ∼23 cm.*

Table 1: I’d like to see the deployment date and the date of final data acquisition, so we can have some idea of the lifetime of the instrument.

Figure 1a: I think the size of the triangle potentially obscures some of the track here. Can you make the marker smaller or outline it in black or something?

Figure 1b: Parts of the OIB tracks are off the map edge here: make sure all your data are displayed in the figure!

Sect 2.4: At some point here you need to make the link to dynamical thickening, and the fact that buoys cannot observe it. As soon as their ice begins to thicken dynamically (something that can contribute a significant amount of thickness!) the buoy dies.

L173: Thorndike citation should not be in parentheses

L175: So are you using a parcel-tracking approach to get the second term on the right? If so, say so. If not, explain why not.

L234: I’d probably ditch the voltage/pressure analogy here - too colloquial in tone. And although the flows are described by the same maths, the physics is of course different. You couldn’t explain why heat flows by appealing to electrons or fluid flow.

L269: This sentence seems repetitive? Or am I missing something?

L270: Better define ‘too thick’ and ‘too thin’. What’s it’s too thick with respect to?

Figures 5 & 7: I suggest you plot the y=x line on these subplots to help the reader interpret the data.

271: I think the Bitz and Roe citation is warranted and useful, but ‘thin ice grows slower’ is not really *the phenomenon described* by B&R. The phenomenon they describe is how thicker ice thins faster in response to a climate perturbation. And they explain that by appealing to the ‘thick ice grows slower’ mechanism.

L274: Basal *heat* flux

L285: I think you should point out somewhere in this para that you're talking about *sensible* heat flux. There's also heat flux that occurs at the interface from release of latent heat, but I guess you're not addressing that?

L295: This is a great para, very useful to see this info set out like this.

L298: Medium is singular of media

L338: I think 'leading' is a non-standard term. I did a search for it and couldn't find any uses in the literature as you're using it. Perhaps consider 'lead-forming'.

L350: initialised at 0.05 m *thickness*

Figure 6: Like Figure 3, this is poorly arranged leading to the subplots themselves being too small. One thing that would have significantly improved the size of potential subplots would be to put things like [SLICE-CS2SMOS] at the top of the column, and not repeat it every row (since it's the same). It's also noticeable that the colorbar labels often are uncomfortably close to these labels. Again, subplot letter annotation can be moved so as to allow more vertical space into which subplots can be expanded. You can probably get away with one colorbar. Since you're only displaying data for first April, no need to include this in every subplot. It's also unclear why there's a gap between the middle two columns when the others are so closely squeezed together that colorbar labels impinge on the row labels?

L451/452: I disagree about how dynamic processes do not directly change sea ice volume. Within a satellite footprint or model grid cell, it is easily possible to have a lead open through local convergence which is not reflected by the ice motion vectors. This lead will freeze, but then often be 'closed' very quickly by ice dynamics because a refrozen lead is rheologically weak. This accordion-like action can happen quite frequently, and it can rapidly increase the mean sea ice thickness in an area (see literature in bibliography). Of course sea ice only forms by freezing seawater, and in this sense all ice is thermodynamically grown and the 'dynamical contribution to thickness' does not really exist. But in the sense of the literature, dynamical processes can produce ice very quickly in quite a small area, relative to congelation growth of already established ice.

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