Response to Referee #1 “Review of: A simple model for daily basin-wide thermodynamic sea ice thickness growth retrieval”

By Anheuser, et al.

Author responses in red.

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?

We have redesigned the figure to allow clearer viewing of its content. The panel-letter annotations have been moved to the side of each plot and the y axis range allowed to vary from plot to plot. Previous versions of this figure locked the y axis range between each so that vertical distance equated to the same thickness from plot to plot but by now allowing the range to vary, variations in the profiles over time are clearer. Variations in slope along both the SLICE and buoy profiles are more clear.

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?

This is a valid point. In order to more clearly validate SLICE’s ability to retrieve thermodynamic sea ice thickness growth, we have added new Fig. 3, shown below. This figure compares the input retrieved snow—ice interface temperature to buoy snow ice interface temperature and SLICE thermodynamic sea ice thickness growth rate (computed using buoy thickness) to buoy thermodynamic sea ice thickness growth rate.

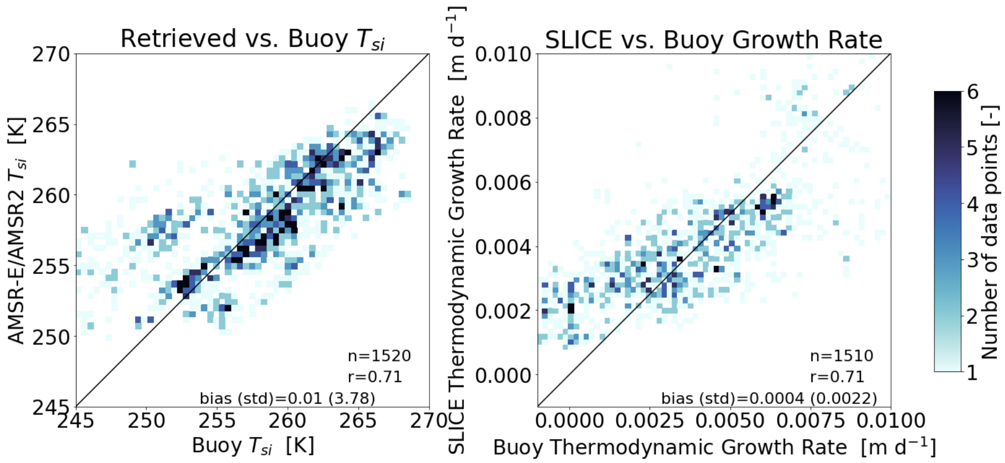

Figure 1: An instantaneous comparison of (a) AMSR-E/AMSR2 retrieved snow–ice interface temperature to buoy snow–ice interface tem perature and (b) SLICE retrieved thermodynamic growth rate using buoy thickness to buoy thermodynamic growth rate. Both exhibit a linear
correlation of 0.71.

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.

We have added the following passage to paragraph #2 of the discussion:

Buoys 2006C, 2012L and 2013G show a SLICE profile that produces greater sea ice thickness than the buoys. There are likely two mechanisms causing this error. For buoys 2006C and 2012L, the initial thicknesses are the two highest of the set and are near 3 m. In these cases, the cold atmospheric temperatures of the growth season have not yet reached the base of the ice, which must be below the freezing point in order for thickness to increase. In other words, the heat stored in the ice from summer has not yet escaped due to the higher thickness and greater heat storing capacity. SLICE assumes a linear temperature profile with the sea ice base at the freezing points---a condition that is not met in reality by buoys 2006C and 2012L until after 1 November. In the case of 2013G, a melt event, which SLICE is unable to capture, occurs in December. Both of these phenomena cause SLICE to overestimate sea ice thickness.

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

We have updated the tick labels in this figure to show the first of the month.

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.

The authors certainly agree with these points. Average sea ice thickness across a satellite footprint or L3 grid cell likely does not yield a perfectly accurate characterization of mean thermodynamic sea ice growth across that footprint. Additionally, leaving out deformation processes like ridging and lead formation does not capture all process affecting sea ice thickness. A perfect match of SLICE to CS2SMOS is not necessarily the goal. Even still, we believe it is appropriate to show the basin-wide results as it lends credence to the efficacy of SLICE. SLICE is a novel, alternative method for modelling sea ice thickness and can be applied on a basin-wide basis, even if we don't yet fully understand the implications of the results. We've added the following passage to paragraph #5 of the discussion:

Indeed, thermodynamic growth rate calculated using average thickness over a 25 km x 25 km grid cell likely does not completely accurately describe thermodynamic growth rate over the entirety that grid cell. Additionally, SLICE does not include the effects of deformation processes, which recently were shown to have contributed roughly 30% of total thickness growth during the Multidisciplinary drifting Observatory for the Study of Arctic Climate (MOSAiC; Nicolaus et al., 2022) field campaign (von Albedyll et al., 2022; Koo et al., 2021).

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).

Even with a lack of deformation processes, which we regard as a source of uncertainty rather than a disqualifying characteristic, SLICE compares similarly to CS2SMOS as PIOMAS. By apparently capturing thermodynamic growth better than PIOMAS, SLICE makes up for this source of uncertainty.

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.

This is a worthwhile exercise. We have added new Fig. 3, mentioned in Section 2.1 results.

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.

We agree that quantifying this aspect is not necessary, however referee #2 had asked for more detail to this passage. In an effort to satisfy both requests, we have added additional citations and background regarding existing sea ice thickness measurement and removed the quantitative discussion of the altimeter characteristics.

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.

You make a good point. We’ve removed this phrase and just replaced it with “However,”.

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...

We have updated this to a more appropriate citation of a reference that describes the algorithm and shows an initial validation.

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

We have added this.

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

This has been updated.

L50: I would add ‘provided initialisation data is available’

We have updated the sentence to:

“By using the satellite retrieved snow–ice interface temperature and an assumed initial ice thickness value in this relationship,….”

L65: ‘as an’ should be ‘is an’

This has been updated.

L75: Lagrangian should be capitalised here and elsewhere

This has been updated.

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

It appears there was an additional OIB flight in 2020. We have updated the manuscript accordingly.

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.

We have updated the sentence to:

"The ATM is a laser altimeter whose return signal is used along with an aerial photography based sea ice lead (fracture) discrimination algorithm to retrieve sea ice total freeboard height---i.e., freeboard plus snow depth---at 40 m spatial resolution."

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.

The Quick Look data is indeed as described in Kwok et al. (2017). We have added the following sentence to this passage:

"The data from 2014-2019, covering all of the data used here except those form 2013, are only available in the Quick Look format and may have increased uncertainties due to a roughly 5 cm underestimation of snow depths by the snow radar waveform retracking method Kwok et al. (2017)."

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.

We have added a final acquisition data column.

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?

We have replaced the triangle with a plus symbol which seems to be less intrusive to the path lines.

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

We have expanded the extent of this figure allowing for all paths to be viewed.

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.

We have updated this passage to be as follows:

"All buoys from the years 2003 to 2016 showing an entire season of sea ice thickness growth were used for comparison with the exception of buoys installed in landfast ice and those that show obvious ice deformation effects, which often lead to the end of data acquisition. As such, sea ice thickness growth observed by the buoys is taken to be caused strictly by thermodynamic processes."

L173: Thorndike citation should not be in parentheses

These has been updated.

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.

We have updated the last sentence in this paragraph to the following:

“Through the following methodology called Stefan's Law Integrated Conducted Energy (SLICE), we retrieve the first term on the right hand side of Eq. 1 and use a parcel tracking approach to approximate the sea ice advection component of the second term on the right in order to model basin-wide thickness.”

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.

This sentence has been removed.

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

This sentence could easily be seen as repetitive—we have removed the repetitive wording.

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

We have updated this sentence to the following:

“This means that in the presence of only thermodynamic effects, a SLICE sea ice thickness profile that is biased relative to ground truth will correct towards the unbiased SLICE thickness profile.”

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

We have added the 1:1 line on these plots.

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.

We have updated this sentence to the following:

“This relationship replicates the phenomenon described in Bitz and Roe (2004), whereby thick ice recovers from climate related perturbations slower than thin ice and has experienced greater thinning on a decadal time scale.”

L274: Basal heat flux

Thanks, this has been updated.

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 address- ing that?

Indeed we are working with sensible heat here. We have added the term “sensible” throughout.

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

Thank you.

L298: Medium is singular of media

We have updated this term to medium.

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'.

We have updated this term to "lead formation" in the two locations where "leading" was used.

L350: initialised at 0.05 m thickness

New ice parcels are initialized at a thickness of 0.05 m.

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?

The gap in the middle was to signify that this is a two column that has been split into two halves which are placed side by side. The titles also signify this so we have removed the gap. We have also taken your other suggestions, all of which have improved the figure greatly. We have also applied all of these changes to Fig. A2.

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 dy- namics 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.

This effect is indeed what we intended to describe by the following statement:

Though dynamic processes do not directly change sea ice volume, their changing of the thickness of ice at a given location does impact thermodynamic processes by virtue of f being a function of thickness, H, in Eq. (1). Inspection of Eq. (7) indeed shows that H impacts $\partial H/\partial t$.

By this we mean that within a given time step, thermodynamic and dynamic process are independent . As you describe and as we hoped to capture, dynamics do change the overall thickness field which effects the thermodynamic growth that increases volume. Indeed, we don't capture those types of processes. We will add the phrase "within a given time step" to the above passage.

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Response to Referee #2 “Referee report for “A simple model for daily basin-wide thermodynamic sea ice thickness growth retrieval” by James Anheuser et al.”

By Anheuser, et al.

Author responses in red.

The authors have made a significant improvement to the manuscript addressing my comments. Especially, I appreciate the authors’ hard work on the inclusion of sea ice drift in their methodology. The presented methodology and results in this manuscript are a novel and valuable addition to the sea ice community, but the manuscript still needs some clarifications. I, therefore, recommend the paper for publication following minor revisions.

Comments

Title: Although the key result of this study is the daily basin-wide sea ice thickness record, the new title does not include anything about sea ice thickness. I think it is better to show that this paper is about sea ice thickness in the title. How about something like “A simple thermodynamic model for simulating daily basin-wide sea ice thickness using satellite passive microwave measurements”?

We think the current title more accurately captures the content of the work. It is indeed a basin-wide sea ice thickness record, but only thermodynamic effects are captured. The title change from the first revision was a request from referee #1.

L2: I think it is not appropriate to indicate the algorithm of Lee and Sohn (2015), which was developed 7 years ago, as a “recently” developed algorithm.

We agree, “recently” has been removed.

L11: The word “equally” overstates the result of this paper. I suggest using “comparable” instead. Moreover, the authors should state clearly which quantities are comparable between SLICE and PIOMAS.

We have changed this passage to:

“basin-wide SLICE has equal linear correlation values to PIOMAS of 0.77 and 0.67 when compared against CryoSat-2 and Operation IceBridge, respectively.”

L24-L33: I think the literature review is still weak compared to other sections. The novelty and significance of this study can be highlighted based on the solid literature review. There are various sea ice thickness retrieval methods besides the methods using space-based altimetry only. For example, there is an algorithm for thin sea ice, simultaneous estimation of snow and sea ice thickness by combining satellite altimeter and radiometer measurements (Zhou et al., 2018; Shi et al., 2020), and the simultaneous estimation using two satellite altimeters at different frequencies (Kwok et al., 2020). Or if this study focuses on retrieving the sea ice growth rate, then relevant studies should be introduced.

We have added the studies recommended above to the introduction section.

L49-L50: The methodology also requires good initial guesses for sea ice thickness as well as passive microwave observations.

We have added “and an assumed initial ice thickness value”.

L65: It *as* an→It *is* an?

Yes, this has been updated.

L87-L106: I think these two paragraphs can be shortened and moved into the introduction section.

We have slightly shortened the first of these paragraphs slightly but feel the information contained is important background regarding the instruments providing the data we use in the rest of the paper. We also feel it belongs in the data section for the same reason.

L112: Where did you get the CS2SMOS data? Please provide data availability information.

This information is in the “Data Availability” section.

L123: Also please provide data availability information for the QuickLook product.

We have added a citation for the QL data and added it to the “Data Availability” section.

L129: Two acoustic rangefinder sounders positioned above and below the ice can measure sea ice thickness if there is no snow on sea ice. How can sea ice thickness be measured with snow presence?

We have added the following sentence:

“Winter sea ice growth is derived from the under--ice sounder.”

L185: I think the sentence “In Lee et al. (2018) ... in 1987” is not necessary.

We believe this sentence to be important as it describes the time period over which SLICE is viable.

L205: Explain why there are no upwelling and surface-reflected downwelling atmospheric TBs

in equations (2) and (3). I also want to confirm that the bias correction for estimated T_{si} is not applied in the revised manuscript.

The exclusion of upwelling and surface-reflected downwelling atmospheric flux was an oversight in the last revision. After including these fluxes in radiative balance equation, we find that the ~5 K bias, present in revision 1, remains.

In an effort to utilize unbiased snow—ice interface temperatures, we have adopted a new methodology for retrieving them. We have adopted the methodology described by Kilic et al. (2019). This methodology is a multi-linear regression between buoy snow—ice interface temperatures and AMSR2 brightness temperatures and includes a snow depth term. The results of this algorithm are accurate and do not show a bias, improving the findings of our own work. Additionally, this method does not require reanalysis data. We have updated Section 3.1 to reflect this new snow—ice interface temperature methodology.

Though this change to our methodology may seem significant, the paper’s objective of driving a simple thermodynamic sea ice model with satellite retrieved snow—ice interface temperatures in order to retrieve thermodynamic sea ice growth remains unchanged. Some of the basin-wide comparison results have changed relative to revision 2 but the overall finding of SLICE as an effective strategy for retrieving thermodynamic sea ice growth remains.

L209: horizontal, vertical→horizontally, vertically

This has been updated.

Figure 2: I found Figure 2 is not mentioned in the manuscript. Please mention it at an

appropriate place.

This must have been lost during revision, we have added the following:

Figure 2 shows snow-ice interface temperatures on November 1, 2013 derived from AMSR2 data.

L332: What density value is used for MYI? I can see value for FYI only. Authors may refer to the most recent research by Jutila et al. (2021) and Lee et al. (2021) on the sea ice density issue.

All ice added by SLICE is new ice that has not experience a melt season and therefore is FYI ice (even if it has been added to existing MYI ice thickness). As such, only FYI density is used by SLICE.

L347: It is a little bit strange that Figure A1 appears after Figure A2.

Yes, that is strange and has been updated.

L365: I see that SLICE sea ice thickness is generally greater than buoy sea ice thickness and the difference between them increases with time. It is better to make some discussion/explanation on these results.

We have added the following explanation to the discussion section:

Buoys 2006C, 2012L and 2013G show a SLICE profile that produces greater sea ice thickness than the buoys. There are likely two mechanisms causing this error. For buoys 2006C and 2012L, the initial thicknesses are the two highest of the set and are near 3 m. In these cases, the cold atmospheric temperatures of the growth season have not yet reached the base of the ice, which must be below the freezing point in order for thickness to increase. In other words, the heat stored in the ice from summer has not yet escaped due to the higher thickness and greater heat storing capacity. SLICE assumes a linear temperature profile with the sea ice base at the freezing points---a condition that is not met in reality by buoys 2006C and 2012L until after 1 November. In the case of 2013G, a melt event, which SLICE is unable to capture, occurs in December. Both of these phenomena cause SLICE to overestimate sea ice thickness.

Figure 3: Please explain the meaning of color (red and blue) in the figure caption. Y-axis has only two ticks, which is not quite informative. It would be better to make the figure more informative.

We have added the color description to the caption of and made changes to this figure to make it more clear. The y-axis range now varies from pane to pane and we have increased the number of ticks. We have also added grid lines for this purpose.

L395: The word “improved” may not be a good choice because CS2SMOS is not a ground truth measurement (snow depth, sea ice densities, etc. are assumed). It should be better to say such as “shows better consistency”. Besides, I think the reason why SLICE is closer to CS2SMOS than PIOMAS is that SLICE uses CS2SMOS sea ice thickness value for its initial condition. Therefore, it becomes logical circulation if more consistency with CS2SMOS means “improvement”.

Your critique is valid, we will replace “improved” with “lower”.

Figure 5: I suggest exchanging the x- and y-axis. It is generally easier to read plots with the reference variable on the x-axis.

We have made this change.

L405: I suggest removing the word “new”.

We have removed the word “new”.

L419-422: I don't think these sentences agree with the result shown in Figure 3. The difference between SLICE and buoy sea ice thickness increases with time even though the initial value is the same.

For most of the buoys, the bias doesn't increase significantly over time and the SLICE profile follows well. Prior to these lines, we discuss reasons why some buoy results are not as good.

L427: These assumptions are reasonable because of what?

These are reasonably because they are typical in sea ice analysis.

Figures 6 and 7: Is there a reason that K21 is not included in Figures 6 and 7, disturbing the consistency of the paper?

The K21 dataset does not include all of the years that we have used in this comparison. Rather than separating out the years into two figures, we have left K21 out of the comparison. Additionally, K21 includes its own comparison to CryoSat-2 data within the publication.

Figure 8: How did you calculate sea ice volume? Did you multiply sea ice concentration, grid area, and sea ice thickness? Is the 95% sea ice concentration criterion also applied to SC2SMOS and PIOMAS sea ice volume? Were three volumes calculated and compared based on the same area/criteria?

Sea ice concentration did not factor into the calculation. Each of the three volumes are total volumes directly from the product. SLICE and CS2SMOS volumes are thickness of all cells with sea ice multiplied by grid area. PIOMAS volume is taken from PIOMAS output data available at the link listed in the data availability section. We have added the following statement to the discussion of this figure:

The 95% or greater critera for SLICE may also contribute to differences, as the other datasets are not limited by this threshold.

L443-444: Sea ice motion product used in the SLICE method also includes near-surface wind vectors from atmospheric reanalysis.

We of course do not dispute this, however the sentence states that it is referring to the thermodynamics of each of the three products.

L479-481: Again, this conclusion can only be made upon a solid literature review. Please look for more state of art sea ice thickness observation methods.

We have added the studies listed above in the introduction section comments.

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