"Thermodynamic vs. dynamic Arctic wintertime sea ice thickness effects" authors' responses to referee #2

Thank you for the useful and constructive comments. Authors' responses are in red.

Review of "A climatology of thermodynamic vs. dynamic Arctic wintertime sea ice thickness effects during the CryoSat-2 era" by Anheuser et al.

This paper aims to quantify the components of dynamic and thermodynamic sea ice growth in the Arctic during the winter season from 2010 to 2021. The authors make use of three products: 1) The SLICE model (Stefan's Law Integrated Conducted Energy) providing thermodynamic ice growth and introduced in an earlier paper ("A climatology of thermodynamic vs. dynamic Arctic wintertime sea ice thickness effects during the CryoSat-2 era") by the authors of this study. 2) The AWI CS2SMOS sea ice thickness data set providing weekly sea ice thickness grids for the Arctic, and here used to derive total sea ice thickness growth. And 3) NSIDC Pathfinder sea ice motion data to derive advection of sea ice.

The topic is relevant and the approach using the SLICE model is interesting. In general, I think this paper could be interesting and a benefit for the sea ice and climate community. But from my point of view the study is lacking further information on methods (and may be required corrections), and a sound uncertainty estimation. I have a few major concerns:

Thank you for the positive comments.

The SLICE thermodynamic ice growth is subtracted from the total growth in sea ice thickness using the CS2SMOS product. Are the authors aware that the CS2SMOS sea ice thickness for each grid cell does not include open water? So hypothetically assuming that within a grid cell (with pure level ice) sea ice diverges, forming leads, the averaged ice thickness will be the same in CS2SMOS (especially for the CryoSat-2 domain). In other words, thickness=0, is not used for averaging. But I cannot see that this is considered in the current approach.

The SLICE methodology is only viable over 95% or greater sea ice concentration due to open water contamination of the passive microwave brightness temperatures and thus our analysis only applies to times when a given grid cell contains higher than this threshold sea ice concentration. While this was mentioned in the discussion and in the SLICE paper, it was an unintentional omission on our part to not mention this in the data and methodology sections. This condition means that even without considering the effects of changing sea ice concentration, the maximum error in a given term will be 5%. We have also added the below figure showing the percent of the total study time each grid cell is found to meet this criteria and add a 50% threshold in this metric for each grid cell above which results will not be reported.

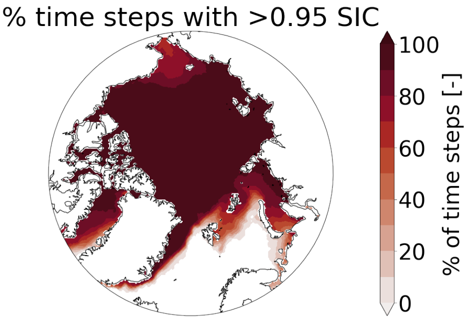

Figure 1. *Percentage of time steps with greater than 95% sea ice concentration for each grid cell. Much of the study area spends greater than 90% of the study period with over 95% sea ice concentration.*

Partly related to 1): I wonder how new ice formation in leads is affecting the overall findings in this study. It is not clear to me how this is handled in this study. SLICE does not seem to consider new ice formation in leads or am I wrong?

You are correct, we have no way of capturing new ice within a gridcell using the methodology of this paper. With a 95% sea ice concentration threshold, this will likely not be a significant source of volume generation. Nonetheless, have added a description of this issue to the discussion.

The way uncertainties are considered in this study does not seem sound, or at least needs further explanation. I would assume that the uncertainty of the climatological mean as calculated here (Eq. 6) is mostly affected by temporal (interannual and seasonal) variability. This needs some improvement from my point of view. Moreover, the SLICE uncertainty for the weekly thermodynamic ice growth in most of the Central Arctic is close to 0, which does not seem realistic, also considering the comparison with independent data sets in Anheuser et al. (2022).

A comprehensive uncertainty analysis is important here, since different input products are used, where either of them adds to the uncertainty budget. Uncertainty of the sea ice motion product is barely mentioned, but especially in the Fram Strait I believe this can lead to significant errors in the final retrievals, also since ice thickness is very heterogenous there.

These are good constructive critiques. We have replaced our standard error approach with an uncertainty propagation approach. Please see the updated section below. A key aspect to this calculation is that uncertainty is reduced through temporal averaging in creation of the climatologies, similar to regridding of satellite altimeter data as in AWI CS2SMOS. In regards to Polar Pathfinder uncertainties, DeRepentigny et al. (2016) found the weekly sea ice motion vectors to have a 7% error and the Tschudi et al. (2020) lists a maximum ice motion error of 0.7 cm s-1. Lastly, we will update the discussion section to reflect the new uncertainty section.

Uncertainty in the individual weekly observations of thermodynamic, dynamic, advection and deformation effect can be calculated using a general formula for uncertainty in a function of several variables (Taylor, 1982):

$$\delta_q = \sqrt{\left(\frac{\partial q}{\partial x}\delta_x\right)^2 + \cdots + \left(\frac{\partial q}{\partial z}\delta_z\right)^2}, \tag{1}$$

where q is the computed value; $x, \cdots, z$ are independent and random inputs to that computed value and $\delta_x, \cdots, \delta_z$ are those inputs associated uncertainties. Applying Eq. 1 to the terms as described in Section 3, we have:

$$\delta_{thm} = \sqrt{\delta_{SLICE}^2 + \left(\frac{thermodynamic\ growth}{CS2SMOS}\delta_{CS2SMOS}\right)^2} \tag{2}$$

$$\delta_{dyn} = \sqrt{\left(\frac{1}{\Delta t}\sqrt{2}\delta_{CS2SMOS}\right)^2 + \delta_{thm}^2} \tag{3}$$

$$\delta_{adv} = \sqrt{\left(\frac{u}{\Delta x}\sqrt{2}\delta_{CS2SMOS}\right)^2 + \left(\frac{\partial CS2SMOS}{\partial x}\delta_u\right)^2 + \left(\frac{v}{\Delta y}\sqrt{2}\delta_{CS2SMOS}\right)^2 + \left(\frac{\partial CS2SMOS}{\partial y}\delta_v\right)^2} \tag{4}$$

$$\delta_{def} = \sqrt{\delta_{dyn}^2 + \delta_{adv}^2}, \tag{5}$$

where δ_{thm}, δ_{dyn}, δ_{adv}, δ_{def}, δ_{SLICE}, $\delta_{CS2SMOS}$, δ_u, and δ_v are uncertainties in the thermodynamic growth, dynamic effect, advection effect, deformation effect, SLICE, CS2SMOS thickness, x direction component of sea ice motion vector, and y direction component of sea ice motion vector, respectively; u is the x direction component of sea ice motion vector; v is the y direction component of sea ice motion vector; Δt is time step size; and Δx and Δy are the grid box size. These uncertainty formulas do not account for covariances between the input terms. Though covariances may be present across the input data, inclusion of their effects on uncertainty is outside the scope of this work.

The uncertainty in SLICE is taken from Anheuser et al. (2022), who report SLICE to have a thermodynamic growth mean bias of 4×10^{-4} m d^{-1} and standard deviation bias of 2.2×10^{-3} m d^{-1} when compared against ice mass balance buoy data. Here we use this standard deviation as SLICE uncertainty. The analysis presented in Anheuser et al. (2022) does not include the effect of uncertainty in initial sea ice thickness, so we add the second term on the right side of manuscript Eq. 3 to account for the uncertainty in CS2SMOS sea ice thickness. Tschudi et al. (2020) lists a maximum ice motion vector error of 0.7 cm s^{-1}, which we use here for the uncertainty in the ice motion vector components. The uncertainty in CS2SMOS is calculated for each week and available in the data product. Lastly, the time step is one week and grid cell size is 25,000 m. Using these inputs, we calculate uncertainty in the thermodynamic growth, dynamic effect, advection effect, deformation effect terms at each time step and grid cell location.

When the terms are averaged to form Fig. 1, the uncertainties are reduced through the averaging. Applying 1 to an averaging operation, we have the following:

$$\delta_{mean} = \sqrt{\left(\frac{1}{N}\delta_1\right)^2 + \cdots + \left(\frac{1}{N}\delta_N\right)^2}, \tag{6}$$

where δ_{mean} is the uncertainty of the mean; N is the number of samples; and $\delta_1, \cdots, \delta_N$ are the individual uncertainties of each sample. Figure 2 shows the uncertainty of the mean for each of the processes studied here. The effect with the greatest uncertainty is deformation as it is a summation of uncertainties in the other terms due to it being calculated as a residual from those other terms.

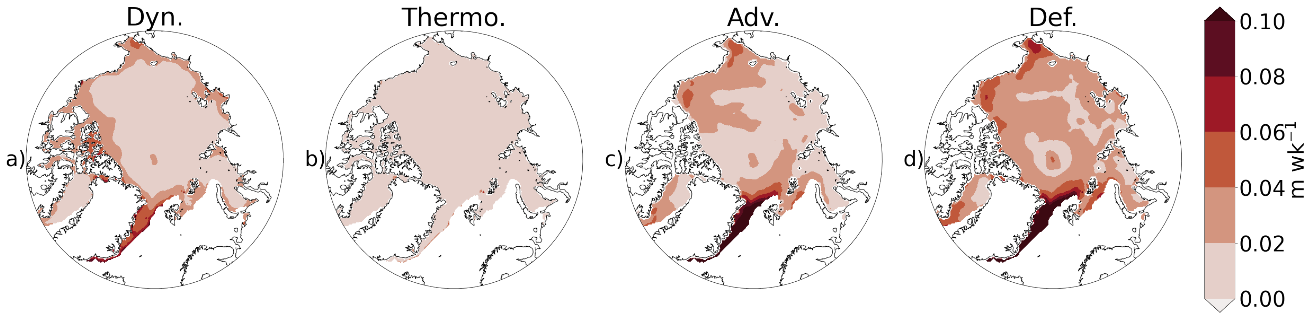

Figure 2. *Uncertainty for each grid cell during wintertime from late 2010 through early 2021 (except the winter of 2011-2012) in sea ice thickness changes due to a) dynamic effects, b) thermodynamic effect, c) advection effect and d) deformation effect. Uncertainty increases with a decrease in latitude as the number of weeks with ice cover decreases and deformation has the highest uncertainty due to being calculated as a residual.*

Given these points, I suggest major revisions are needed.

Specific comments: L51 & 55: AEM (airborne electromagnetic) sounding measures the sea ice thickness, not freeboard (can be retrieved only indirectly).

Thank you for pointing this out, we have made this change.

L66: There are some recent studies that already investigated the ice growth components and should be mentioned and cited here: e.g.: Petty et al. (2018), Ricker et al. (2021). The latter also compared observational ice growth retrievals with model outputs.

We have added these references and included a comparison with our results where appropriate.

L73-75: Why is CS2SMOS not mentioned here (and in the entire introduction) as it is used in thus study?

This is a good point. We have updated the section to reference the CS2SMOS product rather than only CryoSat-2.

L98: I suggest providing numbers here for the footprint (e.g. 300 m (along track) x 1600 m (across track)).

We have added this information.

L155: “so to can” . . . rewording

We have updated this.

L 174: Would it not be correct to use a three-point linear regression over [i-1,i+1], centered at “i”? Otherwise, the gradient is not centered on the target week “i”, but in between “i” and “i+1”.

We have tried this methodology and though it doesn’t appear to significantly change the results, we have implemented this in the next submission.

L193-194: “The most significant negative advection effects, less than 0.04 m wk-1, . . . ”. It is misleading when speaking of negative effects but then stating a "less than" a positive number, which could be again a postitive value.

We have updated this wording.

L197: Is negative deformation = lead formation?

Yes, in our interpretation a negative deformation effect is explained by lead formation and new ice filling those leads, thereby reducing the thickness of the ice within a grid box.

L465: Please state the version number of CS2SMOS. For most recent data and version history: https://spaces.awi.de/display/CS2SMOS/CryoSat-SMOS+Merged+Sea+Ice+Thickness

We have added this information.

Figures 1&2: The upper and lower limits of the color map seem to be saturated in some areas. I suggest to adjust the limits.

We have changed the limits on these plots.

Figure 6: Increase the resolution of the figure.
We have updated this.

References

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