Response to Referee #1

By Anheuser, et al.

Author responses in red.

I thank the authors for their detailed reply on my comments. Many of my questions and comments have been addressed sufficiently. I appreciate the effort of providing a more detailed uncertainty analysis.
But I still have some remaining comments, see below.

Thanks for the additional comments, they continue to help improve the paper.

First, on your reply to my question regarding ice growth in leads:

"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."
There are several papers I believe that point out the role of leads for ice growth, for example, Boutin et al. (2023), where they write: "We suggest a way to estimate the contribution of leads and polynyas to ice growth in winter, and we estimate this contribution to add up to 25 %–35 % of the total ice growth in pack ice from January to March". Of course, this is a model study, and also comes with uncertainties. But at least it seems that leads in fact play an important role for winter ice growth. I think this should be discussed in the paper and will help the reader to better understand the limitations and uncertainties here.

In the Boutin et al. (2023) methodology, the contribution from leads and polynyas is quantified as the thickness growth in the pack ice that falls in the "new ice" category, defined as any ice below 18 cm. Some of that growth will indeed be included in our thermodynamic growth and likely some of that will be included in the residual, though it is unclear how much without a more detailed analysis, which is outside the scope of this study. As such, the 25%-35% number is not an accurate estimate of how much thermodynamic growth will be missed by only considering sea ice above 95% and not accounting for sea ice concentration changes. It is only the initial closing of the open water lead that is missed in our methodology. We have added a bit to the discussion of this issue:

It should be noted that new ice formation in leads that have opened up due to divergence in the flow field will not be included in the thermodynamic effect; rather, the balance of the new ice thickness and the thickness of the remaining ice in the grid cell will be quantified as negative residual effect. This type of effect has the potential to be greater than 5%, as the leads may grow to arbitrary size and not be apparent in the passive microwave sea ice concentration product if new ice fills the lead prior to a new passive microwave brightness temperature is taken. It is this initial closing of the lead with new ice that will not be captured as thermodynamic growth.

L238: Regarding formula 16, I am not sure if the uncertainty of the mean is reduced in any case. Why should it? You are not averaging observations of the same subject (same piece of sea ice), but spatially (or temporally). Yet, I am not an expert in error statistics, and I acknowledge that some assumptions have to be made here.

It may not be the same object we are observing but it is the same location for which we are determining the climatology. If the climatology is a mean of all observations at that location and the observations have random and independent uncertainties, these uncertainties reduce as additional measurements are taken. In this latest revision, Eq. 16 is applied only for temporal averaging; we found that applying this equation to the regional spatial averaging in Fig. 5 led to unreasonably low uncertainties. We are not experts in this either, merely working to most accurately characterize the uncertainty.

Figure 7: I think it would improve the readability to include a legend explaining the different lines and colors. Moreover, I would add the uncertainties here, which would provide useful information on how robust those estimate are. Otherwise the uncertainty estimate are barely used in the paper, only showing the mean fields in Fig. 5.

We have updated Fig. 7 (now Fig. 5 in the revised manuscript) the plot to a bar plot in order to better compare results with existing literature and made sure the legend is clear. Along with this, we have added uncertainty bars to this plot as well as uncertainty shading to Fig. 10.

Response to Referee #2

By Anheuser, et al.

Author responses in red.

Following the first reviews of this paper the authors have made extensive additions to the paper. The work on presenting uncertainties to the estimates are theoretically sound and the additional figures are welcome. However, I can still not suggest this paper for publication and additional major corrections are required. As this is a second review session is it up to the editor whether to pursue this publication to a third round. At the moment the results of the paper are speculative and are lacking context with other budget estimates. Here are the major points:

Thanks for the additional comments, they continue to help improve the paper.

While uncertainties have been calculated and discussed, all reported values and line plots within the paper have no error estimates included. The calculated estimates must be included within these figures and numbers. In particular, figures 7 and 10 need error bars, and the total numbers presented in section 4 need +/- values after each one.

We have added error bars to Fig. 7 (now Fig. 5 in the revised manuscript), shaded error regions to Fig. 10 and uncertainties to reported values in Section 4 where appropriate.

The total budget values, that are the main headline results of this paper need to be contrasted to existing literature. The use of SLICE to estimate total winter thermodynamic growth is a bold but useful result. The context of how these numbers fit with existing published values need to be added to allow the results presented here to be used in any future work. For example, how do the seasonal total or averaged weekly budget values given here compare to those given by Ricker et al.? As mentioned in the previous review, the values given here for dynamic changes (or all the possible residual effects) to sea ice volume are higher than given in previous studies, with values comparable to thermodynamics. Previous work typically has dynamic changes at 1⁄4 of thermodynamics. This is seen in Ricker et al. (2021) and also within all the models shown in Keen et al. (2021). A direct comparison between these total budget values, within the context of the given uncertainty estimates is required for the community to understand the usefulness and accuracy of these new presented results. A table putting all these values together will be helpful. Do the results presented here suggest we need to rethink where sea ice grows in the arctic? Does this paper generally agree with previous work on where ice grows? At the moment this paper just adds confusion to these questions, and this leads a reader to discount the results given here. The current presentation of uncertainty compounds this, as the given maps in figure 5 suggest that the given results are very accurate, but with no presentation of context to existing estimates, the reader is likely to doubt both these budget estimates and uncertainty estimates. The very low reported estimates for thermodynamic uncertainty adds to this (figure 5 has it at near zero). To report that a new experimental data product has near zero uncertainty is highly suspicious and leads to the conclusion that both the data and uncertainty estimates of this product are unreliable. As mentioned in the previous review, a significant aspect of this study is a presentation of the usefulness and context of the SLICE data. This aspect is currently not given enough discussion, and is not mentioned at all within the abstract.

Thank you for this very helpful critique which we feel has greatly improved the paper. We have changed Fig. 7 (now Fig. 5 in the revised manuscript) to be a bar plot of volume changes and changed the regions to match those of Ricker et al. (2021) to enable a more direct comparison. We have also added a plot to this figure showing effects summed across the entire Arctic for comparison to Keen et al. (2021) and added a bar for volume changes from CS2SMOS that occur in areas below the sea ice concentration threshold. Please see below:

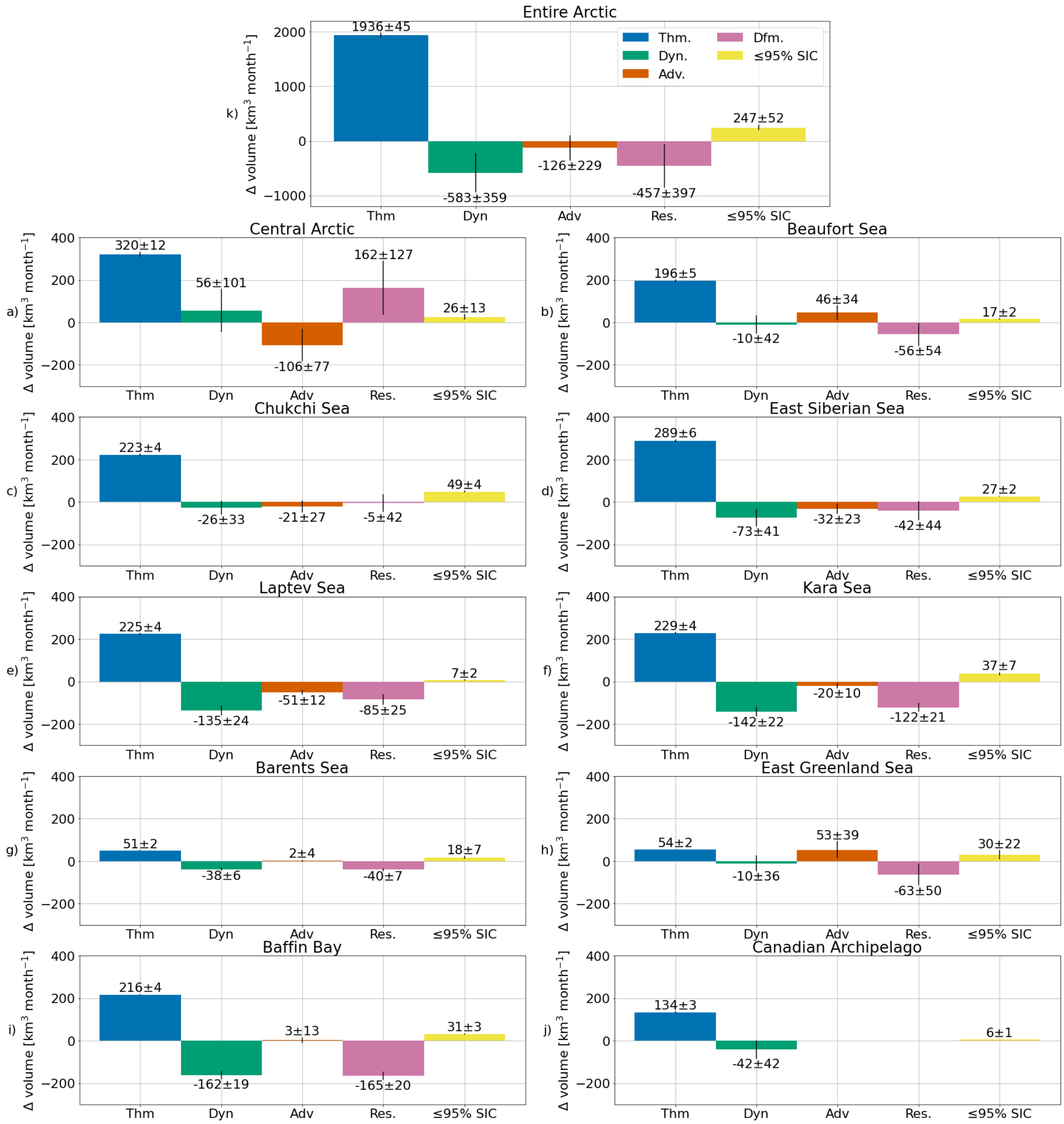

Figure 5: Mean monthly volumetric thermodynamic growth (blue), dynamic effect (green), advection effect (orange), deformation effect (pink) and ≤95\% sea ice concentration volume changes for a) the entire Arctic, b) Central Arctic, c) Beaufort Sea, d) Chukchi Sea, e) East Siberian Sea, f) Laptev Sea, g) Kara Sea, h) Barents Sea, i) East Greenland Sea, j) Baffin Bay and k) Canadian Archipelago. Overall the entire Arctic, dynamics has a negative volume effect that is -25% that of thermodynamic growth.

What we see is that the regional and basin-wide results in fact agree well with Ricker et al. (2021) and Keen et al. (2021). The new information offered by this work does not require a rethinking of sea ice dynamics but rather adds new spatial detail showing some regions deviate significantly from these mean values.

As a part of developing this new figure, we have revisited our uncertainty methodology. To address unreasonably large uncertainties in the dynamic components of this plot, we have added covariances to the time and spatial derivatives of CS2SMOS. We have added the following to Section 3.1 in addition to updated the uncertainty equation and Fig. 4:

The uncertainty in the space and time derivatives of CS2SMOS contain covariance terms. CS2SMOS uncertainty is a significant source of uncertainty within the uncertainty framework above but some portion of this uncertainty would cancel when a difference between time steps or neighboring grid points is taken. Though these covariances have not been explored in literature in relation to CS2SMOS, we look to Fig. 7 within Ricker et al. (2017b) for guidance on correlation between grid cells within a single CS2SMOS field. For the example region depicted in this figure, correlation between thickness observations at grid points located less than 100 km apart are nearly always greater than 0.6. This 100 km radius includes neighboring grid points which are separated by 25 km and displacement during the two weeks between time steps in Eq. 6, which typically won't exceed 100 km. Based on this figure, we assume a correlation between time steps or neighboring grid points of 0.6 as a conservative estimate. Other than in this instance, our 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.

Finally the method of only considering ice of high concentration is sensible in the context of this paper and the SLICE. However, when presenting the total budget values for a season, this study needs to include an estimate of all the volume changes that are not included when ignoring low ice concentrations. While this may be a small number, the reported thermodynamic growth in certain areas (central region) is also small, and the volume change during low sea ice concentration events may be significant.

As mentioned above, we have added a bar to Fig. 7 (now Fig. 5 in the revised manuscript) showing, for each region and the entire Arctic, how much volume growth occurs below the sea ice concentration threshold per CS2SMOS. Across the entire Arctic, this volume growth is 13% that of thermodynamic growth.

The above point with additional considerations from line 449 in the manuscript, can be added to the explanation of the residual data. The missing low concentration contribution, plus additional lateral and new or frazil ice growth (see Keen et al. for all of these), will be apparent within the residual field.

We have added the following to the methodology:

The residual difference of the overall dynamic effect and this advection effect includes the effects of ice deformation and any other effects that are not accounted for in SLICE or the calculation of advection. These additional effects include lateral growth, snow ice formation and any frazil or new ice growth that occurs above 95% sea ice concentration and is not captured within SLICE.

Specific comments:
Figure 2, and then throughout, why is the season 2011-2012 not included? I guess for a technical reason, but this this needs to be clearly stated in the data or methods section.

This was mentioned in the methodology Section is found at line 214.

The 2010 data begins on 15 November rather than 1 November along with the availability of CryoSat-2 data and the winter beginning in 2011 is not included due to a gap between availability of passive microwave data from the earlier AMSR-E and latter AMSR2.

Figure 7. This figure will benefit from the total dh/dt values as well as the components. Is the 'deformation' the same as the 'residual' given elsewhere? If so then it needs relabelling. Uncertainties need to be added (whisker plots or shaded regions). The units are confusing, the y-aixs of m/week clashes with the time period of monthly data.

Please see above explanation of changes to Fig. 7 (now Fig. 5 in the revised manuscript). Additionally, we have updated all figures to be in a m/month unit. We agree this makes the results more streamlined and intuitive.

Figure 10, an improved caption with all lines showing which is from this papers budget calculations and which are from other data is need. Uncertainty values need plotting too.

We have added "from our results" where appropriate in this caption and added uncertainty shading.

L 449, additionally there is new and frazil ice growth terms. See Keen et al. These are considerable and comparable to ice deformation effects in some models.

We have updated this sentence to:

The SLICE thermodynamic growth retrieval also does not account for lateral melt and freeze processes, or any new or frazil ice growth that occurs above 95% sea ice concentration.

Keen, A. et al. 2021. An inter-comparison of the mass budget of the Arctic sea ice in CMIP6 models. *The Cryosphere*. 15, 2 (Feb. 2021), 951–982. DOI:https://doi.org/10.5194/tc-15-951-2021.

Ricker, R. et al. 2021. Evidence for an Increasing Role of Ocean Heat in Arctic Winter Sea Ice Growth. *Journal of Climate*. 34, 13 (Jul. 2021), 5215–5227. DOI:https://doi.org/10.1175/JCLI-D-20-0848.1.