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:

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.

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 ¼ 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.

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.

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.

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.

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.

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.

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.

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.