This paper documents the estimation of thermodynamic and dynamical components of sea ice volume changes during the Arctic growth season. The authors have combined 3 data sets: the AWI-SMOS weekly sea ice thickness data, the Polar Pathfinder sea ice drift data and the authors new SLICE, brightness temperature derived thermodynamic ice growth estimate data. The final results presented are quantified and thoroughly compared to ice volume change studies. Such observational derived estimates of ice volume change are challenging to produce, and the authors must be commended at the efforts they have made. However, if this study is to be of use to the wider scientific community, much more information needs to be included. Readers of this paper must then be informed of the conceptual consequences of using the data presented. At the moment very limited information is included about the most appropriate interpretation of the presented results. From this reviewers point of view, this study is as much an accuracy assessment of the SLICE data as an estimate of the components of Arctic sea ice thickness change, though this aspect of the paper is mentioned very little. A more thorough description of the issues are described below.

My main issue with this paper is the incorrect definition of uncertainty used that causes misleading claims within the results and discussion section. The equational form of (6) will give the interannual variability of the budget terms, with no information of the observational uncertainty given. When uncertainty is described for these observational estimates, the reader expects to see information included on how much we can trust these estimates based on how well defined the original measurements are. What role does this uncertainty have in the final budget calculations? The discussion section does attempt to discuss the role of observational uncertainty in the calculations of the paper, though these are compared to scarcely believable claims that the SLICE data has an uncertainty of less than 1mm/ per week. While the uncertainty in AWI-SMOS is plotted, no accurate information on how this plot was calculated is given, making it difficult to interpret. Uncertainty measurements are included in the Pathfinder data set, and I suggest that similar plots need including too. Pathfinder uncertainties can often be as high as 30-50%. As the authors are the creators of the SLICE dataset, then I expect them to also include similar plots showing the uncertainty in this data also. Possible covariances within the data are not mentioned at all. This has the potential to be the highest source of uncertainty in the final values presented. While calculating covariances may be beyond the scope of the study, the possibility of the occurrence needs to be presented.

The values given in this paper with claims such as 'error is highest in the East Greenland, Barents and Kara Seas, where motion vectors are largest and most variable' are telling us only about which regions are most variable from week to week, and from year to year. Indeed it may be possible that such regions have lower observational uncertainty than regions given here that are said to have low uncertainty, when the metric supplied by this study are only indicate that these region have a low interannual variability, or indeed constant values over the growth season.

The first main issue leads into the second limitation of this study: the limited plotted data supplied in the paper. Arctic sea ice is widely described to have large interannual variability, for example in minimum extent and volume. The results presented and discussed in this paper are mostly from 10 year climatologies, that by definition will remove all such variability. It is thus then difficult to assess the accuracy and role of the different budget components. Each yearly total is shown in an additional plot in the appendix, and a table is supplied quantifying the key regions. No example weekly calculations are presented at all, which is a crucial omission as this is the time scale of the original calculations. To adequately document the calculations performed for this study, and to be of use to modelling studies, time series of the regions presented can be included. An example figure showing the input and output data from a single week will be a very useful inclusion. Uncertainty data for the shorted timescales needs including too. The final data presented in this paper comes from short time scale calculations. Uncertainty in these calculations comes from these timescales also.

Another concern is due to the definition of the budget equations and the interpretation of this definition. There are three main sources of volume change of sea ice used: rate of change of thickness data, the advection of sea ice thickness, and the new brightness temperature derived thermodynamic ice growth. The final deformation-based estimates of ice volume change is given as the product of the three input data sets. This is conceptually fine, and the results of this method will be of use to the scientific community. However, the authors must tell the reader that it is their own interpretation that gives the difference between the input data as deformation. There are many points within the paper where the deformation values are listed alongside the other components as an additional data value. What is a more accurate definition is thus:

The rate of change in volume and advection estimates are compared to a novel source of data on the thermodynamic change in sea ice thickness. There are relatively large differences between these two data, which are presented here. When considering the source in this difference as from sea ice deformation, these results are the consequence.

This is a more accurate and conceptually more useful description for the wider community. As mentioned above, the final deformation values are thus highly dependent on the uncertainty in the input data. The magnitude of the deformation estimates must be compared to the magnitude of any uncertainty.

The previous concern brings up two notable omissions from the data presented in this paper. First the lack of ice concentration data to convert sea ice thickness to sea ice volume, and then the use of the ice drift data to create an estimation of sea ice divergence. The two data sources can then be used to capture wider scale deformation data and compared to the emergent deformation. Indeed, divergence rates are discussed in this paper, but without presented calculations on the deformation rates then this is purely supposition. The authors comment that the use of Pathfinder for vector calculus applications is not appropriate, if this is so then how can the results of this study be accurate?

Specific points:

L3 and 67 This is not the first such study, see Ricker et al. (2021)

L 80 Previous studies using similar methods to those presented in this paper (ricker et al. 2021, Holland et al. 2016) both also use an ice concentration data set too. This data is crucial in high divergence/advection areas (say the Greenland Sea). Ice thickness products using the

Radar Freeboard method (as presented here) will not capture the loss in ice volume due to ice lead opening.

L 153 What is meant exactly here as ‘Deformation effect’? Is it the divergence term that is listed as due to deformation in the previous sentence?

Equations 4 and 5 and the list on lines 174 - 178 Please reformat ‘dynamic effect’ and others using the normal text rather than equation text format.

L 206 This measure of uncertainty appears to be the interannual variability - the standard deviation of measurements over 10 years of weekly data.

L 211 This is a correct reason for uncertainty, but it is equated to a map of variability.

Figure 1. It will be good to see the mean total change in volume on this plot too as it is the main input data for these calculations.

L 239 It is important to express for this experimental data set, that the results obtained in this paper suggest that ‘it experiences a decrease in thickness due to deformation via lead formation’. The data presented here does not observe lead formation, so this must be presented as the authors explanation of the observed results. This is especially true for the interpretation of a 10 year climatology, and even more true for budget calculations that do not incorporate ice concentration data, and thus will not capture the spatial divergence of ice within the time derivative section of equation (2). This rephrasing is crucial to perform throughout the paper.

L 243 ‘as the flow pattern deposits ice and leads to ridging’ again this is not directly observed by the data presented and must be presented as the authors interetation.

L 248 ‘negative effects from both advection and deformation’ again misleading. The advection is an emergent signal present in the data. The deformation is entirely from the resultant difference in the data, and its role as deformation is entirely the authors interpretation. This must be expressed as such.

L 249 ‘location and strength of the Beaufort Gyre’ has this been quantified anywhere? Or is it up to the reader to interpret this entirely from the plotted arrow vectors? If it is from the arrow vectors, please indicate this in the text, as this is a highly interpretive method and not at all accurate. There are other studies that presented data on the Beaufort Gyre and may be a useful inclusion here.

L 251 More information on figure 4 is required. What time scale are these plots calculated? The ration taken at weekly time scales, or over the whole 10 years?

Figure 6: More information is required for this figure caption. Which data come from this study and which are from the MOSAiC campaign? What timescale are the various data on?

L 284 Again, ridging is not observed in this study. What is shown here, is entirely an observed change in sea ice volume that cannot be accounted for using advection or thermodynamics growth estimates. This needs to be written as such before the results can be of use.

L 292, If divergence is discussed, then it really needs to be calculated too.

L 296. It is the other way around. Positive deformation emerges, that can be accounted for through ridging.

L 305, why are they not suitable? Has this been tried? Are the results less certain than the estimation presented in this paper?

L 356 fix equation number.

L 366 Can this claim be backed up? I see no discussion on the uncertainty of the SLICE data. As this is a new data sourced, then it needs to be thoroughly analysed for its accuracy and reliability.

L 384 the observational uncertainty here is compared the data variability. These are not the same thing.

Figure 7. How are these values calculated? Is it the mean uncertainty value taken from the data product?

L 393 I find an uncertainty of 1mm/per week for this data source to be somewhat to good to be true.

L 407. So ice concentration data was used in this study? What data is this?

L 417 A calculation of the resultant uncertainty in advection can thus be calculated. This needs to be quantified.

L 442, Needs to be: where the SLICE thermodynamic growth data can only account for (a third possibly) of, on average, the total observed ice thickness change.

L 447 – this data is not yearly. This is the week to week data surely?

L 453 This claim cannot be made as this is the only uncertainty shown. The uncertainty in Pathfinder and SLICE needs to be shown in similar depth.

L 455 this is a comparison between uncertainty and variability and highly misleading.

Appendix - there is no text in the appendix. Add this figure to the main body of text.

Ricker, Robert, Frank Kauker, Axel Schweiger, Stefan Hendricks, Jinlun Zhang, and Stephan Paul. 'Evidence for an Increasing Role of Ocean Heat in Arctic Winter Sea Ice Growth'.

Journal of Climate 34, no. 13 (1 July 2021): 5215–27. https://doi.org/10.1175/JCLI-D-20-0848.1.
Holland, Paul R., and Noriaki Kimura. 'Observed Concentration Budgets of Arctic and Antarctic Sea Ice'. *Journal of Climate* 29, no. 14 (15 July 2016): 5241–49. https://doi.org/10.1175/JCLI-D-16-0121.1.