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Interactive comment on "Evaluation of Sea-Ice Thickness from four reanalyses in the Antarctic Weddell Sea" *by* Qian Shi et al.

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Review of The Cryosphere Discussion submission tc-2020-71 'Evaluation of Sea-Ice Thickness from four reanalyses in the Antarctic Weddell Sea'

GENERAL COMMENTS

The accurate large-scale measurement and reporting of trends in Antarctic sea ice thickness are two of the major challenges for contemporary geophysical science. Once developed, giving these trends context amongst the multiple drivers of Antarctic sea ice volume change, from natural and anthropogenic forcing over multi-decadal timescales will present and even greater challenge. Given their limited temporal resolution, this cannot be achieved using observational datasets alone. This manuscript evaluates sea-ice thickness from four reanalyses against observational data in the Weddell Sea sector of the Southern Ocean. Reanalyses can play an important role in providing context for change during the observational period, and this work has impressively pulled together a large amount of data, from a variety of sources to provide an evaluation.

It is interesting to see these results and impressive that monthly sea ice thickness distribution is generally represented as expected in this sea ice sector. The simplification of the real world in these reanalyses is obviously a major concern particularly regarding their resolution, this is not a critique of this work, but a description of the current state of the science. The major limitation appears to be that these reanalyses are struggling to capture the thicker end of the sea ice distribution in the Weddell Sea (and from other reading, the entire Southern Ocean) and therefore omitting an important segment of the statistical information on the sea ice thickness distribution. This also highlights the fact that they are failing to, and often provide no attempt to simulate sea ice dynamics, a process at the core of Antarctic sea ice production. Their sparse resolution and missing physics related to smaller scale deformation processes is a serious limitation. There are clearly major developments to be made before these reanalyses datasets can be used to inform the community about sea ice thickness trends in the Southern Ocean. This work successfully highlights this current limitation.

I find the manuscript meets the set criteria for publication in The Cryosphere but I do have some concern related to the 'Originality' and 'Significance' criteria. Although the particular area of study and reanalysis models are on the whole original, when placed in a wider context, other work has already visited this question (Uotila et al. (2019) is an intercomparison while Massonet et al. (2013); Holland et al. (2014) are reanalysis assessments also comparing results to observational data). It is difficult to gauge how valuable continued comparisons are, especially related to the difference/variability of the physical processes that construct sea ice thickness in the models. For instance, have significant advancements been made in the reanalyses evaluated here, since the publication of other studies to warrant new evaluations? I have no specific recommendation for this. This is perhaps beyond the scope of the expected review process though I feel it is important to highlight it.

With the consideration of this concern above left to the discretion of the editor, I support the publication of the manuscript if the specific points outlined below are incorporated into the work. I would like to place specific emphasis on the tightening up of the results section and an attempt to provide more quantitative conclusions.

SPECIFIC COMMENTS

1. Readers may benefit from a concise explanation of the general principles of sea ice thickness estimates from reanalysis products. It will be difficult for non-experts in this specific field to grasp the processes considered and limitations during the construction of a sea ice thickness reanalysis product. I appreciate this is the point of the references but it is often helpful to provide an insight as part of the text to assist the reader (supported by references).

2. I appreciate it is sometimes difficult to fit all the relevant information into the limited word count of an abstract, but I think the reader (and work) would benefit from some sort of quantified reporting in the abstract. Terms like 'well reproduce' are somewhat subjective, is there a way to effectively provide a quantitative measure in the abstract of how well these reanalysis perform compared to one another and the observations? i.e. report the key results in a quantitative manner. This could in some way be related to a 'score' suggested in point 5 below.

3. Although the manuscript is written well and results are well displayed, it would be useful to maintain the colour coding of each reanalysis/observational datasets throughout the figures to avoid confusion.

4. It is clear that the reanalyses underestimate the sea ice thickness distribution when compared to ICESat-1 but maybe some attention should be given to the description of this comparison in the results. "ICESat-1 thickness is much thicker than that of

the reanalyses except GIOMAS in Spring-ON" – is this entirely accurate? GECCO2 also has two instances where the modes are similar when compared to ICESat-1, in Spring-ON (2007) the mode is higher in GECCO2, Spring-ON (2006) it appears to be the same. NEMO-EnkF also has two examples in Autumn-FM where the modes are higher (2006) and similar (2005). Is GIOMAS in Spring-ON really that notable? I am a little confused by the plots in Figure 5 from visual inspection - why does the ICESat-1 thickness distribution change in the same season and year for the SOSE comparison? i.e. the ICESat-1 distributions seem to stay the same in the PDFs in the same year/season for the other plots but the distribution is shifted in the SOSE plots. Is this to do with some different sampling from different geographical coverage of the reanalyses? In addition, why is 2007/2008 omitted for Winter-MJ and 2008 for Spring-ON? Was this decision driven by a lack of ICESat-1 data for comparison? It is stated in the text (L264) that the 'we compare sea-ice thickness from four renanalyses. . .with that from ICESat-1 for the period from 2005 to 2008' but this does not appear to be the case in the corresponding figure. In Table 2 the ICESat-1 measurement periods are described and 2007 (Winter-MJ) does not have a '-' indicating the data is absent but instead 'Winter-MJ' is written. Also 2004 is shown but does not appear to be part of the described analysis, is there a reason for this? My main point related to this section of the study is that there seem to be some discrepancies in how the data is described and how it is presented in figures. This needs to be looked at and all data and descriptions must be consistent.

5. I would expect that the community would look to evaluations like this to understand what reanalyses could be useful for supporting their own work. As the manuscript currently reads, it is difficult to digest and really understand the limitations of each of the reanalyses. It may provide some clarity and assist the readers understanding of the results to have a table with the key parameters the authors are trying to evaluate (including but not limited to thickness, relationship between mean/mode, min/max thickness accuracy, spatial accuracy, sea ice growth/seasonal evolution of thickness, open ocean vs. coastal regimes, ice motion –divergence/convergence) and a score

evaluating how well they have performed. This is not an explicit request, but just a suggestion for the authors to consider in order to improve the communication of important information from this work.

TECHNICAL COMMENTS

The temporal span of this investigation isn't immediately clear. Please make clear by including the time frame of the inter-comparison in the abstract and introduction (the analysis overlaps are written at the beginning of Section 4). I understand it is intermittent given the different lengths of the observational datasets and different reanalyses but some indication via a well worded summary, early in the manuscript would be useful.

L28 – 'crucial component of the Earth system', understand what authors mean but perhaps more specific 'climate system' for example.

L89 – add 'a' between 'introduce' and 'sea-ice'.

Section 2.1 $\sim$ L110 and L130 - should a spatial resolution be reported for GECCO2/GIOMAS as is given for the other reanalyses? I see they are in Table 1 but why report some resolutions in the text and not others?

L118 and L124 – 'âĄř' used in one instance and 'degrees' in another, perhaps adopt one standard.

L136 – to be absolutely accurate perhaps reword '(the part above the sea level)' to '(combined ice and snow height above local sea level)'.

L140 – 'suggested by Worby'? Is a complete reference available?

L149 – I understand that the limitations of radar altimeters are not the focus of this study but the complexity of the technique/its limitations in the Antarctic are understated by these few sentences. Perhaps include reference to other studies highlighting this to provide the reader with some context if they require it. This takes me to another point, it doesn't appear CS-2 is used in the analysis, why is it described in the data section?

L149 – More accurate to say 'the radar altimeter is expected to' (and then provide relevant references) as opposed to 'the radar altimeter can measure'.

L159 – Use acronym 'ULS' once it is provided and throughout manuscript use acronyms/abbreviations once they are supplied e.g. L199 'Antarctic Peninsula' to 'AP' as it is shortened on L195.

L164 – I don't think 'skilful' is appropriate here, perhaps 'accurate' or 'approximates thickness well' or something similar.

L167 – What are these uncertainties? 5 cm/8cm/18 cm etc? Are they a spread around the mean +/- 5 cm or direct positive deviations from other reference measurements? If so are there references for these expected accuracies?

Figure 1 – Standard deviation is abbreviated to SD in the figure but to STD in the text, these should be consistent.

Figure 3 caption – Capitalise 'southern coast'.

Figure 5 – Thickness (m) is not actually labelled on the y-axis. Insert '(red)' after second mention of ICESat-1 in the caption.

L290 – 'this means that the reanalyses may not well represent coastal processes' – what do the authors specifically mean here in reference to sea ice? Dynamics and convergence against the coast? Interaction with the coastline? Inaccurate bathymetry or coastal currents? Some of the concluding statements are a little vague. I think the study would benefit from being more specific and shed light on the limitations that need to be addressed.

L290 – Why is SOSE not included in the spatially averaged differences here?

L325 – 'primary' to 'prime'.

L362 – insert ‘satellite’ before ‘altimeters’.

L369 – ‘still’ before ‘been’.

L388 – ‘improve’ to ‘improving’.

L388 – ‘assimilate’ to ‘assimilating’.

Figure 6 – What time period is this data comparison for? Are they seasonal averages for all years?

In acknowledgements – ICESat-1 data is provided by NASA and NSIDC not ESA.

REFERENCED EXISTING REANALYSES

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Uotila, P., Goosse, H., Haines, K. et al. An assessment of ten ocean reanalyses in the polar regions. Clim Dyn 52, 1613–1650 (2019). https://doi.org/10.1007/s00382-018-4242-z.

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