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The Cryosphere
Discussions

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

Holland, P. R., N. Bruneau, C. Enright, M. Losch, N. T. Kurtz, and R. Kwok, 2014: Modeled Trends in Antarctic Sea Ice Thickness. J. Climate, 27, 3784–3801, https://doi.org/10.1175/JCLI-D-13-00301.1.

Massonnet, F., Mathiot, P., Fichefet, T., Goosse, H., König Beatty, C., Vancoppenolle, M., and Lavergne, T.: A model reconstruction of the Antarctic sea ice thickness and volume changes over 1980–2008 using data assimilation, Ocean Modelling, 64, 67-75, 10.1016/j.ocemod.2013.01.003, 2013.

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.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-71, 2020.

The Cryosphere Discuss.,
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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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I would like to start by pointing out that I was asked to serve as a reviewer in June.

This manuscript evaluates the Southern Ocean sea ice thickness produced by four reanalyses against observations from AUVs, ships and satellites. The manuscript is quite good honestly. Sure it is not how I would have written it, so that I regularly took note of "[whatever] is missing" that I erased after reading the information a few lines later, but nothing that really impairs understanding. There's a lot of figures, but they all have a reason to be here.

I have two somewhat methodological points that I would like to see addressed, and a series of comments to improve the readability, but I consider that it should not require

a lot of work. Hence my evaluation "minor revisions".

1) The regions On Fig 1, you present the four regions into which you split the Weddell Sea, and that you analyse in Fig 3. You base that split on data from ULS, but you present only their mean, not the uncertainty attached to it. I am particularly surprised that 210 and 212 would be in different regions. So at least on Fig 1b, add the errors bars. Then modify the region split if needed.

2) The more recent time period and long term perspective Most of the analysis is performed on the time period common to all four reanalyses (late 2000s), which I understand. Unfortunately, it is a bit old and short. Southern Ocean sea ice has behaved very differently since. So please, include a short extra subsection dedicated to comparing GIOMAS and GECCO to SICCI (CryoSat2 at least) and APP. Ideally, also add something about trends in these reanalyses.

Now for the more minor comments, in order of appearance:

Line 109: say that all the information to come is summarised in Table 1. Try to write this entire section in a more structured manner, giving the same information about all four products (at least time period and resolution).

Line 140: you mention ASPeCT now, but only introduce the product line 170.

Fig 1a: add the lines separating the four regions plotted there too Fig 1b: see comment above, add the error bars, and potentially modify your region division accordingly.

Fig 3d: why is the correlation negative for Envisat? What happens? Is the bias mostly in summer or winter?

Fig 4b-d: why are you showing different thickness bands for different products? They are not even the thicknesses you comment on in the text. Please show only one range, so that the reader can compare the reanalyses.

Table 3: have you checked whether the reanalyses are correlated with each other? It

is suspicious that they all seem to have similar biases when compared against ICESat.

Fig 7: present it like Fig 6, as difference against reference rather than actual values. This way, we can compare with Fig 6 (alternatively, present Fig 6 like Fig 7).

Fig 8 (and text corresponding): since the sea ice concentration is about right, and that all reanalyses present similar biases in thickness when compared to satellite retrievals, can it be that the thickness retrievals are the ones that are not perfect yet? Sea ice concentration retrieval is after all more mature.

Line 331: you meant to refer to Fig 8 here.

Line 345/Fig 9: I know you write that you will not investigate the reasons for biases in the reanalyses, but I find the north sea ice of GIOMAS in winter/spring surprising. Is the reanalysis known for having too fast an Antarctic Circumpolar Current? Or is the ice too thin/mobile?

Line 342-346: you forgot to refer to Fig 9 here. The caption of Fig 9 refers to itself instead of Fig 8 by the way.

Table 5: the units need to be fixed. Indicate the net flux in the reference product as well (at least in the caption).

Line 373: not "sea ice ocean models", reanalyses. Sea ice ocean models have their own series of problems, but that's beyond the scope of this review.

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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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In this study, Shi et al. evaluate sea ice thickness (SIT) in the Weddell Sea from four reanalyses of coupled ocean and sea ice models, against two in-situ observations and two remote sensing datasets. Their results show that these reanalyses have limited success compared with the observations, and they stress the importance of sea ice motion and deformation on the SIT simulations. Modeling and observations of the Antarctic SIT, even for a single region such as the Weddell Sea, are very challenging. This study provides useful insights not only in the SIT reanalyses but also in the SIT observations, which will be a benefit to the sea ice research community. The Introduction, and Data and methods are well written. However, the Results part needs significant improvement. I suggest the manuscript accepted after major revision.

General Comments:

1. There is inconsistency during the comparison in terms of the data. In the "Data and methods" part, the authors state "Comparison are made using monthly means", however, when in 3.3 Comparison with sea-ice thickness from ICESat-1, they are using seasonal mean. This inconsistency must be fixed. It will be much better that the authors describe how they make the comparison in the exact sections.

2. Section 3.1. It remains unclear what kind of mooring data are using here. According to the statement "The aggregate temporal span of ULS observations in AP, CWS, SC and EWS is 148, 79, 185 and 272 months", and consider the numbers of the mooring in these regions, there should be large difference in the mooring data regarding the time duration. I suggest the authors add a plot in their Figure 1 showing the temporal evolution of the mooring observed SIT that are actually used in their comparisons.

3. Figure 4. Not sure why the authors use SITs from different locations in the three reanalyses (Figs. 4b-d). This means they also use different ASPeCT SIT when comparing with the different reanalyses. What can we infer from such different comparisons? I suggest the authors use a consistent comparison: Use the same ASPeCT SIT, with reanalysis SITs interpolated to the same time and same location.

4. Section 3.3. It is not clear what kind of manipulations here for the reanalysis SIT. The authors state "we use October and November to represent spring ...". However, according to Table 2, the ICESat-1 measurement is irregular, and no full month measurements. Do the authors use the same dates as the observations, or just simply use the full two-month reanalysis data? As the authors here try to compare the mean, it is very important to compare the exact corresponding data in terms of time and locations. Also the authors need give a test with confidence level for the comparison.

5. Line 280-281. "Compared with ICESat-1, only NEMO-EnKF has a similar variation of modal ice thickness from Autumn-FM to Spring-ON, while GECCO2, SOSE and GIOMAS have monotonically increasing model ice thickness". It seems to me 2005 &

2006 for SOSE, and 2006 for GIOMAS have similar seasonal variations in Figure 5. Table 4 looks somewhat misleading as its modal SITs not necessary in the same year.

Specific Comments:

1. Line 40. Not clear. Rephrase it.

2. Line 62. "... it is also reported to have uncertainties due to ...". All observations have uncertainties. Perhaps more important to note what kind of uncertainty it is.

3. Line 104. "all available daily records around specified model grids are averaged monthly". How far away from that model grid?

4. Lines 110 & 114. I think the adjoint method and 4-D Var method are the same here. Can the authors give a brief of their differences?

5. Line 195. "Figure 1b", better remove "b".

6. Line 217. "It is noted that the relatively short ICESat-1 record (13 months) limits the accuracy of this assessment". Not sure how the authors arrive to this statement.

7. Line 233. "Envisat has the lowest CC (-0.19) and highest RMSE (2.06 m) among all data sets, and its STD is comparable with GIOMAS". These numbers are almost unbelievable. The authors have any explanation of this? Is there any reference supporting similar findings? Or is there some error in the manipulation of the data?

8. Line 238. "But in the regions with large amounts of newly formed ice (the central Weddell Sea and the eastern Weddell Sea), SOSE tends to underestimate sea-ice thickness with lower STDs than the other reanalyses". It looks to me no data here to support that SOSE underestimates SIT.

9. Line 331. "Figure 7" should be "Figure 8"?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-71, 2020.