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The Cryosphere Discussions

Interactive comment on "Consistent variability but different spatial patterns between observed and reanalysed sea-ice thickness" *by* Joula Siponen et al.

Joula Siponen et al.

petteri.uotila@helsinki.fi

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Thank you for your comments on this manuscript. Please find below our detailed responses to them. In the revised manuscript, we will take all the points you raised into account.

1. This paper presents a comparison between the relatively new ESA CCI sea ice thickness dataset (a combination of CryoSat-2 and ENVISAT freeboards), with sea ice thickness in the ECMWF's ORAS5 reanalysis. A simple RMSE/correlation analysis is used to compare the datasets, and a discussion of possible causes of discrepancies is included.

We thank Referee #2 for her/his comments and suggestions. We think that even if our statistical analysis is described as simple, this does not reduce its value.

2. As stated at the end of the introduction: "The aim of the study is to give an answer to the question: Can the ESA CCI sea-ice thickness product be used for the validation of sea ice in the ORAS5 ocean reanalysis during the growth season? To answer this question, the mean sea-ice thickness as well as trends in sea-ice thickness and sea-ice volume are compared, and their uncertainties are taken into account." Unfortunately, I think this aim is rather basic (the cited references doing similar things are often in the form of technical notes for their reanalyses), and more importantly, the analysis lacks the level of scientific robustness/completeness I would expect from a paper in The Cryosphere. The paper title is also pretty misleading. It's a comparison of a radar observed sea ice record and one ocean reanalysis, so not anything near as complete as the title suggests.

We agree, our aim is a basic one, but we also think it is an important one, as we explained in our response to the 2. comment by Referee #1. We would like to point out that we use new products (CCI and ORAS5) which are higher quality than the previous ones used in this context, and that our analysis methods are vigorous, as pointed out in the 1. comment by Referee #1. We also believe that our results have such a significance, as explained in our response to the 2. comment by Referee #1, that they should be published in The Cryosphere, rather than in a technical report, which would be missed by most scientists interested in the topic, we think.

3. Some more specific comments:

You need to try and quantify the uncertainty. Most of the 'uncertainty analysis' was just discussion about biases/discrepancies which were often very subjective and arbitrary. There was a lot of subjectivity in the introduction and discussion throughout the manuscript too (e.g. the first line!). It's important to base scientific papers in objectives as much as possible. Another example (there were many more) - The Blanchard-

Wriggleworth (2018) study is just one of many new studies looking at snow on sea ice and does not provide evidence of its contribution to sea ice thickness uncertainty. I think someone reading this paper would come away with a misleading idea about the state of knowledge in our field.

We are happy to follow this important suggestion, keeping in mind that our analysis is vigorous. Before beginning, we would like to invite Referee #2 to provide us with the relevant references in addition to Blanchard-Wriggleworth (2018), please. This would be very helpful.

In terms of the quantification of the uncertainty, we will expand the scope of our paper by analysing an ensemble of seven ocean reanalysis which can be calculated from individual product data available at https://icdc.cen.uni-hamburg.de/1/daten/reanalysis-ocean/oraip.html. This will address the issue of reanalysis uncertainty and indicate how significantly reanalyses vary from the observations. We will also explore the effect of snow on ice on sea-ice volume based on ocean reanalyses. This will address probably the most significant issue of uncertainty due to the large effect of snow thickness on sea-ice volume.

The comments about it being better than ICESat were pretty odd – sure it might have better temporal sampling but that doesn't make it a better validation dataset (e.g. it could be less accurate!). What validation/assessment has already been done with CCI? There were only limited comments about other CS-2 derived sea ice thickness products and an attempt to quantify the uncertainty from the choice of re-tracking and other input assumptions.

We and Referee #1 (2nd comment) think that the products we analysed, including CCI, are certainly higher quality than the previously used ones, such as ICESat-1. ICESat-1 was not good because of the temporal coverage. Specifically, we state on line 238 that "the CCI product is a better validation product in the sense of temporal coverage" and on line 338 that "It [CCI] is better than ICESat due to its temporal and spatial coverage

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Discussion paper

over the whole growing season". We do not actually say that CCI is a better validation dataset than ICESat. We will reword these sentences to clarify this and not to mislead the reader. We will also reformulate the title to be less misleading keeping in mind that Referee 1 describes that the title summarizes well the findings of the study.

What's the central ensemble member and why again was this chosen?

The ORAS5 central ensemble member was selected as the first ensemble member, marked as ora0, out of six. We carried out the analysis for this member, and as the sea ice is essentially similar between the members, we decided not to include other members in the analysis. In other words, in terms of results, it did not matter which member was chosen.

What exactly did the recent Tietsche and Zuo studies do and what have we learnt from this?

We will provide more details to better clarify what we've learnt from this.

How is sea ice used in the ORAS5 reanalysis?

In terms of sea-ice observations, ORAS5 assimilates sea-ice concentration from OSTIA (see line 97), but no other sea-ice parameters. To clarify this, we will add some text to specify which sea-ice observations are used in ORAS5.

How important do you think sea ice thickness biases are?

This is a good question, we'd say they are important and in particular RMSE values in the order of several tens of centimetres or greater appear large compared to Arctic mean ice thicknesses. As sea-ice thickness biases result from disagreements in processes related to sea-ice evolution, we'd argue that they're manifestations of model and observational uncertainties and errors. These need to be first quantified and then addressed to improve the representation of processes related to sea-ice evolution in forecasting systems.

Is the aim of ORAS5 to provide a reanalysis of sea ice, or is this just the boundary condition for the bigger focus of providing an ocean reanalysis?

Sea ice is an integral and dynamic part of ORAS5, it is not just a boundary condition. However, given that ORAS5 is global, sea ice is certainly not the main focus of ORAS5.

How does ORAS5 compare to other ocean/global reanalyses in terms of it's sea ice model/assimilation approach etc?

The answer to this question can be found from Table 1 by Chevallier et al. (2017), which is cited in the manuscript. In general terms, compared to other state-of-the-art ocean reanalyses, ORAS5 has a rather simple sea-ice model (LIM2) but a mature assimilation of sea-ice concentration. Like almost all other reanalyses, sea-ice thickness is not assimilated, and therefore the covariance between sea-ice concentration and thickness are not properly accounted for.

This larger context would make the paper much more illuminating.

Thank you for these other important questions. We will address these in the revised manuscript.

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