The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-272-RC2, 2020

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

Anonymous Referee #2

Received and published: 9 February 2020

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.

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

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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.

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.

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 was only limited comments about other CS-2 derived sea ice thickness products and an attempt to quantify the uncertainty from the choice of retracking and other input assumptions.

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

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

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How is sea ice used in the ORAS5 reanalysis? How important do you think sea ice thickness biases are? 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?

How does ORAS5 compare to other ocean/global reanalyses in terms of it's sea ice model/assimilation approach etc? This larger context would make the paper much more illuminating.

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