The Cryosphere Discuss.,
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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 #1

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The manuscript by Siponen and coauthors presents a validation of a recent coupled ice-ocean reanalysis product against a new sea ice thickness satellite climate record joining two altimeters, but only available during the cold season. The validation is rigorous, both the model and observations are at the state of the art, and the manuscript is well-written.

The exercise is unfortunately limited in scope - a monovariate validation - and does not reveal much more than was already known from previous intercomparison articles. The new products compared are certainly of higher quality, but the analysis does not benefit much from this.

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Discussion paper

This becomes cruelly evident when the authors state the objective of the paper: "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?", which is not a scientific objective per se.

The results reveal contrasting findings, well summarized in the title, but the analysis is too shallow, only listing non-prioritized factors without pursuing any of them any more than was done in previous literature.

There are multiple ways the study could evolve into a more informative paper: pursue the ice drift issue by calculating volume fluxes between the different regions, extract the local ice production and melt from the model thermodynamics, the sea ice deformations and the data assimilation increments. The thermodynamics could be further pursued as well by comparing the model snow parameters to the Warren climatology and comparing the full yearly cycle of ice thickness against the Beaufort Gyre Exploration Project http://www.whoi.edu/beaufortgyre, these data are freely available. The validation of atmospheric and ocean parameters of relevance for sea ice could be included as well. There are many different directions this paper could evolve to become more informative.

Such additional analysis may represent significant work and I am not confident that this can be done during the review process, I therefore recommend rejection of the paper and resubmission when the analysis has better documented the likely causes for the differences between model and observations.

Detailed comments:

- Abstract l14: Is an RMSE of 1m a sign of good quality? What is the baseline for "good performance"?

- Abstract l15: the causes of differences should be prioritized.

- l97: when assimilating sea ice concentrations, what is the thickness of the "added ice" and its snow depths?

- l102: What is the "central" ensemble member in this context?

- l126: The orbit of ENVISAT should replace the "track" of ENVISAT.

- l139: I understand that the model concentrations are disregarded here, so talking about "sea ice volume" is misleading because the model volume is a mix of model (thickness) and observations (concentrations). For the sake of clarity, the volume should be replaced by the average thickness.

- l179: Hudson Bay and other semi-enclosed areas receive no ice export from other places so it could be worth concentrating on these areas to evaluate the thermodynamics of the model separately from the dynamics.

- l185 / Table 2: The Norwegian Sea is by definition further south than Svalbard and ice-free. To avoid confusion, the sector should be renamed "Svalbard Area" or maybe "Norwegian Sector" referring to WMO MetOcean areas.

- l192: "coastal polynyas": If the model does not resolve these polynyas, what are the main processes for ice formation in the model?

- l232: The issue of interpolation to the observations or onto the model grid comes twice in the paper, is it important enough to deserve such attention, if yes please indicate the difference in number but if not this text can be shortened.

- l285: Level ice is subject to stresses and undergoes deformations like any other types of ice, so I don't see why there should be no leads in thin ice. The smaller freeboard could be an issue, though.

- l291: "could produce too large drift": you certainly have the model and observed drift available, so these should be compared directly rather than referring to some previous literature.

- l307: have you used negative snow depths in Hudson Bay? If not, what snow depth have you used?

- l310: "which should happen soon". Please refer to ongoing research rather than wishful thinking. papers by Rostosky et al. 2018, or more recently Kilic et al. 2019 and Liu et al.:

Kilic, L., Tonboe, R. T., Prigent, C., and Heygster, G.: Estimating the snow depth, the snow–ice interface temperature, and the effective temperature of Arctic sea ice using Advanced Microwave Scanning Radiometer 2 and ice mass balance buoy data, The Cryosphere, 13, 1283–1296, https://doi.org/10.5194/tc-13-1283-2019, 2019.

Liu J. et al. 2019 Remote Sens. 2019, 11(23), 2864; https://doi.org/10.3390/rs11232864

- Section 5.3: As explained earlier, the comparison of mean thicknesses would make more sense when the model concentrations are ignored.

- l321: The anomalous events are well visible in the time series, no need to remove the trend.

- Figures 2 and 3 are missing the colour scale.

- Figure 6: the CryoSAT2 period is too short for any meaningful trend. These should be removed (also from Table 3)

- Table 3: Splitting the trends per month is not really informative as long as the seasonal cycle is not validated against in situ measurements.

Typos:

- l79: Taken -> taking

- l342: consisted -> consistent

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