

Earth Syst. Sci. Data Discuss., referee comment RC2
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Comment on **essd-2021-334**

Anonymous Referee #2

Referee comment on "Improved BEC SMOS Arctic Sea Surface Salinity product v3.1" by Justino Martínez et al., Earth Syst. Sci. Data Discuss.,
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The paper describes the updated SSS- SMOS derived data set. Inversion algorithm, comparison with other products, including the one previously developed by the same consortium, and partial data validation are described. The paper is clear and the description of process used to move from data to product is appropriate. The main advantage of the new product is its finer resolution which could be relevant for oceanographic processes description. The paper is almost ready for publication except for some minor suggestions which can improve its readability.

Lines 14-15 : there is a repetition here. Please check.

Line 16.19 : you can improve the readability of manuscript here by merging/reformulating the different short sentences.

Lines 20-21 : sentence "...better monitoring the observed changes in the freshwater fluxes". Please add reference for the observed changes which, in my understanding, differs from L-band radiometry.

-Line 28-29 : " Whilst L-band frequency is the region of the electromagnetic spectrum offering the most sensitivity to salinity variations, it decreases rapidly in cold waters " this is not true. Recent paper (e.g. [10.1109/TGRS.2021.3101962](https://doi.org/10.1109/TGRS.2021.3101962)) proves that other frequencies work better and efforts are made to promote it from space (**DOI:** [10.1109/JSTARS.2021.3073286](https://doi.org/10.1109/JSTARS.2021.3073286))

-Line 31-32 – LSC: you can simply mention that the problem is due to the large footprint on the ground (interferometer is obviously worst).

-Line 58 : could you provide an estimate of error introduced by the interpolation process?

-Line 67 – the sentence “The SSS and SST are converted to TB “sounds a bit strange, I guess that you meant that SSS and SST are used as inputs in an e.m model to generate simulated Tb values.

-Line 69_ you mention that data “generated from measurements of the 2005-2017 period” are used as reference but SMOS data refers to 2011-2019 period. Did you evaluate if there is an impact on the obtained the results if the same overlapping period is used?

-Line 90: could you add an accuracy estimate for Tara data?

-Line 104-105: could you estimate/quantifying the differences in considering 64x64 instead of 128x128 point? You mention “without loss of information/resolution “.

-Line 119-120; is not clear if the ionosphere correction is applied. Since 1st Stokes parameter is used for the inversion

-Line 133-135: if I understood correctly you use, as reference, the SSS value obtained from the WOA instead to the one obtained from SMOS (average value). If so, which is the estimated differences between these values? Could you provide an example for some specific regions where the coast contamination is /or isn't relevant?

-Line 173 : the sentence “Only latitudes above 50°N are considered” can be eliminated since it is repetition of line 159.

-Line 189- For the minimization did you use different approaches? For instance, did you check if the introduction of a regularization term could be beneficial?

-Line 203 – 206 why using 100 , 7 and 2 as criteria? Could you better justify it? (i.e. Why not 90 or 110?)

-Line 230-233: Also here : could you better explain how the thresholds were selected?

-line 260: The 12 psu bias has, in my view, severe implication. It implies that the retrieval largely overestimates the retrieved SSS besides the numerous procedures, averaging, de-bias which were conducted to derive it. Did you have an explanation for the bias? There is a problem of representativeness of SSS retrieved by SMOS and what provided by HYCOM? Or there is an absolute error on HYCOM only as bias but it didn't affect the temporal variability? At the end you plan to use it for the temporal correction.

-line 276: how much the number differs from zero? This could be a useful information for the reader.

-figure 5: why the error is only represented for radiometric uncertainty? you have different factors that contribute to the error computation. This is bit reductive.

-Argo validation: not clear to me why BEC V2.0 data provides better results in 2011-2012. If SMOS is affected by RFI (as the authors mention) this impact on all the products. Moreover, for BECV3.1 you develop an approach which is devoted to mitigate RFI effects.

-Tara Validation: from table it seems to me that the affirmation "Arctic+ v3.1 product, better than the previous BEC v2.0 product in most of the seas" is questionable. I see a clear advantage in two cases only (Kara and Beaufort).

-Spectral Analysis : the difference between SMAP and BECV3.1 seems to me very small in the figure then my conclusion is that both method provide similar results. It is correct?

-Lie 427: while I recognize that validation for 3 days product require additional effort, although it could be very interesting for potential users, I believe that validation at 18 days should be simple and can be easily implemented.