

Earth Syst. Sci. Data Discuss., referee comment RC1
<https://doi.org/10.5194/essd-2021-364-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on essd-2021-364

Anonymous Referee #1

Referee comment on "New SMOS SSS maps in the framework of the Earth Observation data For Science and Innovation in the Black Sea" by Estrella Olmedo et al., Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-364-RC1>, 2021

ESSD-2021-364: New SMOS SSS maps in the framework of the Earth Observation data for science and innovation in the Black Sea.

By Olmedo et al.

General considerations

1) The paper is part of series published by the authors in 2020 and 2021 (papers published in IEEE Journal and ESSD in my knowledge). Authors are developing salinity products at 'high' resolution starting from low resolution SMOS data. For sure, such products need to be developed at regional scales. This justifies this and previous publications.

On the base of these considerations, I read the paper looking for the originality and quality of data.

2) Many concepts/ideas have been presented in previous papers and are repeated here. The authors must look at the similarity report showing that many sentences are pasted and copied. Contamination by land, radio frequency interference, effects on temperature brightness are well discussed in other papers by the same authors or many of them (e.g., DOI 10.1109/JSTARS.2020.3034432) and are again presented here. I would expect a short summary with references instead of repetition, eventually synthesizing the

approaches in tables.

3) A crucial aspect in all publication is the use of 'empirical corrections' to 'mitigate' the land sea contamination (as written in the paper DOI 10.1109/JSTARS.2020.3034432), This seems to be a normal practice in 'SMOS community' (e.g., <https://www.sciencedirect.com/science/article/pii/S0034425720303977>). But the approach is not convincing me. In the Black Sea the authors are using a linear extrapolation. May be the authors will use another approximation in future papers on Mediterranean or Baltic Seas.

4) Uncertainties on various data sources are discussed, but their combined effects /error propagation is not. This happens also for residual errors in the correction of parameters used to estimate radio-brightness contrast.

5) Here a robust statistical analysis valid for all semi enclosed seas should be analyzed, compared, criticized, and (in case) adopted. The relationship SST and SSS should be more carefully assessed, as well as the role of stratification in SSS retrieval. I expect a significant difference between in situ S and SSS in areas strongly influenced by river runoff.

It seems to me that authors apply a simple 'cut and try' method, while could be better to say that it is impossible to retrieve salinity data near the coast from SMOS or that the error is relatively high.

Other considerations

Line 71: *salinity retrieval depends from ... sea ice cover* – This is never discussed in the paper. See comment 2 above

Lines 171-172: *both the Baltic and the Black Seas are characterized* – nothing is said of vertical stratification. See comment 5 above

Line 191-192: *None of the existing dielectric models are well characterized to be used in this low (and negative) regime of salinity values.* – See comment 3 above

Lines 219 ... : Seasonality of SSS biases. There is a strong seasonal bias with respect to the global ocean, it would be interesting to know if this is happening in all semi enclosed seas and how the authors manage this in a general way.

Para 3.2: The paper is weak in the selection of data for absolute calibration and validation. This is a compromise between the need for a set of data representative of the Black Sea and the need for a set of data representative of SMOS estimates. In the paper this compromise is confusing.

Conclusions

I would see a 'unique' methodology applied to seas having similar characteristics (e.g., semi-enclosed seas) although lying in different latitudes This approach would assess strength / weakness of the methodology and provide suggestions for general vs specific solutions.

From the reading of previous publications, the authors are finding partial solutions in specific geographical area (e.g., Arctic, Black Sea) avoiding a general common approach.

On the base of these considerations, my opinion is that the paper is not publishable.