We would like to thank the Editor and the Reviewers for their useful insights and suggestions that have helped to improve the clarity of the manuscript. We provide here a point-by-point answer to all the suggestions. The number of figures and tables and the number of lines where text has been modified are referred to the updated version of the manuscript with track changes and our answers to comments are given blue text.

Comment on essd-2021-461
Anonymous Referee #2
Referee comment on "First SMOS Sea Surface Salinity dedicated products over the Baltic
Sea" by Verónica González-Gambau et al., Earth Syst. Sci. Data Discuss.,
https://doi.org/10.5194/essd-2021-461-RC2, 2022

Need for satellite-based sea surface salinity (SSS) determinations is very urgent in the Baltic Sea, because in the sea with large-scale estuarine gradients, salinity is important both for the physical and ecosystem dynamics. The MS is a major contribution in the development of regionally adjusted SMOS method, which is timely and technically sound. The products of SSS have been prepared as well. The work has been done within the project of ESA initiative Baltic+.
The MS should be published. I made some comments, which can be taken into account when preparing the revised version of the MS. The comments reflect an oceanographer's opinion.

Thanks a lot to the reviewer for his/her revision.

1) The introduction is lacking the description of SSS of the Baltic Sea. Since it is the target of present study, the main SSS features should be outlined and their forming mechanisms should be explained. Mentioning that the salinity dynamics is complex (for example, on lines 26-30) should be preceded by simple basic facts. For example, due to the geographical separation of locations of major freshwater sources and the straits channeling the saline water inflow, the SSS has persistent large-scale variations between and along the basins. I recommend rewriting the introduction accordingly.
It could be also interesting to compare the large-scale SSS variations (about 6 psu per 400-500 km in the Gulf of Finland and the Gulf of Bothnia) with other oceanic regions of SMOS applications.

We have modified the introduction for including simple basic facts about SSS dynamics in the Baltic Sea (lines 20-41).

2) The statement "the Baltic Sea is one of the most challenging regions for the SSS retrieval from L-band satellite measurements" (lines 43-44) is followed by a lengthy text, where the main peculiarities cannot be found easily. What I found from the text, the main challenges for SMOS application are the vicinity of coasts, seasonal ice cover and low salinity range. I would like to read (such) a short message somewhere before or after the lengthy presentation.

We have modified the presentation of the main challenges to be addressed for the SSS retrieval in the Baltic Sea (lines 64-74) and the main algorithms used to overcome them (lines 85-95), focusing on those algorithms/modifications not published previously.

3) Chapter 2 “Generation of Baltic+ SSS products” has mutually dependent sub-chapters 2.1 “Data sets used in the generation of the products” and 2.2 “Algorithm description”. A reader interested in SSS results, but not familiar in details of SMOS method, would benefit if there will be a few introductory sentences before the title of 2.1. Presently, the data sets like “SMOS Brightness Temperatures” are listed perhaps too abruptly, without explanation of their role in the algorithms. Therefore, rewriting is recommended. I understand from 2.1 that SMOS brightness temperature is the basic observed quantity for SSS retrieval, while auxiliary data like sea ice cover, rain rate, 10-meter wind speed, 10-meter neutral equivalent wind, significant wave height of wind waves, 2-meter air temperature, surface pressure, and vertically integrated total water vapour are used as well. SST fields are used for correction of systematic biases of SSS. Sea ice data are used to exclude the ice covered areas from SSS estimates. Error filtering and validation of SMOS products is done by comparing with independent SSS reanalysis and operational forecast products.

Before subsections 2.1 and 2.2, a general block diagram of the Baltic+ SSS processor has been included, detailing the processing steps jointly with the input data needed for that processing (Figure 1). Besides, in all the subsections we detail in which processing steps are used each one of these datasets.

4) The chapter 2.2 “Algorithm description” is much broader than just description. It contains significant research to study the options of implementation and fine-tuning of algorithms. The title could be modified accordingly. It is like the “Results” chapter in many of the papers.

Thank you for bringing up this point. As the reviewer points out, the research that has been required to implement and/or fine-tune the algorithms to retrieve SSS in the Baltic Sea has been a huge piece of work.
We have changed the title of section 2.2 to: “Algorithm developments for Baltic+ SSS products”.

5) For the attraction of readers, the chapter 2.2 could contain a flow diagram explaining basic steps of the data flow and calculations within the algorithm.

We have included this flow diagram at the beginning of section 2 (Figure 1), with the main processing steps of the Baltic+ SSS processor (introduced in lines 109-119) and the datasets used in each processing step.

6) The sub-chapter “2.2.1 Generation of brightness temperatures” starts from corrections to the basic algorithm. I recommend revision that the presentation starts with a description of how brightness temperature is generally calculated. Then details could follow.

We would like to point out that the initial version of the manuscript contained more details on the processing from the very low level of processing up to the generation of the L3 and L4 SSS maps. However, it was required before the manuscript was accepted for discussion that the explanations of all those algorithms/corrections previously published were reduced (while maintaining a self-consistent manuscript), focusing, here, on the new algorithm developments and on the improvements/fine-tuning of the already existing methodologies for the specific case of the Baltic Sea. This is the reason why this section is focused on the ALL-LICEF calibration and the Gkj correction, since these are the main differences in the algorithms used to generate the brightness temperatures with respect to the nominal brightness temperatures. Two references (one of them is a quite recent review) on the image reconstruction strategy are included (Corbella et al., 2009 and 2019).

7) The “Gkj correction” (line 132) has to be explained in present MS.

We have included more details in section 2.2.2 to explain how the Gkj correction is applied to the visibilities (lines 180-185).

8) “Half first Stokes” parameter (line 167, Fig. 2 caption) has to be explained.

The definition of the first Stokes parameter has been included in the manuscript (line 222).

9) The statement “For very diluted solutions, the conductivity depends almost linearly on the salinity” is clear without Fig. 3. The figure could be omitted. For clarity, there could be a reference to the oceanic algorithms on how salinity is calculated from the CTD data (EOS-80, TEOS-10).

We agree with the reviewer. We have omitted that figure and added the following reference:

IOC, SCOR and IAPSO, 2010: The international thermodynamic equation of seawater – 2010: Calculation and use of thermodynamic properties. Intergovernmental Oceanographic Commission, Manuals and Guides No. 56, UNESCO (English), 196 pp.

10) Significant downward spike of TB around SSS=-7 in Fig. 2b has to be mentioned and interpreted. Negative values of SSS should be explained on the figure as well.

We have included a note in the caption of Figure 3b to point out that negative SSS values do not have any physical meaning, instead they only reflect the presence of instrumental biases that need to be corrected.

Figure 3b shows the modeled half first Stokes parameter for a given incidence angle and SST value. Problems at low SSS values are evident: the dielectric model presents at least a maximum for very low SSS, which causes an inversion problem for values of TB close to the maximum since the same TB can be attributed to two different SSS values. This behavior is nonphysical. Models are constructed by fitting experimental observations (taken at larger values of salinity [32-38] psu) with rational functions (i.e. quotient of polynomials) and the value at low SSS is an extrapolation.
Moreover, as stated by the reviewer, a singularity (i.e. the polynomial in the denominator is equal to 0) is observed around SSS=-7.

11) The title “Definition of a SMOS-based climatology” (line 189) is not clear in the context of the algorithm. Perhaps it is “Estimation of SSS systematic errors of SMOS with respect to reanalysis”. If not, some rewriting could be useful in order to improve clarity of the title and the text. “SMOS-based climatology (denoted as sssclim)” is defined only at the end of the sub-section (line 218), until that the reader is unclear what is meant under the term in the title of the sub-section.

The reviewer is right. The important point here is the correction of SSS systematic biases. So, we have changed the title to “Characterization and correction of SMOS SSS systematic errors”. It must be pointed out that this estimation of SSS systematic errors is not with respect to reanalysis. We compute the typical SSS value (we call it SMOS-based climatological data) SMOS measures under a given acquisition condition. Then, for each SSS measurement we compute the SMOS SSS anomaly with respect to the corresponding SMOS-based climatological data. Finally, we add an annual reference salinity field to provide absolute SSS values.

We have also changed the term “SMOS-based climatology” by “SMOS-based climatological data” through the manuscript.

12) The statement “non-expected spatial gradient appeared close to the coasts” (line 200) has to be explained. What was the situation, (a) high gradients found in the SMOS data that reflect real coastal dynamics, as seen by in situ data and reanalysis, or (b) high gradients not corresponding to the real gradients, (c) something else?

We observed high spatial gradients close to the coasts that do not correspond to geophysical gradients (they are not observed either in the reanalysis nor in the in situ measurements). A dependence of these differences with the SST was found. For this reason, the SST is also considered in the classification of the SSS systematic errors. We have clarified this point in the manuscript (lines 254-256).

13) Caption of Fig. 4 should indicate that the monthly mean differences are presented for the year 2013. Why this year has been selected, some reason should be given in the text.

This point has been included in the caption of Figure 4. In the development of Baltic+ SSS products there were two phases. In the first stage, we developed a first SSS product prototype for the period 2011-2013. It is from these first SSS maps that we analyzed the mean differences between SMOS and the reanalysis. Taking into account the larger RFI affectation in 2011-2012, we selected 2013 for doing this analysis.

14) The statement “To avoid lack of statistics” (line 209) is not clear. In addition, the content and need for Table 1 could be better explained.

The introduction of the SST in the classification of SSS systematic errors leads to a reduction in the number of measurements under given acquisition conditions. Therefore, to increase the number of measurements and have significant statistics, we extend the SST range to compute the SMOS-based climatological data for the lower ranges of SST (as shown in Table 1). We have clarified this point in the manuscript (lines 265-269).

15) It is not clear why the constant exclusion criteria ("larger than 2 psu are also discarded", line 250) was applied for SMOS SSS anomaly instead of location-dependent variable criteria. The SSS variance in the Baltic Sea (can be simply determined from CMEMS reanalysis) is rather variable, as can be seen from the studies of fronts in the Baltic Sea. For example, the Baltic Proper is much more homogeneous regarding SSS than the Belt Sea and the Gulf of Finland, and the strait areas to the Gulf of Bothnia and the Gulf of Riga. Therefore, it seems that the Baltic Proper could have much smaller exclusion criteria than regions of high SSS variability. A more detailed reasoning could be useful.

We do not apply any filtering based on the SSS variability in the different subbasins. We apply a very simple threshold based on the SMOS SSS uncertainty itself that we derived directly from the radiometric errors (following Eq (1) in Olmedo et al., 2021). We observed that this threshold of 2 psu was quite efficient in masking those SSS retrievals with low quality (mainly retrievals very close to coasts and ice, affected by RFI, by residual LSC, see Figure 8). We have added a reference to examples in Figure 8 (line 315).

16) The statement in line 240 "Any raw SSS out of the range [-150, 100] psu is not considered as part of the valid raw SSS values" does not follow the oceanographic point of view. In my understanding, "raw" data mean almost real data but they need to be filtered and corrected. If the defined range of raw SSS is completely out of range of the real sea surface salinity in the Baltic Sea, the quantity should have another name than "raw SSS", even if this term has been published for other seas with higher salinity.
To clarify, when studying the living room temperature, if some intermediate result is 300 degrees Celsius, I would not call this as raw temperature but something else.

We understand the point by the reviewer. First of all, it must be highlighted that negative SSS values do not have any geophysical meaning. Indeed, they reflect instrumental biases and other systematic errors that need to be corrected.

For example, considering that (i) the mean SSS value in the Baltic Sea is ~7 psu, (ii) SMOS TBs have a radiometric accuracy (that depends on the acquisition conditions, incidence angle and across-track distance) between 2 and 6 K and (ii) the sensitivity of the SSS to TBs can drop to 0.1 [K/psu] in cold waters, the expected salinity values due to radiometric errors of the instrument and the low sensitivity can be in the range:

$$7\,psu \pm \frac{4\,K}{0.1\,K/psu} = [-33:47]\,psu$$

This estimation is without considering systematic biases. Indeed, we call this magnitude raw SSS because we need to correct it from systematic biases and filter it.

For these reasons, we have maintained the term "raw SSS".

17) The filtering criteria (lines 239-255) should give some examples of bad and good data.

The filtering criteria based on the SSS uncertainty itself (>2 psu) can be observed in the examples provided in Figure 8. This filtering is mainly affecting ice-covered grid points, grid points very close to coasts and Skagerrak and the Kattegat straits.
Regarding the filtering of SSS values that deviate too much from the SMOS-based climatological data, we have added a figure (Figure 6) that shows the mean and the standard deviation of two SMOS-based climatological distributions for two different bins of SST (see answer to the comment arised by the other reviewer). It must be pointed out that the expected geophysical variability of the SSS is much lower than the standard deviation of the distributions, so this filtering is not very restrictive.

Some clarifications have been added in the text (see lines 303-323).

18) It is not clear why the oceanic limits [0, 35] (line 253) are used in the Baltic Sea for the exclusion criteria since in the major part of the sea SSS will never exceed 10 psu. The problems with dielectric constant models (they have been tested on oceanic salinity range) were mentioned earlier in lines 52-54. Is the criteria in line 253 related to this or is there some other reasoning?

We agree with the reviewer. These limits were set considering that the SSS product would also cover the entrance of the Atlantic waters, the Skagerrak and Kattegat straits. Finally, the SSS retrieved over this area were of poor quality and we decided to limit the product to longitudes >14ºE. We plan to apply other techniques (such as the fusion of TB with SST, see Olmedo et al., 2022) in order to improve the quality over these areas in future versions of the product.

In fact, we have checked that this filter is not discarding points, since the maximum SSS value we find in the L3 SSS maps is 12 psu (except for a very few maps in the beginning of 2011, which are strongly RFI-contaminated). For this reason, we have removed this sentence.

19) In the title "2.2.7 Mitigation of time-dependent biases" (line 267) the term "mitigation" is not in the right context, see https://www.britannica.com/dictionary/mitigating or similar.

We have changed the title of subsection 2.2.7 to "Correction of time-dependent biases".

20) The statement "However, due to the lack of Argo floats..." (line 277) should be rephrased, since Argo floats are used in the Baltic Sea (https://www.euro-argo.eu/News-Meetings/News/News-archives/2019/Argo-floats-complement-the-Baltic-Sea-observation-network).

The reviewer is right. However, the Argo floats are restricted to the Bothnian Sea and Gotland Deep and Bornholm Deep with very scarce spatio-temporal coverage. So, there are not enough measurements to use them for computing the temporal correction for each 9-day SSS map. This is the reason we cannot apply the same strategy as in the Mediterranean Sea (Olmedo et al., 2018b) and Arctic Ocean (Olmedo et al., 2018a).
We have clarified this point in the manuscript (lines 344-345).

21) For assessing the temporal corrections to SMOS salinity, in situ measurements are taken from SeaDataNet (lines 278-279), which, to my knowledge, is not collecting and disseminating the whole set of Baltic-wide FerryBox measurements in operational time scales. Why this extensive valuable dataset is excluded here?

Our first approach was using a subset of in situ data to compute the temporal correction and another subset of in situ to validate the SSS products. However, as it is shown in Figure 8, the scarce and inhomogeneous spatial distribution of the collocated in situ measurements led to discard this approach.

22) A spike in Fig. 8 must be explained. Is it due to the problems of in situ data or satellite retrieval? Or is it due to the fragmentary spatial distribution of in situ data compared to the regular reanalysis data?

The coverage of the SMOS SSS map for this date is similar to the previous dates. The issue comes from the very few in situ data available for that period. In the last 10 days of March 2013, there are several 9-day maps with only 2 SDN collocated measurements. Due to this fragmentary spatial distribution of in situ data, we decided to use the reanalysis data for correcting the temporal biases.
A clarification has been added in the caption of Figure 9.

23) In the section "3.1.2 FerryBox lines in situ salinity" the source of data has to be specified. It is not clear, from where the data quality flag definitions are taken from. The flag PSAL_QC is used only on line 340, perhaps to omit the notation.

We downloaded the data from the following FTP: ftp://my.cmems-du.eu/Core/INSITU_BAL_TS_REP_OBSERVATIONS_013_038/history/vessel/. This dataset was retired in March 2020 and replaced by INSITU_GLO_TS_REP_OBSERVATIONS_013_001_b (https://doi.org/10.48670/moi-00039). We have added the source in the manuscript (lines 404-406) .

We apply the data quality flag definitions in OceanSITES reference Table 2 (see reference below). However, we agree this notation is not needed in the manuscript and we have omitted it.

OceanSITES Data Format Reference Manual, NetCDF Conventions and Reference Tables Version 1.4 July 16, 2020. Online available at: http://www.oceansites.org/docs/oceansites_data_format_reference_manual.pdf

24) The Table 2 presenting the FerryBox ship routes and periods is not complete. The data are collected and disseminated by CMEMS. Comparing with my downloads, data from Baltic Princess and MS Romantika are not included.

We downloaded this data at the beginning of the project (2019), when we validated the first prototype of the product. At that time, it contained all the routes collected in Table 2 of the MS and the one from Romantika. However, when we considered the quality control, no collocations with Romantika were found. This is why this route is not included. The Princess line was not available in this dataset.

25) The title “3.1.3 SeaDataNet in situ salinity” is not correct, since two data collections are used: SeaDataNet and ICES.

The reviewer is right. The title of section 3.1.3 has been changed accordingly.

26) It could be interesting to read (perhaps in 3.4.4 Description of salinity dynamics),what could be the reasons of SMOS SSS overestimation relative to FerryBox results positive bias) in the Gulf of Bothnia in years 2012 and 2017, both in the products of L3 (Fig. 13) and L4 (Fig. 16).

In fact, we really do not know which can be the origin of these positive biases with respect to ferrybox data. We cannot evaluate this effect with SDN data because we have a lack of collocations to perform the spatial statistics per year. In fact, this would need further investigation. A comment has been added in the caption of the spatial mean difference of L3 SMOS SSS and ferry data (Figure 14).

27) The tables 3-8 have similar structure and perhaps some of them could be combined. Besides yearly statistics, summary values for the whole period could be useful as well.

Thanks for the suggestion. We have combined all the statistics computed for L3, L4 and filtered L4 with respect to ferry data in Table 3 (Tables 3, 4 and 5 in the previous version of the manuscript) and with respect to SDN in situ data in Table 4 (old Tables 6, 7 and 8). In addition, we have included the statistics computed for the whole period.

28) Technical issues with figures (legends, units, color scales) and abbreviations in the text etc should be corrected.

We have reviewed issues with all the figures and some acronyms which were missing in the previous version of the manuscript.

Additional remark. The MS uses extensively the term “climatology”, with an interpretation as “climatological data” (https://community.wmo.int/wmo-climatological-normals). Climatology is “the description and scientific study of climate” (https://glossary.ametsoc.org/wiki/Climatology) or a “branch of the atmospheric sciences concerned with both the description of climate and the analysis of the causes of climatic differences and changes and their practical consequences” (https://www.britannica.com/science/climatology). I would prefer using the term “climatological data” and leave “climatology” for the classical approach as a branch of science, although data-oriented jargon as in the present MS is used sometimes in the papers dealing with technical aspects of oceanographic data processing and model development.

We have considered the suggestion by the reviewer. We have replaced the term "SMOS-based climatology" by "SMOS-based climatological data".