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Interactive comment on "Nine years of SMOS Sea Surface Salinity global maps at the Barcelona Expert Center" *by* Estrella Olmedo et al.

Anonymous Referee #2

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This paper presents a new reprocessing of the BEC SMOS SSS global L3 and L4 products v2.0 for a 9-years period comprising 2011 to 2019, with an updated methodology with respect to previous BEC version. The updates concern new filtering criteria, a new empirical correction for latitudinal and seasonal biases, an improved scheme for preserving small-scale gradients close to the coast, and the provision of a sea surface salinity uncertainty. Three BEC products are presented : a high resolution product, a low resolution product (smoothed with a radius of 50km) and a L4 product produced using a multifractal fusion technique applied to SMOS L3 using OSTIA SST fields.

After the methodology presentation, evaluation exercises are performed :

-by comparing the new BEC products with Argo salinities over the whole time series

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-by comparing the caracteristics of the new BEC product relative to other satellite salinity products and to a CMEMS salinity product. These comparisons are restricted to the 2017 year. They concern not only comparisons to Argo but also comparisons of singularity exponents, of spectral properties and results obtained with triple colocations.

This is a huge piece of work by one of the two data centers delivering SMOS Level 3/4 products. Given the unique length of SMOS satellite salinity, it is a very important topic. The paper is clearly written and contains many interesting results. Nevertheless, I think several aspects would be worth to be deepened before publication, as this paper will serve as reference for future work:

- The 2017 year is chosen to intercompare the various products because it is close to a normal year ('there is not any large-scale geophysical phenomenon'). But given that the BEC products are calibrated using a climatology and that the methodology makes use of many filterings, it would be important to evaluate the product skills relative to others during a non normal year, e.g. 2016.

- The metrics used in the comparisons with Argo are mean, standard deviation and rms difference. They should also include R2 : indeed, the BEC methodology employs many filterings and R2 would allow to measure their efficiency in maintaining the SSS variability.

- By construction, the singularity exponents of the L4 product are expected to be close to the ones of OSTIA SST so I don't think that comparing BEC L4 and OSTIA SST properties provides an independent validation of BEC L4 products.

Detailed remarks :

Abstract : not sure doi are useful in the abstract.

Abstract : last statement (iii) should be moderated (see below my comments about the triple colocation section) : I don't think the authors have an absolute reference allowing a measure of the error. Given the huge spatial variability in regions affected by rainfall and by continental freshwater discharges, I am surprised that a low resolution product is more accurate.

L20 : verb is missing

L126 : radiometric sensitivities wording is confusing (see Randa et al (Recommended Terminology for Microwave Radiometry, National Institute of Standards and Technology Technical Note 1551 (August 2008) : Radiometric sensitivity is often used to mean radiometric resolution, but this use is discouraged in view of the definition of sensitivity). I guess the authors mean radiometric resolution.

Section 2.2.3 : It is useful to recall all these filterings. It might be useful to point out more specifically the changes with respect to Olmedo et al 2017 and to display maps of the number of filtered points. In particular, the distribution of the salinity is naturally skewed in rainy regions and in river plumes, what is the effect of such filterings in these regions ?

Equation 4 corresponds to a one sigma sorting, which seems very stringent, what is the rationale for this choice ? how sensitive is the result to this ?

Figure A2 : I suggest to add a figure displaying the polynomial correction.

Section 2.2.6 : A major difficulty is how to filter SMOS retrieved salinity given that salinity distribution is very skewed and that RFI contamination might lead to artificial skewed distribution too. In the updated methodology, more stringent filterings than in Olmedo et al. (2017) are applied. After having performed the serie of filterings and corrections, an inconsistency between the WOA reference and the mean corrected field appears in river plumes which is likely an effect of the skewed salinity distribution (Figure A3) but it seems that there is also a global north-south difference : could the authors refine the color scale of Figure A3 to allow a better display (e.g. with a 0.02psu resolution)?

L243-247 : the (Boutin et al., 2016, 2018; Kolodziejczyk et al., 2016) publi-

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cations refer to earlier versions of CATDS CEC products than version 4. A summary of the version 4 updates with respect to earlier versions is available on https://www.catds.fr/Products/Available-products-from-CEC-OS/CEC-Locean-L3-Debiased-v4.

Section 3.1.3 : It is indicated line 407 that CMEMS product assimilates SMOS CATDS SSS observations. This could be indicated in this section.

L294 : Figure A11 is mentionned in the text just after Figure A3. Figures might be reordered.

L351 : hsst mentionned twice.

Table A1 : CMEMS product

L415 : see my general comment about making the validation for the ‘normal’ year 2017. What would be obtained for year 2016 ?

L437-439 : I don’t understand. CATDS CEC v4 fields use the combination of all ISAS fields over the 2012-2018 period to calibrate the full time series (see https://www.catds.fr/Products/Available-products-from-CEC-OS/CEC-Locean-L3-Debiased-v4: ‘These relative salinity variations are then converted, in a last step, to salinities by adding a single constant determined, in each pixel, from SSS statistical distribution over the whole period (SMOS SSS distribution compared to ISAS SSS). This last step only determines the absolute SSS calibration in each grid point; the SMOS SSS temporal variation is independent of this adjustment.’ In other words, both BEC and CATDS are calibrated using a climatology. CATDS does not use ISAS SSS to perform an adjustment to a specific year.

Figures A5 to A10 and Table 4: I recommend to add information about R2.

Figure A7 : Since the paper contains many figures, I suggest to remove Figure A7 which contains information in some way redundant with Figure A9. In case the authors prefer to keep Figure A7, it would be interesting, in addition to the longitudinal mean, to display standard deviation.

Line 447 : ‘In the case of satellite products,’ could be removed.

Line 463 : Remove ‘ESA is funding’.

Line 478 : This -0.4psu difference observed in December with all satellite products is surprising : is it homogeneously spread over the region or concentrated on a few grid points?

Section 3.3.2 : The singularity analysis is interesting as it is a way to check how natural gradients are retained by the various processings. However, I think the ‘best performance’ obtained with BEC L4 is artificial because BEC L4 products are built assuming that SE of OSTIA SST and of BEC L4 SSS should be similar. Hence the results obtained with BEC L4 on Figures A12 and A13 are not independent validation (section 3.3 is validation) but rather a verification of the proper behaviour of the L4 construction methodology (which is good to show). Hence the results presentation needs to be revised. L508 ‘For instance’ might be replaced by ‘By construction’. In the following, the performances obtained with BEC L4 should not be compared with the ones obtained with other products, but they might be presented as an ‘ideal’ case.

OSTIA SST is taken as a reference but OSTIA SST is not perfect. I guess SST SE could be different if derived with another SST field. This could be discussed a bit and maybe an order of magnitude of the uncertainty associated with OSTIA uncertainty could be derived?

Section 3.3.3 : what are the confidence levels on the spectra (Fig A14 & A15) and on their slopes ((Fig A16) ? In rainy regions like ITCZ can we really expect PDS of SST and SSS to be similar ?

L559-560 : I don’t understand the meaning of this sentence: SMOS and SMAP are not the same instrument.

L563-565 : isn’t it by construction ?

Section 3.3.4: I am puzzled with the results (or their interpretation) obtained with the triple collocation. According to Figure A17 and A19, BEC products would have the lowest errors in very variable SSS areas with respect to other products. Why ? Could it be an effect of filtering ? This result is surprising given the Argo comparisons presented in Table A4. For instance, from Table A4 in the Amazone region, the std difference and the RMS difference between satellite SSS and Argo salinity obtained with BEC products are the highest, which suggests a higher error with BEC products. This is opposite to what is indicated with Figure A17 and Figure A19. In the equatorial band, in Table A4, the REMSS products display the best comparisons to Argo which seems again opposite to the errrors displayed on Figures A17 and A19. The apparent contradiction between results obtained with triple colocation and Argo comparisons should be further explored before a conclusion on the error is drawn.

L584 : a verb is missing

L617-623 : a caveat for very variable regions (Amazone) should be added.

L626-628 : I expect this to be by construction (see previous comments)

On the figures displaying maps (A3, A5, A6, A17, A18, A19), latitude and longitude labels are missing.

Figure A13 : The y axis of last figure should be std(SSS).

Figure A13-A16 (in particular A14-A15) : colors are hard to see (especially yellow)

Table A2 : I guess Indic should be Indian ? (same remark for Figures A8-A10)

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