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Comment

Interactive comment on “Sea surface freshening inferred from SMOS and ARGO salinity: impact of rain” by J. Boutin et al.

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Below and attached are our answers to reviewers 2 and 3:

There are no units of salinity used throughout the article e.g. line 19 page 3333 - "in situ salinity measured at various depths between 1m and 10m differ by 0.1 to 0.5 in 20% of the cases ". Salinity is a parameter that has a unit associated with it, and I suggest that the authors insert units where appropriate in this paper.

We agree that no units on salinity may sometimes be confusing and it is common in papers published in remote sensing journals to find salinity expressed in ppt, psu or pss. However, according to Unesco (1985), the practical salinity scale defined as conductivity ratio has no units. Hence, most papers using in situ salinity derived from

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Interactive Discussion

Discussion Paper



conductivity measurements and published in oceanographic journals use the no unit convention (see for instance the following recommendation of The Marine Chemistry journal concerning the units to be used in submitted papers: ‘The salinity can be expressed in ppt (o/oo) if not determined on the Practical Salinity Scale. This scale is not a unit and does not require o/oo, ppt (i.e., $S = 35.000$)’). All the in situ salinities used in this paper have been reported using the practical salinity scale and must therefore be reported without unit. Since L-band brightness temperatures are related to salinity via water permittivity which mostly depends on sea water conductivity, since SMOS Tbs are calibrated every two weeks with respect to climatological salinity on practical salinity scale and in order not to use different convention for in situ salinity and satellite salinity, we apply this no unit convention to both in situ and satellite salinity. Hence we propose to keep the no unit convention in the paper, and to report the above explanations in the data and method section.

UNESCO (1985) The international system of units (SI) in oceanography, UNESCO Technical Papers No. 45, IAPSO Pub. Sci. No. 32, Paris, France.

P3334 L6: can you provide a reference for the comment regarding the ocean haline skin? A reference is given at the end of the corresponding sentence: (Zhang and Zhang, 2012)

P3337 L15: Argo has no data above 5m, so how can there be inaccuracies i.e. when there is no data? Do the authors interpolate to the surface? The inaccuracies referred here correspond to the no pumping effect which has been evaluated during some tests with ARVOR float that are described in the paper. In order to clarify the text we propose to change ‘This leads to...’ by ‘The no pumping leads to...’. We do not interpolate to the surface; we propose to add this in the text.

P3341 L20: Do you have a reference to back up the statement that the ECMWF rain rate is a poor indicator of local rain? This statement comes from comparisons done by ourselves. It is however well known that rain is difficult to predict in exactly the right

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place and time (e.g. Geer et al. 2008).

Geer, A. J., Bauer, P. and Lopez, P. (2008), Lessons learnt from the operational 1D + 4D-Var assimilation of rain- and cloud-affected SSM/I observations at ECMWF. Q.J.R. Meteorol. Soc., 134: 1513–1525. doi: 10.1002/qj.304

Technical Corrections The use of the word "rainy" is not appropriate in this article e.g. abstract L9 should read "when rain events". We agree; We propose to change in the abstract 'SMOS rainy events' into 'when SMOS observations concomitant with rain events' and to make similar modifications in the rest of the paper.

Abstract L7: the sentence beginning "The mean SSS..." should be improved, as it's very unclear. We propose to replace 'The mean SSS –0.1 bias' by 'The averaged negative SSS bias (-0.1)'

Abstract L9: as detected from We agree

P3333 L1: omit particularly We agree

P3335 L15: sentence beginning "Although very..." is awkward and should be re-written We propose to rewrite it as follows: 'Although these studies are very encouraging, they also point out SMOS data deficiencies: large SSS biases related to continents proximity, to radio frequency interferences and to orbit orientation, the latter being likely due to imperfections in the thermal antenna model (Kainulainen et al., 2012).'

P3335 L22: no brackets around Reul et al. We agree

P3337 L21: what is pression? It is 'pressure'; this is a typo.

P3338 L22: on Fig. 1 -> in Fig. 1 We agree

Answers to reviewer 3:

General comments: In this paper, authors investigate the possibility to infer the sea surface freshening from SMOS and ARGO salinity measurements. After explaining

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Interactive Discussion

Discussion Paper



first the context of the study, then the ingredients used and the employed methodology, they show the importance of having SSS variability from satellite and in-situ measurements compared to climatology in particular during rainy events. They discuss about the limitation of the retrieved SSS and argue on several parameters able to play a significant role or not. They found a linear correlation between the SMOS freshening and SSM/I rain rate. Finally they recommend to use satellite measurements only in non-rainy conditions.

There is a confusion here; the recommendation made page 3344 line 20 concerns only calibration and validation activities. In order to clarify our meaning, we propose to replace this sentence by: ‘Concerning calibration and validation of satellite L-band Tb and retrieved SSS using in situ SSS recorded at a few meters depth, we recommend considering only non rainy conditions.’

1.Data and Method §2.3 About SSM/I RR, I suggest to the authors to give an observation error for this particular area. It should be useful to have the accuracy of the RR retrieval compared to the estimated errors of geophysical variables involved in the SSS retrieval algorithm.

Estimating the accuracy of satellite rain rate at high spatial and temporal resolution is a very difficult task given the variability of precipitation within a satellite pixel and the undersampling of in situ measurements. Considering temporal averages, Bowman et al. (2009) found that SSM/I RR retrieved by Remote Sensing System are in good agreement with respect to in situ rain gauge measurements in the tropics. Hilburn and Wentz (2008) found that once the diurnal variability of the precipitations is taken into account, the intersatellite SSM/I bias is less than 3% and, in the tropics, the SSM/I RR is very consistent with the Global Precipitation Climatology Project (GPCP) RR and RR trends.

Bowman, K. P., C. R. Homeyer, and D. G. Stone, 2009. A comparison of oceanic precipitation estimates in the tropics and subtropics, Journal of Applied Meteorology and

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Interactive Discussion

Discussion Paper



Climatology, 48, 1335-1344. Hilburn, K. A. and F. J. Wentz, (2008) Intercalibrated passive microwave rain products from the Unified Microwave Ocean Retrieval Algorithm (UMORA), Journal of Applied Meteorology and Climatology, 47, 778-794.

2. Error source P. 3342 between line 5 and line 15. This section seems unclear, maybe because the effect is not well known in the L-band Tb?

In order to clarify this part, we propose the following modification:

‘Apart from the atmospheric effect, very little is known about the impact of the modification of the ocean surface by rain on L-band Tbs. Actually, sea surface waves may be affected by rain splash, rain created ring waves and rain damping effect, the importance of these effect being dependent on their wavelength. According to (Contreras and Plant, 2006), the resulting effect on the backscattering signal depends on the measured wavelength: the backscattering at Ku, Ka and C-band is dominated by an enhancement effect whereas the rain damping is the dominant effect on L-band backscattering.’

P. 3342, line 19. Retrieved SMOS wind speed is mentioned for the first time. Maybe a reference could be useful to understand where this measurement comes from, is it inferred from the multiangular and position information mentioned P3335 (line10-11)? Is it really a wind speed retrieval? P3343 (line 1-5),

We propose to add the following page 3335: ‘As described in (Boutin et al., 2012), the retrieval scheme implemented in the ESA (European Space Agency) processing uses the Levenberg and Marcquardt iterative algorithm for retrieving SSS, wind speed, sea surface temperature and total electron content, from the multiangular and polarised information contained in SMOS Tbs along a dwell line. Prior values for wind speed and sea surface temperature are taken from ECMWF forecasts; in the version considered in our study, errors of 2 m s⁻¹ and of 1°C have been put on wind components and SST respectively.’

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Interactive Discussion

Discussion Paper



About the SST cooling, maybe it will be interesting to compare SST used by SMOS SSS retrieval and SST measured by ARGO (if it exist) and/or SST from satellite. Here, you could mention the SST used in the SMOS SSS retrieval and the associated error.

We have looked at the comparison of SST used as prior in the SMOS SSS retrieval with respect to ARGO SST colocated within ± 5 days and ± 50 km: the rmse is 0.34°C and 0.37°C in subtropical Atlantic and tropical Pacific regions respectively. No significant correlation of the difference was found with respect to SSM/I RR ($r^2=0.003$). We also compare the SMOS retrieved SST with the prior SST but again no significant correlation of the difference was found with respect to SSM/I RR ($r^2=0.001$) probably because the possible cooling effect (possibly a few tenths of degrees) was small in terms of radiometric signal (less than 0.1K) compared to other error sources.

Technical corrections: Just few comments on the ARGO OI reference which is a bit confusing. The figure caption 1 uses ARGO OI whereas ARGO SSS interpolated map is mentioned on the figure 1. Moreover, in the text, ARGO SSS is referred to ARGO float salinity data but we often find out ARGO single SSS. In addition ISAS appears on Figure 2 whereas it has never been defined (It seems to refer to ARGO OI). Please, once it has been named, use the good reference in all text.

We propose to name the ARGO interpolated maps ARGO OI SSS everywhere in the manuscript, to suppress ARGO 'single' SSS, which appeared in the first submitted version, and replace it by ARGO SSS. This naming convention will be described page 3337 and page 3336.

Please also note the supplement to this comment:

<http://www.ocean-sci-discuss.net/9/C1491/2013/osd-9-C1491-2013-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., 9, 3331, 2012.

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