Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., author comment AC2
https://doi.org/10.5194/hess-2021-637-AC2, 2022

Reply on RC2

Robin van der Schalie et al.

Author comment on "Characterizing natural variability in complex hydrological systems using passive microwave-based climate data records: a case study for the Okavango Delta" by Robin van der Schalie et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-637-AC2, 2022

Dear anonymous referee #2,

Many thanks for your efforts in reviewing the paper, we are pleased to read the positive and constructive response. Below we will reply to the individual comments, either by clarifying our choices or by providing a plan to resolve issues in the revision. With this, we hope we can take away the last concerns, especially concerning the figures and analysis.

Kind regards,

The authors

Majors comments:

1) Concerning the PMW CDR:

- Too little information is given on the inter-calibration and retrieval model. I acknowledge that these two compounds are supported by published materials but the author could add information in an annex on the inter-calibration (such as the cost function used in the optimization during inter-calibration). Also a figure with raw (original) CDR time series over the two regions of interest (ROIs) before retrieving the climatological mean.

Thank you for your advice. We thought the published material in Van der Schalie et al. (2021) would be sufficient to cover the intercalibration. However, as it is perceived as too little, we will take this into account for the revision and extend the section to include more information on the intercalibration.

It is possible to add a figure with the original (non-anomaly) data in the appendix. However, we explicitly made the choice during the paper writing to not include this because we want the main focus to be on their ability to properly detect anomalies. As we feel adding this might be distracting to the main message. Secondly, for the ROI_2 we show the underlying climatology of different datasets in Figure 6.

- In Table 1, the resolution of the pixel measured by PMW at the surface is needed as it is said that Brightness Temperatures (BT) have been aggregated at 0.25° to obtain gridded products.

Thank you for this advice, we will make sure that in the revision we are more specific in distinguishing between the resolution of the gridded product and the resolution of the passive microwave footprints.

- If descending orbit only have been processed such that it can be compared with MODIS, such information is needed p7-L192

The descending orbits were not only chosen because of the MODIS comparison. For passive microwave retrievals (e.g. the soil moisture, land surface temperature and vegetation optical depth) the night time retrievals are also of higher quality. This is caused by the thermal equilibrium during nighttime and the model assumption that the vegetation temperature is equal to that of the land surface temperature. This is not achieved during daytime, which in reality has a much higher variability over time (e.g minutes to hours). This is reflected back in the data quality, being more noisy. Especially as we include TRMM in the analysis, which does not have a stable local overpass time due to its non-polar orbit. Also, because of the timescale of the evaluation being monthly/seasonal, the daytime is not necessary to reach the goal of the study. We will clarify this better in the revised manuscript to avoid confusion.

2) Concerning the LST analysis:

- Land surface temperature from ERA5 (LSTe5) has been extracted only for the first layer (0-7cm), is there any information on the penetration depth from the PMW observation? The infrared MODIS-based LSTmd is used for comparison, as infrared LST has no penetration depth, how this could impact the analysis. Please comment on p12-L315.

Penetration depth for Ka-band observations are about 1 mm, slightly varying with soil wetness (Holmes et al., 2013; Ulaby et al., 1986), so between the IR and ERA5 depths. This information will be added to the revision.

The mismatch in depth is also a reason why we choose for night time comparisons, as there is much more thermal stability expected. Especially when looking over longer periods (e.g. weeks, months) we assume that the slightly different definitions of soil temperature should still show a similarity in underlying anomalies. Not in the absolute

sense, but relatively, as for example can be seen in the comparison of the anomalies in Figure 4. We will make sure that this is clear for the reader in the revision.

- No Time series is plotted for LSTe5 and LSTmd. For a systematic analysis, these two must be added in Figure 4. It should better support the author's statement on LSTe5 through the manuscript (p22-L533; p23-L564) and in the abstract. This is not shown in the analysis yet.

We agree that it would be better to show those images. We previously chose to leave those out to have manageable number of figures. For the revision we plan on either including it in Figure 4 or in an appendix.

- I would suggest adding LSTmd climatology in Figure 6

For both improved interpretability and after Section 4.3 showed a reduced skill of LST_{MD} as compared to LST_{E5} and LST_{MW}, we made the decision to exclude LST_{MD} for this image. If requested, we can add it to the appendix.

3) Concerning the VOD:

- Climatology for ROi1 could be added in an annex to see if the LAI and VOD seasons are less correlated over catchment as it is stated p23-L595.

The statement relates to Figure 3, which is not on different seasonal dynamics within a single season, but more about the intraseasonal (season to season) comparison. Here you can see a more sustained increase in the anomalies for VOD in 2008-2012, as compared to the LAI. This is highlighted in the discussion and linked to the ability of VOD to detect the build-up of biomass in this longer wet period, which is not perceived in the LAI.

-Xband is less sensitive to leaves over dense forest, any experiment has been conducted in using/not using Xband for VOD in the omega-tau model?

The retrievals of soil moisture and vegetation optical depth are entangled, therefore you cannot replace the VOD within the algorithm used by something else in the Land Parameter Retrieval Model. X-band is not less sensitive to the leaves over dense forest, the issue is that the signal gets saturated with the vegetation signal and shows less variability therefore. Choosing another frequency, e.g. L-band, reduces the time coverage of the total record and therefore also the value of the anomalies. For example L-band only goes back to halfway 2010 with SMOS.

4) Concerning the Figures:

- All scatterplots must have Xlabel and Ylabel for clearer reading. I suggest introducing correlation numbers inside the figures.

In the revised version we will add the X and Y labels where missing.

- All correlation numbers must have at most 3 digits as the 4th is not meaningful.

We agree with this statement, and therefore will adjust where necessary to have a maximum of 3 digits (like 0.77) for correlations.

- "Absolute anomalies" in the title is misleading as "absolute" has another mathematical meaning. Replace by "raw"?

We agree that it is currently not completely clear what "absolute" anomalies mean in the context of this study. However, we think "raw" is also confusing. Therefore we will highlight the meaning of the absolute anomalies in the context of this paper. Concerning the z-score, we will explain its meaning as "standardized anomalies".

- Add PRim in Figure 5 as well as PRe5 and PRim for RO1 (can be in annexes) as it is stated that PRe5 has high positive anomalies over the catchment (p21-L478) with no supporting information.

To make sure all the information is included in the figures, we will include the PR_{E5} (ROI_1) and PR_{IM} ($ROI_{1/2}$)in Figure 5 so it includes information on all support data.

5) Concerning the Linear regression experiment:

- RMSE for Z-score is difficult to analyze, pleased replace by bias and std metrics in table2

Due to the completely different underlying values for river discharge, inundated area, precipitation, and soil moisture, we decided to use the z-score anomaly as a tool to normalize this difference and improve the interpretability of the balance between their contributions in the regression activity. The advantage of using z-scores or normalized values in regression is that regression coefficients are directly comparable or interpretable in terms of strength of relationship between dependent and different explanatory variables. In the revision we will better clarify this choice. On top of that, in a regression

exercise the bias does not have much meaning.

- Please consider doing the Linear regression experiment for the catchment ROI1 to see if the SSM is more related to the precipitation upstream (as stated p22-L517).

A linear regression activity does not add the same value as in the Delta. Because ERA5-Land is mostly driven by its precipitation forcing, it will always match best there compared to anything else, without providing any insight into the true quality. The unique opportunity in the Delta is the contribution of the Okavango River inflow and the inundation, so the water is not coming from an individual source.

- In the Table specify the considered ROI.

Thank you for your advice, we will specify the ROI.

- It said that OIAD show some lagging from ORD, could you find optimal lag with cross-correlation between ORD-SSM and SSM-OIAD. This might lead to finding some buffering effect in SSM between ORD and OIAD.

That is indeed an interesting suggestion. We will have a look to see if this is feasible and potentially include in the revision.

Minors comments :

-p2-L36, miss-record

-p2-L51, "the The"

-p3-L63, BAMS, acronym is not defined

-p3-L93, PMW is not defined,

-p4-L111, use Section instead of "Chapter"

-p4-L114, LPRM is not defined yet E5L should be E5

-p7-L195, The sentence is misleading since not only the Xband is used

-p9-L224, what is an E-type gauge?

-p11-L289, VODCA is not defined

-add Z-score equation

-Caption of figure 3 seems misplaced (not attached to the figure on the same page)

-p21-L474, what "memory" replaces to buffer effect?

-p21-L489, SSM not SM-In Fig6: +-15d is used for visualization only or to compute anomalies also? If it has been used for

computing anomalies, this could lead to over-smooth the anomalies with the 90 days moving average

window.

-p23-L534: could you be more specific. The increase of SMM with available solar energy, increases ET and

avoids a false increase of LST but how is LSTe5 between 2011-2014?

-p23-L553 verb is missing

We thank you for the thorough review and will correct or clarify all minor points in the revision.