Revisions for HESS Paper **Multi-decadal analysis of root-zone soil moisture applying the exponential filter across CONUS** (HEES-2017-118)

Please note that modified text in the manuscript is indicated in revised manuscript is indicated in red.

Anonymous Referee #1

Main Comment 1: The distinction among the different eras is done by considering AMSRE soil moisture product as a reference (pre-AMSRE, post-AMSRE etc. . .). Given the paper results, it is also clear that the gaps contained in the surface soil moisture data have an effect on the performance of the exponential filter (although this effect is limited for gaps < 2days). Based on that, I wonder if the rationale in the choice of the different eras should not take into consideration also the advent of the ASCAT sensor. Indeed, according to the paper of Dorigo et al. 2015 (Figure 5) it can be observed a significant increase in the data density after 2007, i.e., when the Advanced Scatterometer ASCAT has started to be operational. From the figure, we can also observe a significant reduction of the data density during 2001-2003, which might have an effect on the performance of the exponential filter (especially after the quality check, which might significantly reduce the data density).

Response: One of the surprising results from this study is that the objective metrics generally did not show much variation over time (lines 363 to 364). Therefore, I do not believe that changing the era definitions would have a great impact on the results of this study. The availability of the ASCAT sensor would only impact results using CCI-Active and CCI-Combined data in some instances. Redefinition of eras would have no effect on RZSM estimates based on AMSR-E or CCI-Passive.

Main Comment 2: It is not completely clear from the paper how the problem of gaps in the satellite soil moisture data is addressed within the application of the exponential filter. I suppose the authors use the exact time difference between two valid satellite observations, i.e., tn-tn-1. When this time difference is large (even weeks in northern CONUS), as it might happen during the winter season, is the filer re-started? Please add a brief discussion on that.

Response: I believe I mentioned that when gaps extend beyond 12 days the filter is reset (lines 181 to 182). This time length is consistent with the major drop-off in objective functions evident during our data denial experiment. Yes, the filter is reset (K=1) when the gaps are greater than 12 days.

Main Comment 3 and Specific Comments I to K: The quality of the figures is not appropriate and some captions are not self-explanatory. This makes difficult the interpretation of the results. I will list below specific comments on them.

Define n in the table . . .. Improve the legend quality and provide explanation about grey areas. . . Add letters to identify subfigures and add a legend.

Response (Main Comment 3): We will improve the figure quality and legends particularly for Figures 1, 2, and 10 and Tables 3 to 6 as suggested.

Response (Specific Comments I to K): See response to main comment #3 above.

Specific Corrective Actions: In the captions for figures 1and 2 the significance of the gray versus white areas are explained. Also the legend quality for Figures 1 and 2 has been enhanced by adding a title. Additionally letters and a legend have been added to Figure 10. Finally, an explanation of what n means has been added to Tables 3 to 6 in the revised paper.

Main Comment 4 and Specific Comments B: The version of the specific CCI product is not mentioned. There are sensible changes between the different versions of the products with respect to the merging procedure and the sensors used (e.g. SMOS) that justify the inclusion of the product version in the manuscript.

Indicate the CCI version here. This is important.

Response (Main Comment 4): We will provide information about the specific version of CCI used in this study in the revised paper.

Response (Specific Comments B): This will be added to the revised paper.

Specific Corrective Action: The requested information is added on line 136 in the revised paper.

Main Comment 5 and Specific Comments E and F: At a point of the manuscript (line 256 pag 10) it reads: "The TOpt and lagged r-values discussed are based on results that have a low absolute bias (± 10%)". Nothing is said before about any distinction between stations with low and high absolute bias and why this distinction is included in the results. Nor the authors provide the number and which of the stations show low and high absolute bias. I found this confusing. This issue of the bias is also confounding with the notation used at page 9 line 224 where SWI is ndefined as "rescaled SWI". Do the authors use any rescaling technique? If so, how it is related with the bias? Which rescaling technique has been used?

"low absolute bias" (see point 5 of the main comments). . .. See previous point

Response (Main Comment 5): More information about the rescaling procedure will be provided in the revised paper which is quite a simple method commonly used in the soil moisture community. As for the mention of low versus high bias this actually turned out to be an

important distinction in data quality where low bias results nearly always outperformed sites exhibiting higher bias. More explanation of the impact of bias on the presented results will be offered in the revised paper.

Response (**Specific Comments E and F): **See response to main comment #5 above

Specific Corrective Action: An addition sentence (lines 182-183) was added describes how the SWI_{mn} generated from the exponential filter was rescaled to match the range of data observed in the *in situ* data. The questions regarding the bias where addressed with sentences added at lines 192-194 and 258-262.

Specific Comment A: Why not add a table of the selected eras. This would simply the reading.

Response: This will be added to the revised paper.

Specific Corrective Action: A new Table 1 has been added to the revised paper that has this information. Also other tables have been renumbered to reflect this revision.

Specific Comment C: "Days in which the minimum air temperature was less than $0 \circ C$ were removed from the SWI dataset." This is an important point. I think it is not completely correct to remove these values at this point (i.e., after the application of the filter). Indeed, given the recursive nature of the filter any surface observation characterized by a temperature lower than $0 \circ C$ has a detrimental effect on future observations. Hence, this masking has to be carried out on the surface observations and not on the SWI.

Response: In actual fact surface observations were actually removed before the application of the filter to less the detrimental effect of sub-freezing retrievals. The revised paper was modified to reflect this fact.

Specific Corrective Action: Section 3.6 has been reworded in places making our approach clearer.

Specific Comment D: Here the authors uses "lag correlation", in section 3.3 simply "correlation" while in the figures "lagged correlation factor". Later lagged r-values. Please use a consistent notation.

Response: We will correct the notation in the revised paper.

Specific Corrective Action: We opted to use lagged r-values and this has been changed to be consistent throughout the revised paper.

Specific Comment G: You may also cite this work (Massari et al. 2014) in which the authors used ERA-Interim root zone soil moisture within a simple hydrological model to infer the catchment wetness conditions before flood events.

Response: We will add this reference to the revised paper.

Specific Corrective Action: This citation has been added to the revised paper.

Specific Comment H: "NS<1" should it be NS<-1.

Response: This will be fixed in the revised paper.

Specific Corrective Action: This mistake has been corrected on line 336.

Anonymous Referee #2: The paper shows an important systematic study of the application of the exponential filter to estimate SWI and RZSD from various SM satellite products. The results are of great interest for research and applications that use these products and prefer a simple and

reliable method such as SWI for estimating RZSD. However, the conclusions are limited to commenting on the different performance of the SWI method comparing RZSD estimates and in situ measurements, using a series of literature metrics (N-S, bias, RMSE). I think the paper conclusions would surely be more effective if it could include further insights in the discussion e.g. linking the failure of the exponential filter in some areas and under some conditions, tying the behavior to hydrological/ morphological factors, e.g. The nature/type of the soil, the landscape, LU/LC, etc.

Response: In the revised paper will more closely explore in more detail why the expo-nential filter failed in certain conditions by examining the land cover, soil types, etc. in the satellite soil moisture grid that is collocated with the in situ station.

Specific Corrective Actions: Added a paragraph (lines 335 to 343) to the discussion section that highlights our results from this analysis of how soils, land use, and climate zones controls the performance of the exponential filter.