Hydrology and
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Discussions

Interactive comment on "Multi-scale analysis of bias correction of soil moisture" by C.-H. Su and D. Ryu

Anonymous Referee #1

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This is an excellent paper that makes a fundamental contribution to soil moisture time series analysis. In particular – it highlights the (temporal) scale dependence of relative multiplicative bias in modeled, in situ and remotely-sensed soil moisture data sets. This is a wholly new insight which has very important consequences for a number of important data assimilation and merging applications. I strongly recommend publication following minor revisions.

Below are some specific points to consider prior to publication. They are all minor suggestions except I did have problems following the motivation for Section 7 (see points 6C and 7 below). In addition, I believe there is a loose end involving additive biases which requires further clarification (point 2).

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1) One advantage of applying time series analysis is that you can assume stationarity (at least in the weak sense that soil moisture expectations will no longer vary across the seasonal cycle). From this point of view, the seasonal cycle at a point is a deterministic feature that must be accounted for before random time series models can be applied to soil moisture time. For this reason, hydrologists often view seasonal dynamics as a unique time scale – and not simply just another time scale in a spectral range. Given that dealing with non-stationarity is a strength of wavelet analysis, the authors might want to discuss the implications of seasonality on their analysis. Instead of avoiding the issue by removing seasonality...it seems like the authors are addressing it head-on (with a statistic tool that explicitly addresses seasonality). This seems like a step forward which might warrant a little more discussion.

2) The other advantage of dealing with anomalies is that you can assume biases are wholly multiplicative and lack an additive component. How are additive biases accounted for in (2-3)? Are they just passed along to the approximation time series at the coarsest scale? This issue is addressed in Section 5 (lines 15-20 on page 9005) via the explicit removal of biases but it also seems relevant to results presented in Section 4.

3) The reference to "Fig. 2" right at the start of Section 3 does not seem consistent with the Figure 2 contained in the manuscript.

4) Superscript "(a)" in (1) is not defined at first use.

5) Figure 3 – clarify difference between (a) and (b) in caption (hard to see small difference in super-script)

6) Figure 5 contains a lot of information...a couple of things I struggled with when interpreting it:

A) In column (a) the "target" is the TC-based results (correct)...but in column (b) the target is unity? That change makes it a little difficult to read the figure horizontally.

C4463

Maybe break-out column (a) into another figure?

B) "OLS" and "TC" can also be rescaling strategies...so it took me awhile to realize the color/symbols refer to strategies for calculating alpha AFTER various re-scaling strategies (listed horizontally along the top of the graphs) have been applied. Is that the correct interpretation of Figure 5? If so - is it really necessary to show the OLS results in each column? We already know they are biased by noise..you can already see that in column (a)?

C) Page 9009. Last paragraph. I don't follow where "...but we also observed noise amplification in AMS at j=3,7..." Is shown. In Figure 5f (top row)? This seems like a key point but it could be tied better to the results in the figures. Does "alpha_Y,j < 1" refer to the "OLS" results in column (f)? If so, doesn't that just indicate the short-coming of OLS as an estimator of post-rescaling alpha's and NOT an indication that the alpha's have been poorly scaled? The author's should consider re-writing this paragraph for increase clarity.

7) Page 9010. I don't quite follow the rationale for linking bias correction and noise correction here. I suspect that my problem is linked to something I missed in Figure 5 (see specific points above...especially point 6C). As a result, while Section 7 is interesting (and seems like a very nice extension of the MRA-based approach presented here), it does not seem tightly linked to the rescaling focus of the paper. However – as noted above – this might be due to my miss-interpretation of Figure 5. I'd recommend that the author's rewrite/re-clarify this connection for future readers of the paper.

8) Section 8. Regarding the potential impact of this work, I'd argue that the authors could be a little more assertive. For instance, it seems likely that the scale dependence of multiplicative biases explains the VERY poor (i.e. negative variance!) TC results that Draper et al. (2013) [RSE, "Estimating root-mean-square errors in remotely sensed soil moisture..."] notes when applying TC to a raw (as opposed to climatological-anomaly) soil moisture time. Also, Yilmaz and Crow (2013) [already cited in paper] demonstrated

the link between poor rescaling and errors in sequential data assimilation. Residual multiplicative bias (at any time scale) will cause filter innovations (i.e., back-ground minus observation) to contain residual signal (i.e., leaked signal). Leaked signal = auto-correlated innovations = sub-optimal filter performance. This is all admittedly a little bit bit-speculative but I would recommend that the author's be a bit more proactive about articulating the potential positive impact of this work. This is NOT a meaningless exercise in statistical estimation and it would be a shame if it was interpreted as such.