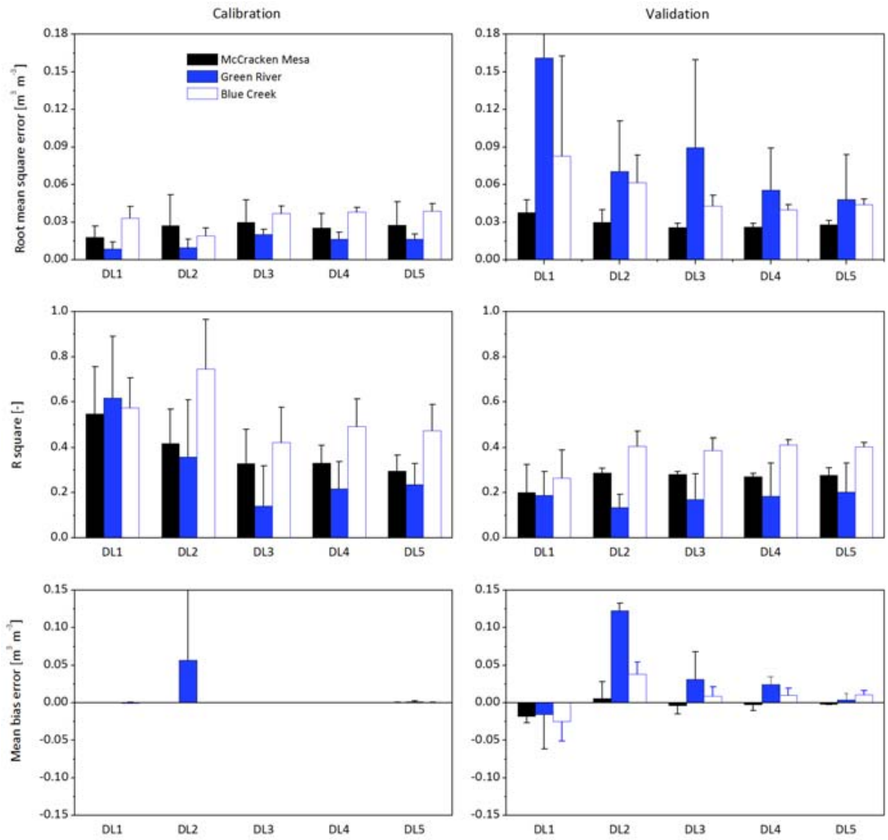
Responses to the comments of Dr. Yijian Zeng

The authors developed a CDF matching-based approach to estimate the profile soil moisture. The preliminary application of the approach over different climate zones (e.g. with a certain number of in situ stations) shows the promising results. I have only two main concern as follows:
>> Thanks a lot for your comments and helpful suggestions. The followings are our point-to-point responses.

Major concern: 1. The author indicated the validation and calibration periods specifically for the current scaling approach. On the other hand, it is not clear why the certain year was selected for validation, why the certain year was selected for calibration. Please, the author clarifies, for example, why 2012, 2013 selected for validation, instead of 2014, 2015? I'm also curious how the selection of different cal/val periods will affect the results.
>> When analyzing the effects of data length on the performance of observation operators, two years was used initially as the maximum data length. The data in the 2010 and 2011 were used for calibration and that in 2012 and 2013 for validation. But we found that two years were not long enough after checking the changes of prediction accuracy with data length. Therefore, we extended the maximum data length to four years and the data in 2014 and 2015 were used. In order to minimize the workload, the data in the 2012 and 2013 remained for validation. And in the following analysis for different climates, soil moisture in 2013 (some stations used 2016 or 2010 because considerable data missing in 2013) was also used for validation.

To our knowledge, the validation of one method requires independent data which however is not necessarily later in time than calibration data. But it is interesting to test the effects of different calibration and validation data on the performance of observation operators. Therefore, we used the data in 2014 and 2015 as validation data and the other data for calibration and made recalculations. The statistics were shown below and the results showed that similar trends of statistical metrics with data length were observed as compared with Figure 3. And the two years of data length remained the optimal data duration. Furthermore, the station of Little Red Fox in the Figure 3 should be McCracken Mesa and has been corrected.



2. For figure 5, you can find negative NSC. There is no further discussion in the paper to explain why. Does it imply a limitation for applying the current developed CDF matching-based approach?

>> We are sorry that the caption of figure 4 is wrong and this should bring confusing. This graph shows the changes of NSC with the T parameter used in the exponential filter method in order to identify the optimal T value. The proper title should be "The changes of Nash-Sutcliffe coefficient with T parameters for nine SCAN stations in different climate regions".

For the Holden station, negative NSC was observed for all T values (from 1 to 50). According to Nash and Sutcliffe (1970) and Albergel et al. (2008), a value of 1 corresponds to a perfect match between predicted and observed data. A zero value indicates that the predictions are as accurate as the mean of the observed data, whereas a value of less than zero occurs if the observed mean is a better predictor than the model output. Therefore, negative NSC values here mean that for the Holden station, the predictions of the exponential filter mismatched the observed data.

Albergel, C., R¨udiger, C., Pellarin, T., Calvet, J.C., Fritz, N., Froissard, F., Suquia, D., Petitpa, A., Piguet, B., Martin, E.: From near-surface to root-zone soil moisture using an exponential filter: an assessment of the method based on in-situ observations and model simulations. Hydrol. Earth Syst. Sci., 12, 1323–1337, 2008.

Nash, J. and Sutcliffe, J.: River flow forecasting through conceptual models, part Ii – a discussion and principles, J. Hydrol., 10, 282–290, 1970.

Minor comments: See supplement.

1. It is recommended to also cite following recent paper on this perspective:

Yu, L., Y. Zeng, Z. Su, H. Cai and Z. Zheng (2016). "The effect of different evapotranspiration methods on portraying soil water dynamics and ET partitioning in a semi-arid environment in Northwest China." Hydrol. Earth Syst. Sci. 20(3): 975-990.

>> We agree. It has been edited in the text.

2. It is recommended to also cite following recent paper on CDF application for spatial upscaling:

Zeng, Y., Z. Su, R. van der Velde, L. Wang, K. Xu, X. Wang and J. Wen (2016). "Blending Satellite Observed, Model Simulated, and in Situ Measured Soil Moisture over Tibetan Plateau." Remote Sensing 8(3): 268.

>> We agree. It has been edited in the text.

3. Please indicated the specific period used for each SCAN station.

>> We agree. They are shown as follows.

(1) Blue Creek, Green River, McCracken Mesa: From Jan. 1, 2010 to Dec. 31, 2015; (2) Centralia Lake, Holden, Molly Caren, Perdido Riv Farms, Sevilleta, Silver City: From Jan. 1, 2013 to Dec. 31, 2015; (3) Shagbark Hills, and Youmans Farm: From Jan. 1, 2014 to Dec. 31, 2016; (4) Willow Wells: From Jan. 1, 2012 to Dec. 31, 2013, and from Jan. 1 to Dec. 31 in 2010. We check that the explanations in line 6 and 7 were wrong and they have been edited in the text.

4. Why such choice? Why not 2010, 2011, 2012, 2013 to establish the observation operators, and then 2014, 2015 for validation? Similar comments for the data length DL4 & DL5.

>> Please see our response to the major concern #1 in Page 1.

5. It is not clear if the authors applied the specific observation operator for individual climate region or not. And if it is so, how the climate regions were classified (e.g. according to which climate classification map), as well as how the observation operator for each climate region was determined (e.g. using average of SCAN station, or certain representative station for that climate zone?)

>> First, the climate regions were classified according to Köppen climate classification as indicated in Table 1. Second, we selected three primary climates in the continental USA in terms of humid continental, humid subtropical and semiarid climates. In each climate, we selected three stations as three replicates to test observation operators. We did not use all stations in a given climate because (1) a lot of stations had considerable missing data probably due to sensor malfunction especially in humid continental and humid subtropical regions, and (2) in our opinion three replicates were enough to do a reasonable test.

6. It is understandable to have an independent well-applied method to estimate the profile soil moisture. On the other hand, it is a bit awkward to call it as the reference method. As in this study, the reference data are from SCAN network, not from exponential filter, as well as the CDF based approach is not developed along the line of exponential filter.

>> We agree. Line 26 in page 6 has been changed into "…, served as an independent method to judge the performance of observation operators in different climate regions".

7. L=L1+L2?

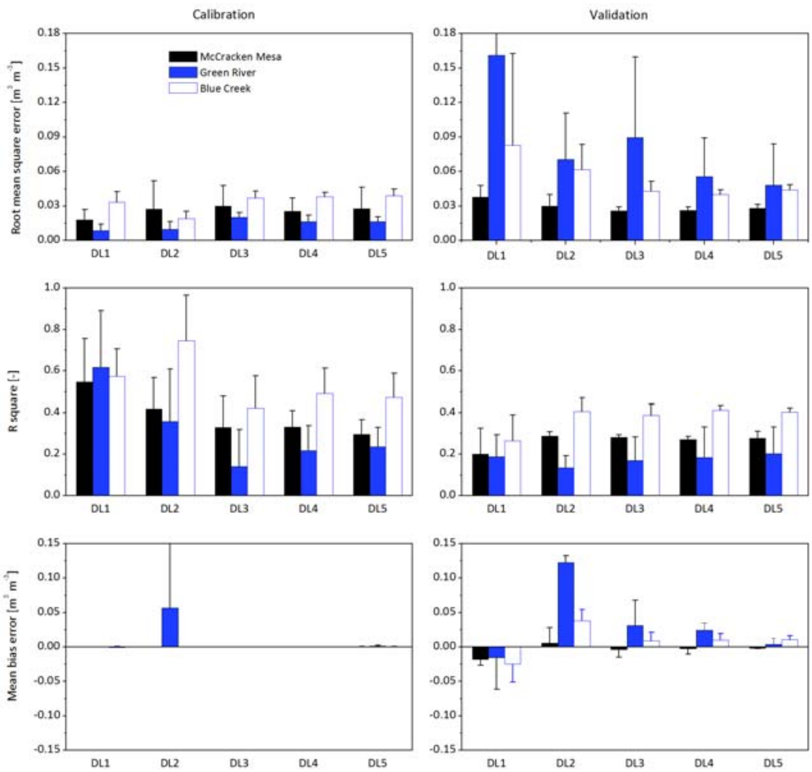
>> L and L2 should represent one parameter, i.e., the depth of the second layer. It has been corrected in the text.

8. What is Tau? What is t?

>> According to Wagner et al. (1999) and Albergel et al. (2008), Equation (8) is the analytical solution of Equation (7). Here both tau (τ) and t represent time. Tau is a temporary parameter and used in order to differentiate t in the integration expression in the Equation (8).

9. I am really curious how the determination of calibration and validation period will affect the results here. Please discuss a bit.

>> We agree. To our knowledge, the calibration data and validation data should be independent each other. But it is interesting to test the effects of different calibration and validation data on the performance of observation operators. Therefore, we used the data in 2014 and 2015 as validation data and the other data for calibration and made recalculations. The statistics were shown below and the results showed that similar trends of statistical metrics with data length were observed as compared with Figure 3. And the two years of data length remained the optimal data duration. Furthermore, the station of Little Red Fox in the Figure 3 should be McCracken Mesa and has been corrected.



10. It is not clear what are those three primary climate regions. Are these nigh stations listed also in Table 1?

>> They are humid continental, humid subtropical and semiarid climates. The nine stations including Centralia Lake, Molly Caren, and Shagbark Hills in humid continental climate; Perdido Riv Farm, Silver City, and Youmans Farm in humid subtropical climate; and Holden, Sevilleta, and Willow Wells in semiarid climate. They were all listed in Table 1.

11. In fact, the exponential filter here is an independent method to estimate profile soil moisture. It seems a bit awkward to call it a reference method, as the reference datasets are from SCAN not from exponential filter.
>> We agree. Line 26 in page 6 has been changed into "…, served as an independent method to judge the performance of observation operators in different climate regions".

12. why this negative NSC values? Does it imply a limitation for the current developed CDF-based approach?
>> We are sorry that the caption of Figure 5 was wrong. Please see our response to the second major comment in Page 1.

13. This is not appropriate. The relative high soil water content is mainly caused by the humid climate with more frequent precipitation events.
>> We agree that the relative high soil water content is because of the high precipitation in the humid climate. Here the optimal T value (T_{opt}) is much lower in humid subtropical climate and a lower T_{opt} reflects a faster response of second-layer soil moisture (w2) to surface moisture (w1) there. Here we mean that the faster response of w2 to w1 in humid subtropical climate can be because of high hydraulic conductivity (vertical water flow velocity) due to high moisture content.

14. This is not appropriate. It is mainly due to the lack of precipitation. However, the hydraulic conductivity is not necessary lower in semi-arid regions than in humid areas.
>> We agree that the low soil moisture in semiarid region is due to the lack of precipitation and that the saturated hydraulic conductivity is not necessary lower in semiarid regions than in humid regions because saturated hydraulic conductivity is primarily controlled by soil texture. But unsaturated hydraulic conductivity is greatly affected by soil moisture content. Dry soils usually have low hydraulic conductivity because water flow in soils is very slow. Therefore, here we mean that the soils in semiarid region are dry and thus the velocity of water flow between surface and subsurface soils is low, i.e., low hydraulic conductivity. This indicates that slower second-layer response to surface infiltration, which corresponds to the higher T_{opt} value in semiarid climate.

15. What is the criteria to choose calibration and validation data?
>> The primary criteria of choosing data are the completeness of the soil moisture observations. In the calibration period, the soil moisture in 2015 for the Perdido Riv Farms and that in 2014 and 2015 for the Willow Wells have a lot of missing data. In the validation period, the soil moisture in 2016 and 2010 was used for the Perdio Riv Farms and Willow Wells, respectively. This is because of considerable missing data in 2013 for these two stations. Furthermore, note that only soil moisture data in 2013 was used for validation for other stations because soil moisture data in 2012 included a lot of missing values for several stations.

16. 'high' soil hydraulic properties?
>> It should be high soil hydraulic conductivity and has been corrected in the text.

17. Please indicate climate zones in the figure.
>> We agree. It has been added in the Figure 4.