Reply letter

Dear Editor,
Thank you for the additional comments which are very helpful. Please find the responses one by one in this reply letter.

1. The comments from Referee 2 are quiet helpful, however, some of them are not well addressed. For example, Question 2.c) and 2.d are misunderstanding. It is normally assumed that higher dynamics of a variable, more difficult to be reproduced and bigger magnitude of errors. Following this logic, the better model performance of depth-layer soil compared to upper-layer soil should be discussed. In addition, Question 4c and Question 7 should be clarified in the revised manuscript.

1) We agree that the comments from Referee#2 are very helpful. Here we did what is suggested in 2.d), in order to address other points. As can be seen in figure 1 below, the surface soil moisture shows larger degree of temporal dynamics in terms of both averaged value and the standard deviation in No assimilation experiment. However, we notice a relatively larger standard deviation in depth-layer (50cm) compared to the surface layer (5cm). This is due to the fact that more soil moisture grid cells are above saturation threshold in deeper layers, which leads to a larger averaged deviation spatially. When the model parameters are perturbed for the 'true' model in the synthetic experiment, the level of saturation especially in depth-layer can be altered, which could lead to a larger RMSE for the depth-layer compared to the surface layer. We agree with the logic that the higher dynamics of a variable, the more difficult it is to reproduce and therefore lead to bigger magnitude of errors. However, when including those inactive soil moisture cells (above saturation), the 50cm layer has relatively larger standard deviation for the entire domain, which make it more difficult to reproduce. This explanation is also added in the revised manuscript (P11 L26-29).

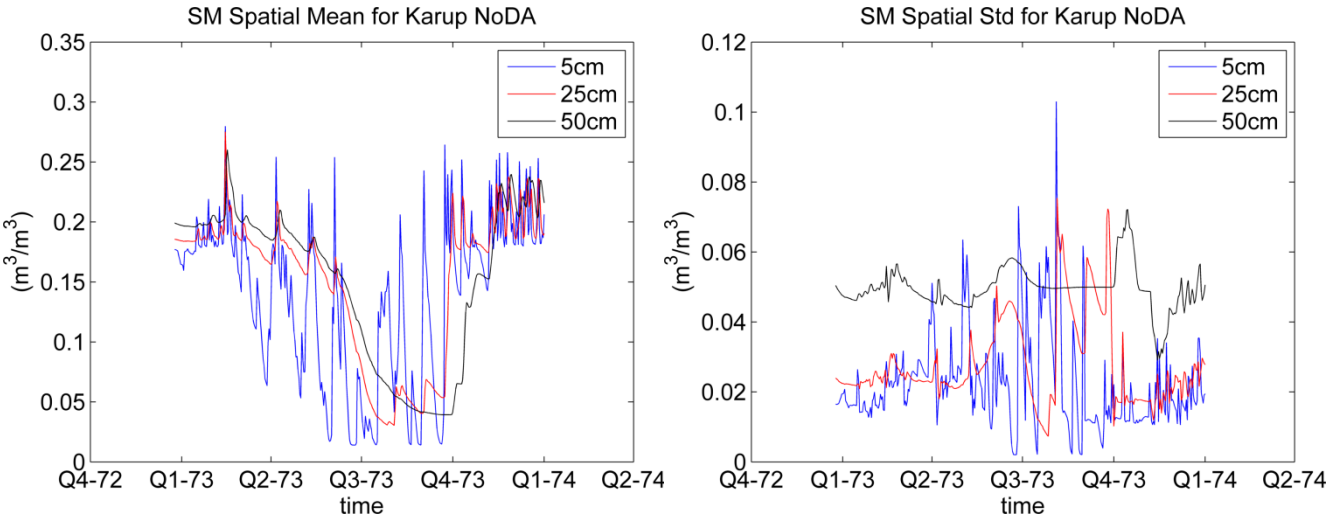

Figure 1 The spatially averaged mean and standard deviation of soil moisture at three depth in NoDA (No assimilation) model for simulation period of 1 year.

The Question 7 is a wrong observation from the reviewer as we also clarified in 1.a. In our study, both synthetic and real cases support this argument. Localization does provide better results compared to the case where no data is assimilated (NoDA).

2. Cross-validation is recommended as the same station data are used as both input and validation. For example, leave 1/3 stations for validation and 2/3 as input.

2) We agree that cross-validation can be adopted to study the DA performance with respect to different observation amount and observation sites. One example can be seen in the referred reference (Zhang et al., 2015). The main focus of this study is to investigate the DA performance with respect to different assimilated variable, which can also be seen as a cross-validation for the integrated hydrological modelling. Overall, in literatures of DA application, it seems that the sites-wise cross validation is not commonly used, not as common as in the model calibration/validation applications. The reason is the fact we usually would like to have the better observations in terms of the quantity, quality and variety assimilated in real DA applications.

3. I think it should be fully discussed (e.g., previous investigations) concerning to "assimilation of groundwater head does not improve soil moisture and vice versa". As is also stated by Referee 3.

3) We agree the result of multivariate assimilation should be more discussed. This is done in the revised manuscript in the last section. We added a long paragraph explaining the saturated and unsaturated zones coupling and the multivariate assimilation result. We believe the readers which are not familiar with how process coupling is done in MIKE SHE can have a better understanding after

reading the latest manuscript. As recommended by Referee #3, We also added the previous findings ((Camporese et al., 2009)as pointed out by Referee #3) and relevant discussion in the revised manuscript (P17 L11-23).

4. The comment from Referee 3: “Page 17, line 4: improvements in discharge and ET are very small”. The suggestion is not addressed.

4) The suggestion is included in the revised manuscript (P18 L4).

5. All the tables and figures should be checked and revised following HESS “Manuscript preparation guidelines for authors”.

5) All tables and figures are checked and revised following HESS “Manuscript preparation guidelines for authors” in the revised manuscript.

Best regards,
Donghua Zhang

Camporese, M., Paniconi, C., Putti, M., and Salandin, P.: Comparison of Data Assimilation Techniques for a Coupled Model of Surface and Subsurface Flow, Vadose Zone J., 8, 837-845, 10.2136/vzj2009.0018, 2009.
Zhang, D., Madsen, H., Ridler, M. E., Refsgaard, J. C., and Jensen, K. H.: Impact of uncertainty description on assimilating hydraulic head in the MIKE SHE distributed hydrological model, Adv. Water Resour., 86, Part B, 400-413, http://dx.doi.org/10.1016/j.advwatres.2015.07.018, 2015.