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Hydrology and Earth System Sciences Discussions

Interactive comment on "Monitoring of water and carbon fluxes using a land data assimilation system: a case study for southwestern France" *by* C. Albergel et al.

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1 Summary

The manuscript investigates the impact of assimilating surface soil moisture and LAI information into a land surface scheme to improve the models predictive skills. It analyzes the impact of single and joint parameter assimilation using a simplified extended Kalman filter approach. Different model setups with different forcing and paramterization are used. The potential of using a simple proxy for root zone soil moisture which is derived from surface soil moisture only observations is evaluated.

The paper presents new results as it is using a SEKF scheme for surface soil moisture assimilation. Previous studies have mainly focused on the use of EKF or EnKF as well as variational techniques. More rectently, also particle filter methods have been adressed.

The paper is very well structured and written in a concise manner. I recommend the paper for publication, subject to modifications concerning the following comments.

2 Comments

2.1 Majors

- section 2.3,3.3: the authors use the dimensionless SWI as an additional proxy for root zone soil moisture. They argue that the simple exponential filter approach might be used as an alternative to an LDAS where the needed meteorological forcing is missing and where applications mainly focus on soil moisture dynamics. However, I doubt if these conclusions can be easily drawn from the analysis presented in the paper. A critical aspact of the exponential filter is the choice of the characteristic time scale parameter T. The authors used a value of $T = 11$ days from a previous study. This value was obtained by minimizing the differences between the filter results and measured profile soil moisture for the same site (SMOSREX) as used for the present study. Thus it is best calibrated for the conditions of that particular site. Other authors suggest different characteristic lengths (e.g. 20 days Wagner et al., 1999). As the T parameter was already calibrated to the local conditions, it is clear that it provides good results for the soil moisture profile in the present study. However, one can not argue that one will obtain similar predictive skills in other (even close nearby) sites. The authors therefore need to futher ellaborate if the approach is really transferable and could really be used as an alternative to LDAS for certain (limited) kind of applications.

- section 2.5.: the authors set the errors for the model and observations based on a priori assumptions of the error statistics which are mainly based on findings from previous studies. An appropriate selection of the error (co)variances is crucial for the performance of the Kalman filter. The Kalman filter allows to analyze if these assumptions are in general valid by analyzing the filter innovation statistics. The innovations of the filter should be serially uncorrelated with zero mean if the assumptions for **B** and **R** are correct. The authors are asked to provide additional information about the innovation statistics and discuss if the assumed errors are correct.

- section 3.1: CDF matching: The authors apply a CDF matching prior to the assimilation of the surface soil moisture observations. The application of CDF matching is an important step to ensure that the general assumptions of the Kalman filter (unbiased zero mean differences) are matched. However, from an application point of view, one might want to calibrate the CDF polynomial using data which is independent from the observations assimilated into a model. Typically a monitoring of the relationships between the observations and model predictions will be used for that purpose for a training period. The estimated polynomial might then be applied for the data assimilation. We wonder, if the authors did apply such an independent CDF calibration. If not, what would be the impact if the CDF function would be estimated from a subset of the available data. We expect that additional bias might be introduced in the analysis if the CDF calibration is based on a subset that does not represent the full variability of the data. Could the authors comment on this issue and it's impact to their results?.

- section 3.3/Table 4: While the analysis of the different assimilation experiments results in absolute error estimates [m^3/m^3], the errorof the exponential filter (SWI) results are given as relative values. This is a bit confusing and makes

it different to compare the different results. We therefore recommend to convert the relative error in absolute errors of the model by rescaling the errors using the wilting point and field capacity of the model respectively. It is suggested to provide these rescaled errors in addition to the relative errors for the SWI.

- section 3.4.2: The section on the assimilation of TB derived soil moisture is rather short. The main rationale of the paper is the investigation of the general sensitivities and potentials in assimilating multivariate observations (sm and lai) to improve the predictive skills of the model. However using *real* brightness temperature data is very important for the practical application of the methodology. We suggest to investigate a few more aspects in the TB assimlation scheme: a) the optical depth is a very critical parameter in the inversion of SM from brightness temperatures. Typically, the optical depth is parameterized using information on the vegetation water content or multiangular measurements (which are available in the case of the LEWIS radiometer). The optical depth is correlated with LAI. It is recommended that authors comment if and how they did include the relation between LAI and optical depth in their analysis, which leads to a more consistent assimilation setup. Further, it has been shown in recent studies, also taking the SMOSREX data set, that a litter layer can have considerable impact on the microwave emission and might detoriate the soil moisture retrievals (e.g. recent papers from Saleh et al). Have authors taken into account litter the effect? How will this change the results of the TB assimilation, if this is done?

2.2 Minors

- eq. 1,3: what does the superscript '0' mean (e.g. x^0). We guess it's the beginning of the assimilation window, but this should be clarified. The difference between x_f^t and x_f^0 is therefore not clear. Please clarify.

- p. 1712, l. 8: how large is N?

- p.1714, l.27: a rooting depth of 95 cm seems to be rather high for grassland. can authors provide a reference for that value?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1705, 2010.