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Hydrology and Earth System Sciences Discussions

Interactive comment on "Application of satellite microwave remote sensed brightness temperature in the regional soil moisture simulation" *by* X. K. Shi et al.

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Dear reviewer:

Thanks very much for your suggestions on how to improve our manuscript. There are differences between our research objective in this paper and what you want. Your general comments and specific comments are responded one-by-one as shown bellow.

Response to general comments

Paragraph 1: "The submitted paper aims to investigate the potential of integrating satellite based surface SM estimates for an improved modeling of land surface fluxes within

a test site in China".

Response: In our paper, we said "The objective of this paper is to provide a simple approach to assimilate satellite microwave remote sensed brightness temperature data into the WRF model for improving the regional simulation of SM." and we didn't say to do an improved modeling of land surface fluxes.

Paragraph 2(1): "The objective of this paper as such is a very interesting and challenging topic".

Response: Thanks for your positive commend.

Paragraph 2(2): "The authors present the result of their study in a very basic manner. They do not provide any information on the performance/skills and/or improvement of the model simulations using the satellite data".

Response: Thank you for your suggestion. First, the objective of this paper is to provide an approach to apply satellite microwave remote sensed brightness temperature data into the WRF model for improving the regional SM simulation. Secondly, at temporal scale, Root Mean Square Error (RMSE) values between simulated and observed SM are decreased 0.03 and 0.07 m3/m3 by using the NR method in the Maqu Station and the Tanglha Station, respectively. Third, according to your advice, statistical inter-comparison like e.g. rms error, correlation coefficient has been processed now in the revised manuscript. Fourth, the inter-comparison of surface air temperature and humidity or the fluxes are our next objective.

Paragraph 2(3): "The used data sets show in general a very low variability. However, the authors come to major conclusions (section 6) that SM retrievals from AMSR-E have a good accuracy and show good agreement with the in situ observations".

Response: There are misunderstandings here. In this paper, we concluded that "The result shows that the SM values estimated by this method are acceptable and the SM distribution is somewhat better than that from the AMSR-E daily product over the eastern part of the Qinghai-Tibet Plateau." and "The temporal variations of the estimated SM are in good agreement with ground daily precipitations". The last sentence has been modified as "The temporal variations of the estimated SM show a good response to ground daily precipitations" in revised manuscript.

We have modified these sentences in revised paper to make meaning clear and accuracy. A field experiment was conducted in Badain Jaran desert in 2008, results showed that the measured SM are almost all smaller than 0.10 m3/m3. So, our estimate and AMSR-E SM product are better than that of the NCEP. Second, our description might be confuse, we want to express that the distribution of estimated SM shows a good correspondence to the distribution of the desert. We have added these descriptions in the revised manuscript.

Paragraph 2(4): "Further, they conclude that the used data assimilation techniques is highly favourable compared to interpolation methods".

Response: Thanks for your suggestion. In this paper, we said "Compared with the NO or DI method, the NR method shows a great advantage in simulation of SM variation". The word "great" is not accurate here, we have changed it as "better" in revised manuscript. In addition, the NR method is also an interpolation method in deed.

Paragraph 3: "Some of the substaintial limitations of the papers might probably me associated to the fact that the data analysis is limited to a very short (1 month) time period. The extension to a longer time record and much more substantial data analysis would be favourable for the paper".

Response: Thanks for your advice. First, Zhao et al. (2007) used this method to estimate the SM in Anni station in central Tibetan plateau and the results are satisfy. Second, WRF model is a weather model and usually used for short time simulations, We think one month SM is enough for testing WRF simulation results. Third, our method to estimate SM is suitable for a short time for decreasing errors induced by the hypothesis in parameter settings. To get a longer time record by using this method only once time,

however, it needs further proof-testing.

Zhao, Y. Z., Ma, Y. M., Huang, Z. and Yuan, T.: The retrieval of soil moisture from TRMM/TMI data in central of Tibetan plateau, Chin. J. Pleateau Meteorology, 26(5), 952-957, 2007.

Paragraph 4: "The English of the manuscript needs major revisions".

Response: Thanks for you kind suggestion, grammatical and logical errors have been modified in revised manuscript.

Response to specific comments

Comment 1: "…and thus influence the climate change by land-atmosphere interaction in the near surface layer": Authors state here, that SM influence climate change. I hardly doubt that this statements holds. One can argue, that land-atmosphere coupling is (in some parts of the world) highly dependent on SM availability.

Response: There is a logical error in this sentence. We have corrected this sentence in the revised manuscript.

Comment 2: '…which couples a land surface model'. This sentence is not understood.

Response: We have modified this sentence in the revised manuscript. Land surface models can be coupled in some numerical models (such as the WRF model) to provide bottom conditions for air simulations.

Comment 3: The paper completely lacks a review of state-of-the art in SM monitoring using satellite data.

Response: A review of recent progress for estimating the SM from satellite data by using this method has been added in the revised manuscript.

Comment 4: Authors state that microwaves can penetrate vegetation. This statement is not valid, as the penetration depth into the soil as well as through the vegetation is highly frequency dependent. Further, current SM retrieval algorithms always have to compensate for vegetation effects. None of this is outlined in the paper.

Response: The meaning of this sentence in paper is not clearly, and this sentence has been deleted in the revised manuscript. Its meaning is explained in the following sentence "Compared to the SSM/I, AMSR-E's 6.9 GHz and 10.6 GHz channels have much longer wavelengths, which have better ability to minimize the effect of surface roughness and vegetation cover and are more sensitive to the change of dielectric constant of soil". Discussion about the contribution of vegetation layer on SM retrieval are expressed in the revised manuscript.

Comment 5: The authors give references to some papers related to SM retrieval from satellite data. However, no review of the literature is given in terms that the results of different studies are related to each other, nor it becomes clear how this relates to the present study.

Response: Thanks for your advice. These references have been deleted. A review on recent progress for estimating the SM from satellite data by using this method has been added in the revised manuscript.

Comment 6: "Huang et al. (2007) confirmed that the assimilation procedure. . .". Which one, there are so many? Relationship to present paper?

Response: The assimilation procedure is ensemble Kalman filter. This reference is used to surpport the necessary of using assimilation procedure. In the revised manuscript, we have deleted this reference and added a review of recent progress in Newtonian relaxation method for assimilating the observation data.

Comment 7: WRF model: no description of WRF model is presented by the authors, nor an appropriate reference is given

Response: Descriptions and references about WRF model have been added in the revised manuscript.

Comment 8: Lack of review of state-of-the art of hydrological data assimilation.

Response: This has been provided in the response to specific comment 6.

Comment 9: Where does land cover information come from?

Response: It is the USGS data and used in WRF model.

Comment 10: The authors use brightness temperature products from AMSR-E(L2A). The authors state that the spatial resolution is 12 km. The frequency channels of AMSR-E that might be used for SM retrieval have a much coarser spatial resolution. It might be, that the product the authors use has been resampled to a 12 km grid, however this does not enhance the resolution of the data set. A discussion of that point is missing and it seems to me that authors took the product "as is" within their study.

Response: Thanks for your suggestion. The satellite brightness temperature data deployed in this investigation are the AMSR-E L2A re-sampled product which spatial resolution is 12 km, but its real original resolution is 25 km. We have revised this description in the revised manuscript.

Comment 11: Noah model description is not understood.

Response: We have revised this paragraph for grammatical errors. This Noah LSM is coupled in WRF model and its description can be found in WRF handbook. The reference of this handbook is given in the revised manuscript.

Comment 12: The vegetation optical depth is also polarization dependent.

Response: The reference of Meesters et al. (2005) had been given to parameterize the vegetation optical depth.

Comment 13: Authors provide a simplified equation for MPDI. What are the assumptions behind that simplification; please give reference.

Response: We have added the references in the revised manuscript.

Comment 14: The authors rewrite the formulation for MPDI under the assumption that some of the model parameters, namely the optical depth, roughness parameters and temperature difference between air and skin surface show a low temporal variability and can be replaced by a monthly mean value. While this statement might be valid for limited time periods and places in the world, it will certainly no be valid in large parts of the world. Temporal dynamics of vegetation optical depth is dependent on vegetation dynamics and might change rapid changes e.e. during springtime. Further optical depth is also dependent to intercepted water. The difference between air and skin temperature (dt) is dependent on the amount of sensible heat flux and certainly not constant throughout the month. As the assumption behind Eq. 9 is rather essential for the data analysis within the paper, authors have to provide much more justification why they think that this assumptions holds within their test site, within the specific time period investigated within the paper.

Response: In the revised manuscript, the sensitivities of parameters Q, h and dt to SM retrieval in this method have been discussed.

Comment 15: "The nudging method…". This sentence is not understood at all. What is meant by inertia-gravity wave?

Response: We have revised this sentence. "inertia-gravity wave" is a clerical error here, we have deleted this part. Thanks for you suggestion.

Comment 16: The authors propose a nudging scheme for the assimilation of satellite observations into a soil model based on the Richards-equation. Within each assimilation procedure, an appropriate characterization and balancing of model and observation uncertainties is critical. While the assimilation as such is the central part of the paper, the authors do not provide any information how they consider uncertainties (model, observations) within their procedure. In section 5 the authors provide some information about how they set the uncertainty parameters within Eq.11. However, these seem more or less to be arbitrary chosen. In most cases the authors set the quality

factor to 1.0 and thus giving full confidence to the satellite observations which is certainly an invalid approach, as many studies have shown the potential but also the limits and uncertainty ranges of AMSR-E SM products.

Response: This question has been answered in the last section of the revised manuscript. The shortcoming of NR method is that its parameter settings need an experience. The best way to set the quality factor is to use the statistical errors between actual SM values and estimated SM values. However, these statistical errors can't be achieved in a short period for lacking of actual SM measurements in the Qinghai-Tibet Plateau. So, the quality factor is set according to the SM difference between estimated SM and representative SM at the same land use category. In addition, there is a clerical error that the quality factor should be 0.9 instead of 1.0 in this paper. The four-dimensional assimilation weighting function is set to 1.0. Hurkmans et al. (2006) designed three empirical functions for setting the four-dimensional assimilation weighting function. Another important parameter that could influence the assimilation result is the model's error, however, the NR method is limited by its definition without considering the effect of model error and regards it as zero.

Comment 17: The authors provide a qualitative intercomparison between station SM and NCEP/AMSR-E SM data. Statistical intercomparison and provision of standard skill scores like e.g. rms error, correlation coefficient, model efficiency is missing.

Response: Statistical intercomparisons by rms error and correlation coefficient have been added in section 4 and section 5 in the revised manuscript. Thanks for your suggestion.

Comment 18: The SMOS launch has been postponed. Please modify.

Response: Thanks, we have modified this in the revised manuscript.

Comment 19: After the assimilation, the authors compare the assimilated model runs again against in situ data. They only provide RMS error as a statistical score, which as such does not contain any information whether the assimilation procedure was able to improve the model predictive skills. Further statistical skills are required.

Response: This also has been provided in the response to specific comment 17 and the response to general comment paragraph 2(2)

Comment 20: Fig.3 and 5. The two figures show both the measured SM in comparison to simulated and satellite derived SM. For the analysis of the assimilation experiment, the authors use only the second half of July 2008. Why, if the entire month is available?

Response: We didn't clarify that we are running WRF instead of the offline Noah LSM. If we use the offline Noah LSM, then the simulation time is very short. However, the WRF model was used in this paper, and it consumes more computer time because of its complicity and is usually used for short time simulations. So we just used data in 14 days to test our method in this paper.

Comment 21: The different simulation results, shown in Fig.5 are rather different from the in situ observations, which show almost no dynamic.

Response: First, the situ observation data are got at 14:00 every day, the simulated data are got at 6h interval every day, so the variation of simulated SM can't be revealed completely by the situ observations. Second, the frequency of inputting data to model from the NCEP data is four times that from the estimated SM, and it make the estimated SM has a uptrend at un-assimilation time because of high NCEP data. Third, to improve the SM simulation in WRF by using the estimated SM is our objective, and the results of rms error and correlation coefficient show that the best simulating result is achieved by assimilating the estimated SM with the NR method.

Comment 22: however, the authors come to the following conclusions: (1) AMSR-E SM is "acceptable" for the analysis of the present study: How do the authors define acceptable? From Fig.3 it can be seen that only the dynamics at the station Maqu is in some agreement with the in situ observation. Where do the authors gain their

confidence that the AMSR-E data is useful for their purpose? (2) AMSR-E SM is in good agreement with local precipitation measurements: Fig.3 does not support that statement. Some examples: Tanghlha station: Strong precipitation event on Jul 3rd: AMSR SM goes down!; Precipitation event on 14th, 24th: SM goes down or remains constant! (3) NR data assimilation method shows superior performance: Fig.5 does not support that statement. None of the model simulations seems to be in agreement with the local SM observations.

Response: (1) We have revised this paragraph. First, according to the data of rms error and correlation coefficient, the values of estimated SM and SM product are better than that of the NCEP, and the values of estimated SM are in the reasonable range of SM measurement, which is lower in desert region and higher in grassland. Second, the distribution of estimated SM shows a good correspondence to the distribution of the land cover type. In addition, more response also has been provided in general comment paragraph 2(3).

(2) The data of precipitation is 24h accumulated precipitation, the data of SM is at 14h. These precipitations may happen before or after the time 14h. We also can see that the situ observations have the same situation.

(3) This also has been provided in the response to general comment paragraph 2(4) and in the response to specific comment 21.

Response to technical corrections

Comment 1: 1235,1.2: partitioning

Response: Thanks, we have accepted this advice in the revised manuscript.

Comment 2: 1235,1.10: "to get soil moisture" : better retrieve, obtain, measure...

Response: Thanks, we have accepted this advice in the revised manuscript.

Comment 3: 1237,27: "„,keeps human activity away...". This does not sound.

Response: We have modified this sentence in the revised manuscript.

We acknowledge the reviewer's comments and suggestions very much, which are valuable in improving the quality of our manuscript.

Sincerely yours

Xiaokang Shi on behalf of all authors

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1233, 2009.