

Review of the paper:

Shi et al.

Application of satellite microwave remote sensed brightness temperature in the regional soil moisture estimation

General comments

The submitted paper aims to investigate the potential of integrating satellite based surface soil moisture estimates for an improved modelling of land surface fluxes within a test site in China (Qinghai-Tibet plateau). The authors use AMSR-E derived soil moisture estimates and integrate these into a land surface model (NOAH). For the validation of soil moisture simulations and satellite retrievals, information from 2 meteorological stations are used.

The objective of the paper as such is a very interesting and challenging topic. However, the submitted manuscript does not make a considerable contribution to the subject. 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. The used data sets show in general a very low variability. However, the authors come to major conclusions (section 6) that soil moisture retrievals from AMSR-E have a good accuracy and show good agreement with the in situ observations. Further, they conclude that the used data assimilation techniques is highly favourable compared to interpolation methods. I doubt from the results presented in the manuscript, that any of these conclusions can be drawn from the presented data sets.

Some of the substantial limitations of the papers might probably be 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.

In addition, the English of the manuscript needs major revisions. It is sometimes very hard to follow the ideas of the authors. Given the major scientific deficits of the paper, it does not make a considerable contribution to a paper like HESS and I therefore recommend to reject the paper. See specific comments below.

Specific comments

1235, l 6ff: "... and thus influences the climate change by land-atmosphere interaction in the near surface layer": Authors state here, that soil moisture 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 soil moisture availability

1235, l. 9: "... which couples a land surface model": This sentence is not understood.

1235, 20ff The paper completely lacks a review of state-of-the art in soil moisture monitoring using satellite data.

1236, 2 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 soil moisture retrieval algorithms always have to compensate for vegetation effects. None of this is outlined in the paper.

1236, first paragraph The authors give references to some papers related to soil moisture 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. Example: “Simonetta et al. (2006) estimated SM from AMSR-E observations by applying a simple RT model. McCabe et al. (2005a,b) also estimated the SM from AMSR-E by a single frequency channel radiation transfer model and then discussed the impact of vegetation on SM calculations.” This is mostly a qualitative description of the content of these papers, but does not provide any insight into the conclusions that were drawn from these papers, nor how they relate to the present study. I would discuss about the obtained accuracies and thus a conclusion what accuracies are expected from AMSR-E SM data for the present study.

1236, 26ff: “Huang et al. (2007) confirmed that the assimilation procedure ...”: which one, there are so many? Relationship to present paper?

1236, 8 WRF model: no description of WRF model is presented by the authors, nor an appropriate reference is given

1237: Lack of review of state-of-the art of hydrological data assimilation

1238, 6 where does land cover information come from?

1238, 21 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 soil moisture 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.

1239, first paragraph: Noah model description is not understood

1240, 14 The vegetation optical depth is also polarization dependent

1241, Eq 8 Authors provide a simplified equation for MPDI. What are the assumptions behind that simplification; please give reference!

1241, Eq.9 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 not be valid in large parts of the world. Temporal dynamics of vegetation optical depth is dependent on vegetation dynamics and might change rapidly e.g. during springtime. Further optical depth is also dependent on 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 assumption holds within their test site, within the specific time period investigated within the paper.

1242, 12 “The nudging method ...” This sentence is not understood at all. What is meant by inertia-gravity wave?

1242 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 soil moisture products.

1245 The authors provide a qualitative intercomparison between station soil moisture and SNCEP/AMSR-E soil moisture data. Statistical intercomparison and provision of standard skill scores like e.g. rms error, correlation coefficient, model efficiency is missing.

1246, 9 The SMOS launch has been postponed. Please modify

1249 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

Fig. 3 / Fig. 5 The two figures show both the measured soil moisture in comparison to simulated and satellite derived soil moisture. For the analysis of the assimilation experiment, the authors use only the second half of July 2008. Why, if the entire month is available? The different simulation results, shown in Fig. 5 are rather different from the in situ observations, which show almost no dynamic however, the authors come to the following conclusions:

- 1) AMSR-E soil moisture are “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 Macqu is in some agreement with the in situ observations. Where do the authors gain their confidence that the AMSR-E data is useful for their purpose?
- 2) AMSR-E soil moisture is in good agreement with local precipitation measurements: Fig. 3 does not support that statement. Some examples: Tanghaha station: Strong precipitation event on Jul 3rd: AMSR soil moisture goes down!; Precipitation event on 14th, 24th: soil moisture 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 soil moisture observations.

Technical corrections

1235, l 2: partitioning

1235, l. 10 „to get soil moisture“: better retrieve, obtain, measure ...

1237, 27: “... keeps human activity away ...“ This does not sound