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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:

We are very grateful for your comments to our manuscript. We revised the manuscript in accordance with your advices, and carefully proof-read the manuscript to minimize the grammatical and logical errors. Here below are one-by-one response to your general comments and specific comments.

Response to general comments

Comment 1: The authors just used Yan's method but do not present any evidence on its reliability throughout the paper.

Response: 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, the statistical inter-comparison like e.g. rms error, correlation coefficient has been investigated additionally. These two points were added into the revised manuscript.

Comment 2: Why the authors, without showing observed data, consider SM values ranging over 0.06-0.08 m3/m3 are reasonable in the deserts?

Response: 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.

Comment 3: The comparison between in situ data and satellite remote sensing is not scale-match but the authors did not mention the possible effect of the heterogeneity of SM on the comparison?

Response: Thank you for your suggestion. Scale-match is an important factor to influence the comparison results. With considering the scale-match between in situ data and satellite remote sensing or model simulating results, these two observation stations were all set up in flat and open topography to increase their regional representative and minimize this scale-match problem. In the revised manuscript, we have added these descriptions of the possible effect of the heterogeneity of SM on the comparison.

Response to specific comments

Comment 1: What is the meaning of Ta and how to get its value or how to define the thickness of “the uniform atmospheric layer”?

Response: Ta is the physical temperature of a uniform atmospheric layer (Jin 1998;

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Yan and Jin 2004). In the reference Yan and Jin (2004), Ta is rewritten as Ta=(1-dt)Ts with 0<dt«0. We think the uniform atmospheric layer should not be a deep air layer above ground. And then the parameter dt could be used in these method, sensitivity test shows that the increase of dt from 0 to 0.2 in grass land makes the surface SM value could decrease 0.02 m3/m3, so it has few effect on the SM retrieval. In the revised manuscript, the sensitivities of parameters dt,Q and h to SM retrieval in this method have been discussed.

Jin, Y.-Q.: Monitoring regional sea ice of China's Bohai sea by SSM/I scattering indexes, IEEE J. Oceanic Eng., 23(2), 141-144, 1998.

Yan, F.-h. and Jin, Y.-q.: Statistics of the average distance of polarization index derived from data of space-borne microwave remote sensing and soil moisture mapping, Chin. J. Radio Sci., 19(4), 386-392, 2004.

Comment 2: 'In general, 0<Q<0.5, Q=0.174 is taken in this investigation.' However, it is indeed important to determine Q and h. At least, the authors should discuss the sensitivity of such a choice or provide any evidence why they make this choice. Response: In the revised manuscript, the sensitivities of parameters Q, h and dt to SM retrieval in this method have been discussed.

Comment 3: "the increasing of the SM boosts R+ much more than R-, therefore it yields a bigger radiation difference and MPDI value.", any mistake here?

Response: This sentence has been revised as "the more increase of SM, the more increase in R+ than in R-, therefore it yields a bigger radiation difference and MPDI value."

Comment 4: R+ is a function of SM, Q, and h, uncertainty in Q and h would affect the derived SM. Please explain the sensitivity.

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

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Comment 5: There is no tail on how to determine the weighting function w(x,y,1,t).

Response: We have added this description in revised manuscript. In our investigations, the four-dimensional assimilation weighting function is set to 1.0, it means there is no influence on any other points or times when the estimated SM is assimilated into model.

Comment 6: Is the monthly average of the derived SM equal to AMSR-E global monthly SM product? How about their similarity in temporal variability? It would be desirable to show AMSR-E global monthly SM product in Fig.3 and 5.

Response: The comparison result shows the derived SM almost close to AMSR-E global monthly SM product. We also have added the AMSR-E global monthly SM product in Fig.3. In addition, we only estimate SM for one month, the similarity of temporal variability between the derived SM and AMSR-E global monthly SM product are not analyzed.

Comment 7: The nominal spatial resolution of AMSR-E product is 25 km or 54 km or so. Please explain why the resolution is 12 km in this paper.

Response: 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 original spatial resolution is 25 km. We have revised this description in the revised manuscript.

Comment 8: 'The estimated SM values in the desert areas are ranged 0.06-0.08 m3/m3 and indicate a good correspondence to the distributions between the estimated SM and the land use classification.' The authors did not provide any observed data for the individual land use types. To show the reasoning, please provide observed data or relevant references for the observed data.

Response: This has been provided in the response to general comment 2.

Comment 9: "The NCEP SM provides much higher values than the ground observations, while the SM estimated from AMSR-E yields lower values than the ground observations. Furthermore, the estimated SM has smaller relative error compared to

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the NCEP SM.” This description is confusing. “Furthermore” should be “however”?

Response: Thanks for your advice. These errors have been corrected in the revised manuscript.

Comment 10: “Gruhier (2008) pointed out that, the daily AMSR-E SM product is not able to capture absolute SM values at current stage, but it provides reliable information on land surface SM temporal variability, at seasonal and rainy event scale.” This conclusion might be specific rather than a general one. Contrast results are found by Choi and Jacobs(2008). Again, I suggest the authors to show AMSR-E global monthly SM product in Figures 3 and 5.

Response: AMSR-E global monthly SM product has been shown in Figures 3 in the revised manuscript, and it shows a low but reasonable value.

Comment 11: “Based on previous comparison, the estimated SM is smaller than the ground observations. A possible reason is that the monthly average SM resulted from the monthly AMSR-E SM product at July is not accurate enough though several other factors have been considered, such as the soil type, surface roughness and vegetation optical thickness”. I could not understand why SM heterogeneity is completely neglected here. This would be a major factor to interpret their difference. At least, the authors should clarify the possible effect of such spatial heterogeneity of SM when interpreting the difference from observed one.

Response: This has been provided in the response to general comment 3.

Comment 12: A review of recent progress in AMSR-E land data assimilation techniques is expected.

Response: A review of recent progress in Newtonian relaxation method for assimilating the observation data has been added in the revised manuscript.

Comment 13: Figures 5: After assimilation of estimated SM into Noah LSM in WRF, the authors only presented the results of SM; however, what we do expect is its effect on the land-atmosphere interactions. Perhaps, the effect of SM assimilation in WRF is not much different from the assimilation in the offline Noah LSM. I would like to see the comparison of surface air temperature and humidity or the fluxes at Tangla PBL site, as they directly affected by soil moisture. Moreover, they have better spatial representativeness than in situ SM.

Response: Thanks a lot for your suggestion. 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. The inter-comparison of surface air temperature and humidity or the fluxes between model simulation and ground observation is our next objective.

Comment 14: "In assimilation test, the simulated desert SM values in Qaidam basin, which locates in the north Qinghai-Tibet Plateau, are ranged 0.1-0.15 m3/m3 and have a better agreement with the distribution of desert area than the result in none assimilated test". Again, there is no observed data to justify this statement.

Response: This has been provided in the response to general comment 2.

Comment 15: In section 5.2, the authors should clarify that they are running WRF instead of the offline Noah LSM. In the text, this has never been clarified explicitly.

Response: Thanks. We have clarified that we are running WRF instead of the offline Noah LSM in the revised manuscript. Sorry for leading so many misapprehends to the reviewers.

Comment 16: "The temporal variations of the estimated SM are in good agreement with ground daily precipitations". Are you going to say "The temporal variations of the estimated SM are in good response to ground daily precipitations".

Response: Yes, thanks for your modification, it has been added to the revised manuscript.

We acknowledge the reviewer's constructive comments and suggestions very much,

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Interactive Discussion

Discussion Paper

which are valuable in improving the quality of our manuscript.

Sincerely yours

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