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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assessment of soil moisture fields from imperfect climate models with uncertain satellite observations" *by* G. Schumann et al.

G. Schumann et al.

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Reply to comments from Anonymous Referee #1 Received and published: 7 April 2009

General Comments The paper addresses the important issue of how to uncertain remotely sensed soil moisture data can be used for evaluating uncertain land surface models. The main idea of the authors is to introduce fuzzy logic, whereby the membership functions are constructed by using two independent remote sensing data sets. This idea is new to me and appears very attractive. However, the paper is very short and does not present the results in sufficient detail in order to allow the reader to assess the value of the method. Many statements are also very speculative. My rec-

ommendation is therefore that the authors significantly expand the paper (in particular, the methodological section and the results section should go much more in depth) and carefully reconsider their conclusions.

Reply: first of all, we want to thank the reviewer for his helpful comments. We agree with the reviewer in that the methodology and results sections need elaborating. We will expand the methodology section in particular. This has also been noted by the other two reviewers.

Specific Comments

1. The model simulations and the remotely sensed data represent different soil layers. How does this affect the results?

Reply: the upper layer of the models is 10 cm and this was compared to the first few cm of the soil from the remote sensing data. Given the other sources of uncertainty and unknowns (e.g. different interactions on the surface for the two radar sensors, differences in model simulation and data inversion from remote sensing), which will also be commented on in the updated version of the paper manuscript, we believe that the impact of this depth difference on the results of our study is relatively minor.

2. Both the regional and global models built upon MOSES. Spatial and temporal patterns of both models should be shown to understand the differences between them.

Reply: we will add a plot to illustrate the seasonal differences between the two climate models (similar to the additional figure uploaded) and comment on this in the text of the new manuscript.

3. Soil moisture values are averaged over one month. Given the different sampling intervals and the high variability of surface soil moisture, should this not introduce major uncertainties?

Reply: averaging soil moisture over one month will inevitably filter out natural variability but the study focuses on seasonal estimations from climate models and we believe that this is a fair test, as current CMs cannot be expected to reproduce the natural variability of soil moisture on a daily or weekly basis.

4. In particular, the ERS coverage of Europe is much poorer than for AMSR-E. How does this affect the results?

Reply: this might be more of an issue when dealing with hydrological models on a shorter temporal scale but is assumed minor for CMs.

5. Both the active and passive measurements represent the same soil layer. So why are two different ranges given for the penetration depth of ERS and AMSR-E?

Reply: there is a very small difference of 1 to 2 cm (when looking at the product description file) but given that they refer most likely to the same layer, we will amend this.

6. The satellite data are affected by snow and frozen soil. Where these erroneous measurements excluded, and if yes, how?

Reply: there are quality flags in the products available for snow/frozen soils but of course there are some remaining pixels that have most likely not been excluded. We will make a note of this in the updated manuscript.

7. Satellite retrievals are characterized by error intervals. Should this error interval not be considered in equation (1), e.g. by making the trapezoid wider?

Reply: this is an interesting point we will definitely consider. Many thanks for the suggestion.

8. What are the P values in equation (2)? I presume it the same as A in equation (1)?

Reply: the reviewer is right. We will change this in the equation.

9. Without seeing much more extensive results, the discussion in Chapter 4.1. is very speculative. In particular, just based on this very coarse scale comparison, are

the authors really in the position to comment on the physical appropriateness of the satellite retrievals?

Reply: we will also expand the result section and so this will hopefully become clearer.

10. In section 4.2. the authors note that the improvement of LAM over GCM is striking. Sorry, but I do not see this in Figure 5.

Reply: the improvement of the LAM over the GCM is high (the word ‘striking’ is indeed too strong and will be removed). We will further support this with a correlation or a histogram comparison plot of the performance of the two models.

11. The authors state at the end of section 4.2. that improved runoff processes are more important than improved evapotranspiration. Which results of this study justifies this statement?

Reply: we agree with the reviewer that this statement is not directly linked to the results of this ‘acceptability’ study and we will remove this sentence from the updated manuscript or formulate it differently at least.

Note: We will soon upload a final document with all the replies from all the reviewers showing the changes made in the new version of our paper manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2733, 2009.

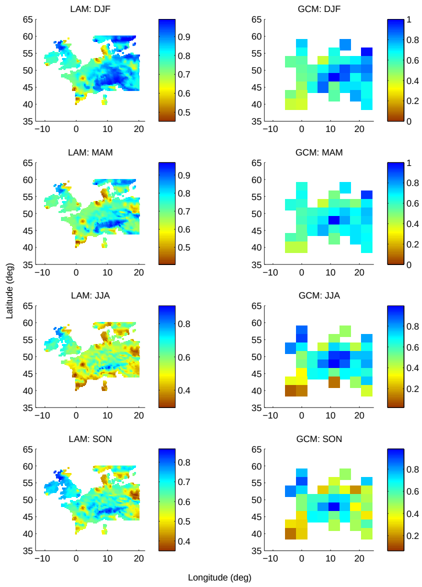

Fig. 1. Temporal differences between the LAM and GCM