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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the comparison between the LISFLOOD modelled and the ERS/SCAT derived soil moisture estimates" *by* G. Laguardia and S. Niemeyer

G. Laguardia and S. Niemeyer

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First of all, I would like to thank the Referee for his careful work and his useful suggestions. I will try to take advantage of his advice for improving the revised version of the article. In the following pages I will try to describe to the reader (and to myself) how I intend to tackle certain issues raised by the referee. For an easier comprehension, the comments of the referee (R2) are also reported.

R2: "GENERAL COMMENTS This paper compares soil moisture from the LISFLOOD model, a continental scale rainfall-runoff model, and from ERS scatterometers, a coarse resolution active microwave instrument. The subject is of great interest for the audience of HESS as its findings influence two areas of active research which cur-

rently receive much attention. First the forecasting of floods on a continental scale and secondly the retrieval of soil moisture from coarse resolution microwave observations."

I checked the text to be sure not to have mentioned floods. To tell the truth, I am using the model for droughts.

R2: "A relative weakness of this paper is its lack of a comprehensive scientific discussion. Differences in the datasets are presented without indepth analyses of the causes. The contribution of the paper to our understanding of the characteristics and limitations of each datasets is therefore limited and no substantial conclusions are presented."

I have to admit that it is true! I have been working on the paper with all the figures embedded within the text and I did not notice that sometimes the text is poor in comments to the figures themselves and to the overall findings. Perhaps I trusted too much the self-explanatory effect of the figures. I'll try to describe the analyses and substantiate the conclusions in a better way.

R2: "SPECIFIC COMMENTS 1) The paper is in most parts well structured and contains all the necessary information. However the introduction would need a major revision. It would be helpful if the authors make a clear statement about the research questions they want to follow up and how they address these questions. This should be followed by a review of state of the art results and methods and a clear statement of how the results of this study add to our knowledge."

The main goal of this work is the evaluation/validation of the LISFLOOD soil moisture estimates with respect to independent measurements, namely the ERS/SCAT derived Soil Water Index. We investigated the agreement of the two datasets and the factors controlling the differences among them. This can be seen as a preliminary exercise towards data assimilation. I'll try to specify more clearly our objectives.

R2: "The introduction should also be limited to the main subject of the paper. Currently the introduction is merely a listing of common knowledge which is not up to date and

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does not contain any substance. "

Certain parts are a bit long, but I believe they are useful. We present the possible methods for estimating soil moisture, their basic principles and main strengths and weaknesses. Provided that the main objective is the validation of the LISFLOOD soil moisture estimates, we presented three possible approaches, examples of their application, and their main strengths and weaknesses.

R2: "A) The authors limit their review of soil moisture remote sensing to approaches relying on optical/near infrared observations. Today it is largely agreed that these methods hold little potential for soil moisture retrieval and research focuses on the microwave domain. Also the respective discussion seems a bit out of focus considering that the authors actually use data from an active microwave instrument. A review of recent developments in this area would be more helpful."

We presented microwave techniques as well. The size of the two blocks of text for thermal and microwave techniques is almost the same. The issue on the potential of thermal techniques has a long story and I don't see much agreement on that. Usually hydrologist prefer that approach: in a modelling framework, it helps with energy budget estimation and leaves the modeller some more freedom on the water budget. However it is not the case to open such a debate in a hydrological journal.

R2: "B) Similarly in the section dealing with field campaigns the authors miss to address the latest developments (OK Mesonet, AMMA, SMOS-REX)."

Thanks a lot for the update.

R2: "C) Finally it seems a bit strange that the authors rely on studies dating back as far as the 80s. It is hard to believe that there was no progress during the last 25 years."

The quotations reported in that part of text are meant to refer to the principles of the techniques; that's why they are rather old.

R2: "2) In section 3 the authors use the LISFLOOD soil data to transform the SWI into

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available soil moisture. This has the effect that both data sets are not longer independent. The effect of this transformation on the estimated statistics has to be discussed in detail. Wouldn't it be more effective to transform the LISFLOOD soil moisture to SWI units. This way both datasets would remain independent."

It is a good idea. It does not make any difference on the results, but it makes unnecessary the pre-processing of the independent data. Anyway, the soil data are an independent input for LISFLOOD.

R2: "3) To study the effect of the scale mismatch semi variograms are calculated. However the results are presented without any critical discussion and/or interpretation. Do the two datasets represent a different scale and which consequences does this have for the comparison. How much of the difference in the two data sets can be explained by the scale mismatch? In the conclusions it would also be interesting if the authors could make a statement about what this means for the application of the scatterometer data for hydrologic applications. Is it a useful dataset considering the coarse resolution?"

I will add some comments on this issue in the revised version of the paper. The main differences between the two datasets are related to the shortest lags, where the scales of the basic information (e.g., soil depth) for the model have an effect. At larger scales, the meteorological forcings dominate the response of the model: a better agreement is observed. The behaviour at the short scales could be obtained by combining coarse resolution satellite data and medium resolution static information within a model (e.g., mapping SWI on a soil depth map with higher resolution).

R2: "4) Confidence intervals should be given for the derived statistics. Currently it is difficult to asses which differences in Fig 9 to 14 are significant."

I don't get the meaning of this statement. Figures 9 to 14 are meant to represent the behaviour of R and RMSE with respect to other static or summary information.

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R2: "5) The conclusions are quite shallow. Apart from the observation that both datasets agree well over large areas, what can we learn for the future improvement of the LISFLOOD model and the scatterometer data? Do the statistics tell us anything about the forecast skill of the model? How can we use the results to improve the model."

I will improve that taking into account the suggestion of the referees.

R2: "TECHNICAL COMMENTS 6) Abstract. In the abstract in what units is the RMSE."

pF units - log10(cm)

R2: "7) Section 2. What is the layer depth of the LISFLOOD model. This is important to understand how the two datasets relate to each other"

The soil depth for the model is a map, with values ranging between 0 and 1500 mm.

R2: "8) Figures: All spatial plots are of poor quality. For example use colour classes instead of the continuous colour bars (this would allow to better distinguish different categories)."

I can try to fix those figures. Moreover, for obtaining a better discrimination of the samples, I produced the combined R-RMSE map (fig.4).

R2: "9) What are the red crosses in figures 9-14"

Outliers.

Regards,

Giovanni Laguardia

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1227, 2008.