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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 also Referee 1 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 (R1) are also reported.

R1: GENERAL COMMENTS This paper presents an interesting piece of research where two coarse scale soil moisture estimation techniques are compared. Both techniques are applied to the entire European continent producing regional moisture estimates that can be useful for meteorological and hydrological applications. The issues

analyzed are of relevance for HESS and the paper is well structured and written. However, there are a number of aspects that need to be improved before the paper can be accepted for publication. My main criticism is related to the fact that none of the methods (ERS/SCAT or LISFLOOD) can be considered as a reference to evaluate the other. This complicates the interpretation of results because of the impossibility to know whether observed errors are caused by one of the methods or the other. As a result, substantial conclusions cannot be reached here because the interpretation of results is not sufficiently solid. R1: I know that this is something unlikely to be modified in the manuscript. I still think that the comparison between both methods is interesting and the paper could be published. But I think that the authors should recognize this inherent weakness of the study and interpret the results with caution mentioning these limitations. Consequently, the following sentences, and any other similar comments, should be modified: R1: - Abstract, first sentence: In order to evaluate the reliability of the soil moisture product obtained by means of the LISFLOOD hydrological model, we compare it to soil moisture estimates derived from ERS scatterometer data.; It is not possible to evaluate the reliability of LISFLOOD if ERS/SCAT is also affected by errors. R1: - Introduction, last paragraph (page 1232, lines 11-15): In this work we present the results of a validation exercise of the LISFLOOD modelled...; Although interesting, this study is not a validation exercise.

I agree on that. We started the work with validation aims, then we figured out that such exercise can not referred to as validation, hence we turned to "comparison". Anyway, it was a good exercise for experiencing the features of the datasets and it can be considered as a first step towards data assimilation.

R1: In my opinion, results should be given in soil moisture units, preferably cm3/cm3, to allow an easier interpretation of results by the readers. It is not so easy to deduce whether a 0.5 rmse in pF units is low or high in terms of soil moisture. I think that the whole analysis should be made in moisture units. This is also important for comparison of your results with those presented in the literature.

HESSD

5, S811–S817, 2008

Interactive Comment

We turned to pF for escaping from problems related to soil texture: a certain water content can represent wilting conditions (WL) for certain soils, field capacity (FC) for others. With pF you have the same value for FC and WL for all the soils.

R1: Another important issue is related to the timing of moisture estimates. What is the time step of LISFLOOD? ERS/SCAT provide an instantaneous observation, is it compared to modelled moisture in that precise moment?

LISFLOOD estimates are reported at the daily time step. For numerical stability purposes, several substeps are carried out for each timestep. ERS/SCAT SWI is somehow modelled at the daily scale as well, but reported only for a limited number of days (at least in the version we used).

R1: Two accuracy measures are used for the comparison: rmse and R. Both represent different things, but their meaning is not taken into account in the discussion. Which one is more important? In my opinion R is a very weak measure of accuracy and results should be mainly evaluated taking into account the rmse. In fact, your modelled soil moisture is likely to have a very high R with rainfall patterns, vegetation LAI or other characteristics but these are not real estimates of moisture. This is important in the definition of error classes for Figure 4. Is class 2 better than class 3??

R is weak if used alone. It does not know whether any systematic error occurs. On the other hand, RMSE could have problems in detecting time shifts among two curves if they have a low overall variability. That's why we used both of them. For our purposes it is better to have some systematic error, which you could easily cancel out by means of a quantile-quantile comparison, than a significant phase shift.

R1: SPECIFIC COMMENTS Introduction: R1: -The importance of soil moisture for agriculture (page 1229, line 3) is much more than a mere element for the determination of irrigation practices

I know. Irrigation practice has a very limited extent all over the world with respect to

HESSD

5, S811–S817, 2008

rainfed practices. I'll rephrase.

R1: -Page 1229, line 15. There are some extensive ground moisture databases that need to be mentioned here (for instance SMEX experiments, REMEDHUS network and some others),

I'll add. Thanks for the hint.

R1: -Page 1229, line 17. Quite a few studies on temporal and spatial stability of soil moisture measurements have been published and could be commented here.

Ok.

R1: -Page 1229, line 27. Microwave observations are classified here as direct techniques for moisture estimation. But they are not exactly direct moisture observations. You should define what you consider direct and what indirect.

There "direct" and "indirect" are referred to the remote sensing community dictionary. I'll try to explain it better.

R1: -Page 1230, line 1. The penetration depth is also related to the moisture content of the soil.

I'll add that.

R1: -Page 1230, line 4. Give references (for instance, Verhoest et al. Sensors 2008, 8, 4213-4248; DOI: 10.3390/s8074213) or further information related to the influence of vegetation and roughness on the estimation of moisture from active microwave observations.

Ok.

R1: -Page 1230, line 7. The temporal frequency of high resolution SAR sensors is improved in RADARSAT, ENVISAT with the different incidence angle capabilities. R1: -Page 1230, line 8. What about SMOS?

HESSD

5, S811–S817, 2008

I'll verify and add/correct.

R1: -Page 1230, line 16. Give a more recent reference than that of Price, 1980.

That quotation was meant to refer to phisical principles. More recent applications are quoted in the following lines.

R1: -Page 1230, line 24 to 1231 line 9. This is very weak. More information should be added, mentioning most used models, land surface parameterizations, sources of errors and uncertainty, etc. This is especially relevant for your paper because you are applying a model to very different regions in terms of climate, topography, vegetation... It is not so easy to design a model applicable to so different conditions.

That is true. I will add it in the model description.

R1: -Page 1232, line 10. Some more information on Data Assimilation should be included.

Ok. It goes a bit beyond the scope of the paper.

R1: 2. Data: Section 2.1. ERS/SCAT: -The deficiencies of the technique are very weakly commented: what about the influence of vegetation? roughness variations? How are snow covered areas identified in the observations before they can be masked out? R1: -The literature review should be improved, what are the results of previous studies like? What are the main difference with your study?

I'll check and eventually add some more references for those issues.

R1: Section 2.2. LISFLOOD: -Is the model calibrated and validated? further information should be given in this aspect.

The model is not calibrated with respect to soil moisture processes. From my experience with models calibrated for getting discharge values at gauging stations, I would suggest not to do that if you are interested in surface parameters because you will obtain worse results. The modelled soil moisture and its linked variables (evaporation

HESSD

5, S811–S817, 2008

Interactive Comment

and transpiration rates) have been checked with respect to expert knowledge on the climatology of the study area.

R1: -The uncertainties associated to the estimation of parameters from soil and land cover spatial databases should be mentioned. How accurate are those databases? what errors can be expected in the simulations if you use them? R1: -A critical evaluation of the model and its performance is missing.

I'll add some comments on that.

R1: 3. Methods -As mentioned before is not clear to me why you made your analysis in soil suction units. R1: -Page 1237, line 10. Both methods require soil parameters. What is the uncertainty in the estimation of those parameters, and what errors can be expected in the moisture estimates.

Already answered. As far as you use pF, the problems should be canceled out. With respect to soil parameters, we have used independent maps. I'll try to add a proper reference for the uncertainty of such parameters.

R1: -The equations of rmse and R can be deleted since they are commonly known. R1: 4. Results -It is not clear to me why average errors in the temporal and spatial domain are different. If you calculate the mean error for each pixel and then average out all the pixels you should obtain the same as if you calculate the mean error of each time step and then average out the whole research period.

It is a matter related to the root.

R1: -Page 1239, line 17. You chose a very homogeneous area to calculate the semivariogram. What about the scaling properties in more heterogeneous areas?

The area is not that homogeneous: there are mountains and flat, dry and wet, cold and warm areas. Moreover, it was the only chance to have such a big square on a not fragmented domain in my map.

HESSD

5, S811–S817, 2008

R1: -Page 1240, line 2 - line 9. The discussion of results here is very weak. Explain the causes of the different seasonal behaviour plotted in figs. 7 and 8 and discuss the differences with the paper of Ceballos et al.

Ok. The ERS/SCAT early spring behaviour is dominated by snowmelt. In other seasons the variability is lower, as expected by a lower resolution dataset.

R1: -Page 1241, lines 11-15. This limitation of the ERS/SCAT approach should be commented in section 2.1. What about the behaviour of the model in those extreme climatic conditions? is the model equally reliable in Central European areas and in Southern European Mediterranean areas?

Its climatological behaviour seems to be reliable. I'll try to document that in the revised version.

R1: Figures: -In my opinion the quality of figures is good in general. -Maybe it is not necessary to include all figures from Fig 9 to Fig 14. It could be better to summarize this information in one only figure or table. In case those figures are kept the units in the X axis should be indicated.

Figs 9-14 are meant to further substantiate table nr. 2. I'll try to find a better solution

R1: -Fig 16 is not very clear. Another combination of patterns and colors should be used to provide a clearer interpretation.

Too much info for one figure, but not much chances to modify that.

Best regards, Giovanni Laguardia

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