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Hydrology and Earth System Sciences Discussions

Interactive comment on "Evapotranspiration modelling at large scale using near-real time MSG SEVIRI derived data" *by* N. Ghilain et al.

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The authors tank the anonymous Referee #1 for his/her comments that are very helpful to improve the quality of the manuscript and the understanding of the presented work.

The reply is structured in the same order than the given comments. The reviewer comments are first reported and are followed by the authors' reply.

Thank you very much for a very good manuscript. The article was clearly written on a topic of great importance. While reading i found somethings to be requiring more attention:

On page 7082 (25) you state that you do not use LST as input. however in the method-

ology section Tsk is still used. How this skin temperature is estimated is stated in the article and not clear to me. In addition is Tsk is estimated from a local energy balance, it should be possible use a a two-source energy balance approach. Have you looked into this? Similarly it is not clear to me how you estimate the emissivity. I know that there is a internal LandSAF emissivity product, but is it used in this study?

Response p. 7082 (25): we detail here the answer to the first question, following the 3 points raised.

1. Effectively, the current implementation doesn't use remote sensing derived LST because we aim at producing all-weather ET and as you know, SRS-LST is restricted to cloud free conditions. Instead, the skin temperature needed by the model is computed iteratively from equations (1), (2) and (9), accounting for relations (3) to (8) and (10). The system of equations to be solved can be seen as a system of 4 non-linear equations with 4 unknowns, i.e. u*, LE, H and Tsk. This is the way it is done in most SVAT schemes. This is clarified in next version of the manuscript.

2. The presented model is actually a generalization of a two-source model. As explained in p.7087 (22), ECOCLIMAP provides a decomposition of ecosystem into homogeneous vegetation types with fractions ξi. Also, it provides the monthly coverage fraction of vegetation (fvegi). The energy balance is performed on each of the 3 possible vegetated fractions (i.e. ζi = fvegi ξi) plus on the bare soil fraction of the pixel (i.e. ζi = 1-$\sum(fvegi\xi$i))). So, in that form, the model acts as a generalized two-source model in separating the contribution from vegetated parts and parts of soil exposed to direct radiation. This is clarified in the new version of the manuscript.

3. The emissivity used in the presented model is the monthly emissivity provided by the ECOCLIMAP database (ranging from 0.96 for bare soil to 0.99 for fully vegetated surfaces). The information is now added in the section 2.2 of the manuscript, where we detail the input parameters. LandSAF compute a broadband daily emissivity (Trigo et al., 2008, IEEE Transactions on Geoscience and Remote Sensing, 2, p. 307-315).

But it has not been used in current model implementation because 1) emissivity maps, at least till 2007, were not produced beyond 50° North latitude nor under cloudy sky conditions and 2) emissivity is not a very sensitive variable in the model. Nevertheless, the use of LandSAF emissivity is envisaged in future developments since some studies indicate that the use of satellite derived EM can improve model performance, particularly in semi-arid environments (Jin and Liang, 2006, J Climate, 19, p. 2867-2881). As emissivity is closely linked to vegetation evolution, it will be coherent to estimate emissivity at tile level along with vegetation indices provided by ECOCLIMAP,

On page 7089 (15) you state correctly that the energy balance closure can leaf to 20% uncertainty in your E estimation. However after that you imply that using EC techniques solves this problem. This is not true, as EC techniques also suffer from EB closure problems. Please refraise and elaborate.

Response p. 7089 (15): The EBC problem doesn't affect, at least in a direct way, the model estimation (because EBC is imposed in the model), but is well known to be a challenge in EC measurements of surface fluxes. The point here was to highlight the EBC problem with eddy covariance, while acknowledging the relative homogeneity of the measurements procedures through the FLUXNET network. The EBC problem is kept in mind when comparing the model results to the EC measurements. That's one of the reasons we have introduced the PRD metric/criterion, to evaluate model performances taking into account not only EBC issues but also uncertainty in model parameters and input variables. Since the paragraph about EBC in the manuscript seems a bit confusing, we put the ideas exposed here into other words.

On page 7090 (13) you state the indices that you are going to you for your intercomparison I would prefer to have this in the methodology section. In addition you state that you use the Nash index. Although it is listed in the tables, in the main body of the article no further reference is made to it. In addition the formula of the index is not commonly known and therefore should be put in the article (if used at all).

Response p. 7090 (13): We add the definition of the Nash-Sutcliff index, and we refer to Alberghel et al, 2010 (same special issue) for a short explanation of how to interpret it. This index is more and more often used in those kinds of study. As well, we introduced the discussion of the Nash index in the validation section.

On page 7094 (2) you start to explain the observed differences between the three models for a three variable sets. However the explanation on how these sets are chosen is not shown. A sensitivity analysis should be implemented here. Furthermore you state that the errors in ET are caused by the sensitivity in the Radiation. THis is not true: a high sensitivity to a specific variable set does not necessarily cause the error in the ET, unless the variable itself contains large uncertainties/errors. Finally it is obvious that the algorithm produces larger differences for arid conditions, however the impact of soil moisture is only discussed in the discussion and not in seciton 5.

Response p. 7094 (2): We divide this reply in three parts; accordingly to the structure of your comment.

1. To understand differences between the models, three possible sources of differences can be analyzed: A) model formulation, B) model parameters and C) input variables. While we recognize that the presentation of a general sensitivity analysis of the model would really be meaningful, we are of the opinion that its implementation is out of the scope of the present paper and should be reserved to a specific article with enough space specifically dedicated to this subject. The three proposed variable sets are selected on the basis of our knowledge of sensible parameters of the model. We comment here on how the variable sets are chosen The 3 models are based roughly on the same formulation, with some differences in model parameterizations. One of the most sensitive parameters is the minimum stomatal resistance (Rsmin) that determines the maximum transpiration rate a vegetation type can bear. In the same way, information from vegetation database influences the calculation of the surface fluxes. Land cover and vegetation characteristics strongly impact on the surface fluxes estimated by these models, given that the partitioning of net radiation is function of cover type, state

of vegetation and local atmospheric conditions (under similar atmospheric conditions, partitioning of net radiation is different over forests than over grass or crops). Leaf Area Index (LAI) is a crucial variable because it scales the Rsmin parameter at model spatial resolution. As the ratio Rsmin/LAI is explicitly used in the 3 models, we choose it as one of the potential sources of difference. Land cover is also examined, but only discriminating the fraction of high and low vegetation. Focusing on used meteorological fields, surface downwards short-wave radiation is the most sensitive input of the models, because 1) it drives the global behavior of ET (at least, in temperate regions), 2) it is difficult to evaluate very accurately in numerical weather forecasts models in cloudy periods, and 3) this variable is different for each one of the models (weather forecasts for ECMWF and GLDAS and LSASAF DSSF for MET model). Air temperature and/or air humidity could also be used, however, weather forecasts on the short range (12-24h) are quite accurate, and provide similar results even from different operational models. On the other hand, since the MET model uses ECMWF forecasts for air temperature and air humidity, the exercise wouldn't provide much information about differences. The choice is also in line with the findings of Kato et al, 2007 (Kato et al, 2007: Sensitivity of Land Surface Simulations to Model Physics, Land Characteristics, and Forcings, at Four CEOP Sites, J. of the Meteorological Society of Japan, Vol 85A, pp. 187-204), since surface radiation, land cover and model formulations (parameters) have been pointed out to be amongst most sensitive land surface model features for evapotranspiration output.

2. As stated in the manuscript, the intercomparison doesn't intend to provide an explanation of the ET errors. Instead, we compare the different ET estimates and try to explain the discrepancies. As mentioned in the previous point, downward surface short-wave radiation (SâĘŞ) is one of the main input variables to which the ET is most sensitive and it is different among the 3 models. It means that if we find a high correlation between SâĘŞ discrepancies and ET discrepancies, it may indeed imply that the ET discrepancies are due to differences in SâĘŞ input.

3. In section 5 (p. 7093 (26-29)), we actually tell about the differences in (semi-) arid regions. Of course, an obvious reason for that would be the difference in soil moisture between models, especially between GLDAS and MET. Moreover, when the water availability for ET is low, in the models formulation, differences in the sensitive ratio Rsmin/LAI tends to amplify the differences observed in ET. This mechanism could explain the difference between ECMWF and MET that use the same soil moisture. We add in the new version of the manuscript that soil moisture difference could also be a driver of ET differences between GLDAS and MET for arid regions. In Section 5 we give insight into the intercomparison between the three models. We prefer keeping in section 6, based on the in-situ validation, the presentation of the potential limitation of using ECMWF soil moisture as input into our model, opening the way to further research that should become the subject of a dedicated paper.

on page 7094 line 25 you suddenly use \Delta%DSSF instead of the \Delta%S\arrow, please change this.

Response p. 7094 (25): the notations have been corrected and are now uniform through the text and figures.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7079, 2010.