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Hydrology and Earth System Sciences Discussions

# *Interactive comment on* "Global land-surface evaporation estimated from satellite-based observations" *by* D. G. Miralles et al.

**D. G. Miralles et al.**

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We would like to thank the referee for his objective and thorough review of the paper. All the referee's comments are addressed in the following response. The manuscript is being revised to accommodate these changes.

Referee#1

P8480.L5. This sentence seems confusing. The net radiation does not convey the same information that the surface conductances. For instance, net radiation is also a main driver in Penman-Monteith formulations, which also require surface conductances. Also, it is true that surface conductances cannot be directly estimated from



space, but the same applies to the ET. In the same way that we model ET and drive the model with remote-sensing observations, surface conductances are modelled and then driven by e.g. vegetation indexes derived from satellite observations (e.g. Zhang et al. (2010), A continuous satelliteâËŸARËĞ derived global record of land surface evapotranspiration from 1983 to 2006, Water Resour. Res., 46, W09522, doi:10.1029/2009WR008800).

Reply

We acknowledge that this sentence needs to be replaced. The reviewer is right: we estimate the stress conditions for evaporation through stress factor (S) which, like the surface conductance, cannot be observed from satellites. The rationale of GLEAM is to combine the existing satellite-observable variables within the simplest physical approach possible. In this sense, the minimalistic PT equation is an appropriate method to estimate evaporation based on satellite observations as it requires just a small number of inputs. We will modify the text to reflect this rationale.

Referee#1

P8481.L16. The models description is confusing. For instance, some land surface models (off-line and coupled) also use Penman-Monteith formulations. Also, some empirical models use other statistical tools to derive objective non-parametric identification functions, not just machine learning algorithms. The authors are referred to e.g. Kalma et al., Estimating land surface evaporation: a review of methods using remotely sensing surface temperature data, Surv. Geophys., pp. doi:10.1007/s10,712–008–9037–z, or Jimenez et al. (2010) Global inter-comparison of 12 land surface heat flux estimates", JGR, 2010JD014545, in press, for suggestions.

Reply

Some of the remote sensing-based models are also offline so a classification of these models is problematic. Nevertheless, the classification in the manuscript will be changed to be closer to the one in Jimenez et al. (2010). However we consider the MTE product an in situ observation-based product. We therefore intend to use this classification: 1) based on off line models (e.g. GSWP - Dirmeyer et al., 2006), 2) based on remote sensing observations (Fisher et al., 2008; Jimenez et al., 2009), 3) based on reanalyses (e.g. ERA-Interim - Simmons et al., 2006), and 4) based on the up-scaling of in situ observations (e.g. MTE - Jung et al., 2009).

Referee#1

P8481.L28. Better just LandFlux.

Reply

It has been changed.

Referee#1

P8482.L7. See previous comments for the abstract.

Reply

See reply to the first comment.

Referee#1

P8483.L15. Adding perhaps "most ABSOLUTE uncertainty". It is true in absolute values, as significant interception loss typically occurs in areas with large ET. But in relative terms (i.e., uncertainty over estimated values) there are other biomes (where interception loss should not be very large) also presenting comparable relative uncertainty (judged as inter-product spread).

Reply

We are not sure this needs to be rephrased. The reviewer is right in the sense that what we try to say is not only that interception loss is one of the most uncertain terms, but also that it is responsible for a large fraction of the uncertainty of E estimates. We



also understand that in non-forested regions there are other factors more important to explain the uncertainty of E. As the reviewer pointed out, in absolute terms accurate estimates of interception are important in forests presenting large E (humid tropics). However, we disagree with the reviewer in the sense that the contribution of I to the final uncertainty in E can be also very important in relative terms, especially in areas of low net radiation (like high-latitude forests). This is due to the lower dependency of interception loss than transpiration on the available energy. Summarizing, we would like to rephrase the sentence as: "The neglect of evaporation from wet forest canopies, referred to as rainfall interception loss, has been thought to be one of the main factors contributing to the uncertainty of global evaporation estimates (see Jimenez et al., 2010)."

Referee#1

P8486.L20. This means that the model needs to be run without assimilation for at least a year. It may help the reader to say this more explicitly. The normalization is done for each grid cell independently?

Reply

Indeed, every year needs to be run in three steps: 1) first a complete run without data assimilation of satellite soil moisture, to gather the daily maps of modelled surface soil moisture driven only by the precipitation (and previous day evaporation), 2) the scaling of the daily time series of satellite observations to the daily time series of modelled surface soil moisture (obtained through the previous step) at every pixel independently, 3) a complete run, this time with data assimilation of the scaled microwave soil moisture. We will change the text to mention this explicitly.

Referee#1

P8487.L20. Have other different correlations cut-off values been tested?

Reply

The value of 20% was selected to avoid assimilating the unreliable microwave observations at coastal pixels. Lower values were tried but they appear too strict as they also cut of pixels with small lakes. It really does not have much of an effect. The criterion of no assimilation of pixels that show negative correlation in the time series between modelled soil moisture (without assimilation) and observed soil moisture, is only important in a few pixels in the world in which the seasonality is very low or not well represented by the products of precipitation or soil moisture. There is still one more selection criterion that was not mentioned here and we have now added to the manuscript. Pixels with a fraction of tall canopy larger than 70% are not included in the assimilation either. This is however somehow unnecessary as, given that the Kalman Filter is driven by the errors of the observations and these are fully based on the optical depth, in pixels with very high optical depth all the weigh would be put in the modelled estimate of soil moisture and no weight on the microwave observation. We will give more detail about these requirements in the text.

Referee#1

P8488.L23. The reader may wonder about the validity of these parameterizations at the global scale, as they seem to have been derived from a case-study in a very specific region in the world. Reply These parameterizations reflect the different physical and biological processes at work. Deep rooted dense vegetation extracts water from the whole root zone before significant stress occurs; in contrast in bare soil or shallow rooted vegetation the water transport is by physical diffusion. Hence the markedly different curves. We will change the text to direct the reader to other work such as Owe and van de Griend (Owe, M. and van de Griend, A.A., 1990. Daily surface moisture model for large area semi-arid land application with limited climate data, J. Hydrol. 121, pp. 119–132) which give further examples of this phenomenon.

Referee#1

P8489.L11. It may help the reader to define the Bowen ratio.



Reply

We will include the definition in brackets.

Referee#1

P8490.L1. Any reasons/references to choose the given G/Rn ratios?

Reply

These numbers were taken from a review of the literature as a whole. Some references from field studies regarding the fraction G/Rn at different vegetation types are: Kustas, W. P., and Daughtry, C. S. T. (1990), Estimation of the soil heat flux/net radiation ratio from spectral data, Agric. For. Meteorol. 49:205-233. Santanello, J. A., and Friedl, M. A. (2003), Diurnal Covariation in Soil Heat Flux and Net Radiation. J. Appl. Meteor., 42, 851–862. Ogee, J., E. Lamaud, Y. Brunet, P. Berbigier, and J. M. Bonnefond (2001), A long-term study of soil heat flux under a forest canopy, Agric. For. Meteor., 106, 173–186. Some references will be included on the paper.

Referee#1

P8490.L20. It will help the reader to have a reference supporting this statement, as some ET methodologies do not explicitly model interception loss and use PT equations to estimate the overall ET over wet canopy.

Reply

We agree with the reviewer. These two references will be cited in the manuscript: Shuttleworth, W. J., and Calder, I. R. (1979), Has the Priestley-Taylor equation any relevance to forest evaporation?, J. Appl. Meteorol., 18, 639–646. Stewart, J. B. (1977), Evaporation from the wet canopy of a pine forest, Water Resources Res., 13, 915-921.

Referee#1

P8492.L26. Even with the care put by the authors to scale GPCP estimates to

CMORPH, a discontinuity can be seen in the global map in Figure 5 at 60N over Asia, suggesting very different precipitation regimes from both datasets. Also, later on the authors state as ultimate goal to derive 1983-present estimates, which can obviously not be done with the CMORPH forcing. It may be good to comment on these things, and plans to attack this issue, as the present rest of the model forcings (apart from perhaps also the snow depth) seem adequate for the long term goal.

Reply

There are two important points here so let us address them separately: 1) The reviewer is absolutely right about the discontinuity at 60N. We have been working on this and decided to merge the two products in a different way. CMORPH presents two practical disadvantages for its application in GLEAM: a) its spatial domain (60°N-60°S) does not cover the entire globe, and b) the product tends to underestimate at high latitudes especially because it misses snowfall. Acknowledging these two, GPCP1DD is used to gap-fill CMORPH outside its spatial domain AND it is also used in snow-covered pixels. This corrects the discontinuity mentioned by the reviewer and it simplifies the approach by eliminating any sort of scaling of GPCP1DD to CMORPH, given that this scaling was previously required because of the low values of CMORPH in snow-cover pixels. We admit that the easiest solution would be not to use CMORPH at all, however its application seems to add quite some skill in terms of resolution and we can observed in the global maps that it captures some orographic features that are lost with the application of the lower resolution GPCP precipitation. The text has been changed to include comment as above. 2) Most of the satellite products selected as an input for the methodology present a consistent record of daily global observations for the period 1984-2007. To our knowledge - and as the reviewer pointed out - there are two inputs in which this does not happen. These are the snow water equivalents and the precipitation. The European Space Agency initiative named GLOBSNOW is aiming to produce water equivalents at daily time scale for the northern hemisphere spanning the whole period (1984-2007). For the southern hemisphere, usually only some pixels



around the Andes and Pampa regions present non-permanent snow cover (permanent snow-covered areas are masked out using the IGPB mask – this will be mentioned in the text). For those few pixels in the southern hemisphere the monthly record of snow water equivalents from NSIDC will then be used (at daily time scale). This seems to be the most suitable way to create a long-term consistent input of snow water equivalents to run GLEAM with. The precipitation is for sure the biggest bottleneck for the long-term application of GLEAM. There are global monthly datasets of precipitation spanning the 24 year period, however, the daily nature of the Gash analytical model requires daily input of precipitation. GLEAM can therefore only be run at a daily (and not monthly) timescale. While this good time resolution is an key feature of the methodology, it is also a disadvantage when it comes to collecting input datasets from the 80's. There are three rainfall products that have alternatively been used in GLEAM: CMORPH (satellite-based, 0.07 degrees resolution, it covers 2003-2007), GPCP1DD (satellite and gauge, 1 degree resolution, it covers 1997-2007), CPC-Unified (gauge-based, 0.5 degree resolution, spanning 1984-2005). The choice of the precipitation product used as an input in GLEAM, depends on the purpose of the study. If what we need is the best possible estimate of global evaporation for the period 2003-2007, the ideal would be to use CMORPH (gap-filled with GPCP1DD as explained above). However, if what we want is to analyse long-term trends in evaporation, we would need to keep all the input datasets consistent; in that sense it would make more sense to apply GLEAM using CPC-Unified only. The text will mention the modification to the inputs that are planned to develop this long-term dataset.

Referee#1

P8493.L18. As the LPRM product is used in a global context, it may help the reader to mention briefly the investigated regions for the claimed soil moisture accuracy (semi-arid?). We assume that for regions with large vegetation optical depth the same accuracy in the SM product cannot be obtained, which may have an impact in the SM assimilation.

Reply

The uncertainty of the soil moisture is actually dependent on the density of vegetation (as we mention in the section dealing with the data assimilation of soil moisture). Therefore, as the reviewer pointed out, the mentioned estimate of the uncertainty of the satellite soil moisture (0.06m3/m3) corresponds to a specific level of vegetation density (semi-arid regions). However that value is also the global average uncertainty of the product found by de Jeu et al., 2008 (de Jeu, R. A. M., Wagner, W., Holmes, T. R. H., Dolman, A. J., van de Giesen, N. C., and Friesen, J.: Global soil moisture patterns observed by space borne microwave radiometers and scatterometers, Surv. Geophys., 29, 399–420, 2008) over different ecosystems.

Referee#1

P8493.L20. Any reasons to assign the 60% ratio of herbaceous/tall-canopy ratio?

Reply

The ratio of 60% is somewhat arbitrary and it has not been calibrated. It is based on the observed difference between values over entirely forested pixels and pixels falling in areas where the land use is only herbaceous vegetation. The text will be changed to include comment as above.

Referee#1

P8994.L1. As the LPRM started to be presented by citing frequencies, it may be better to give again a frequency rather than the Ka band name.

Reply

Change will be made.

Referee#1

P8994. L1. In principle the PT equation is governed by the air temperature (Ta) not



by the surface temperature (Ts). In fact, the LPRM Ts product is gap-filled with ISCCP Ta, and not the ISCCP-Ts. It could be argued that for daily averages the Ts is a good proxy for Ta, although that may depend on regions and time of the years. The choice of Ts and not Ta, and the gap-filling with Ta and not Ts, needs to be explained/justified.

Reply

On a close examination of the Priestley and Taylor original paper we conclude that delta should be evaluated at what PT called the ‘appropriate temperature’. This temperature is the mean between the air temperature (at the reference level, where also net radiation is measured) and the surface temperature at saturation. Furthermore, delta appears on the top and bottom of the equation the evaporation is rather insensitive to air temperature. For example at 15 deg C an error of 1 deg C in temperature changes the estimated evaporation by 2%. The validated relationship we use now to interpret Ka-band gives us the Land Surface Temperature (LST) or skin temperature (which refers to a maximum of 1 mm depth). For vegetated areas this will be the temperature of the top of the canopy and should be very close to the ‘appropriate temperature’. Only for bare surfaces it refers to the surface layer of the soil, and not vegetation, in which case we could be overestimating in some cases. Another reason we decided to use this Ka-band product instead of the ISCCP air temperature, is because of the much higher resolution (0.25 for LPRM versus 2.5 degrees for ISCCP). This point will be better explained in the text.

Referee#1

P8495.L16. The symbol for porosity has already been used for background error variance in the Kalman filter, it may confuse the reader.

Reply

This will be corrected. We also recently discover the existence of wilting point and field capacity maps from IGBP, generated using FAO units. The inclusion of these maps would be beneficial in the sense that it would avoid any use of porosity in GLEAM. We are exploring this possibility. This is the reference of the soil properties dataset from IGBP: Global Soil Data Task Group. 2000. Global Gridded Surfaces of Selected Soil Characteristics (IGBP-DIS). [Global Gridded Surfaces of Selected Soil Characteristics (International Geosphere-Biosphere Programme - Data and Information System)]. Data set. Available on-line [http://www.daac.ornl.gov] from Oak Ridge National Laboratory Distributed Active Archive Center, Oak Ridge, Tennessee, U.S.A. doi:10.3334/ORNLDAAC/569.

Referee#1

P8495.L18. Perhaps “Derivation” instead of “definition”? It may help to mention soil moisture rather than just the symbol.

Reply

Agreed; we will substitute the word.

Referee#1

P8495.L10. Although details are given in the related table, it may help to give a reference for SCAN here and to point out that stations are only located in US.

Reply

The text has been changed.

Referee#1

P8496.L1. For completeness, the number 0.57 to 0.60 may also be given in the text. Is interesting to see that GLEAM estimates soil moisture better in the second soil layer (r=0.69) than in the first one (r=0.60), even if the assimilation has a larger impact for the first layer. Any thoughts on this?

Reply



Those numbers have been included in the text. The reason why the correlations are slightly higher for the deeper layer is just that the soil moisture in this layer is less variable and smoother over the year. This makes the soil moisture in this layer follow the climatological expectation a little closer than for the top layer of soil (which is more dynamic as it receives more water and therefore it evaporates more as well). This lower daily variability of deeper layers make them little easier to model accurately.

Referee#1

P8496.L15. As stated in the reference given, the main aims of FLUXNET seem broader than just the carbon fluxes. Adding “water vapor and energy fluxes” may present better the network.

Reply

Agreed. The will be changed to: ‘quantifying carbon, water vapor and energy fluxes’.

Referee#1

P8496.L27. 50% miss-match in energy closure seems very large, do you have the percentage of stations discarded at hand? Assuming that those numbers have been calculated for all stations, and that they can say something about the quality of the fluxes measured tower, it could have been interesting to add an extra column in Table 3 with the percentage of energy closure.

Reply

Despite the fact that the energy closure is not the only indicator of the quality of the tower observations, it may indeed give a coarse idea of the uncertainty of the in situ observations. A column will be added with the % of miss-match in the energy balance.

Referee#1

P8498.L25. It could have been interesting to see some time series for some of the stations (e.g., one station per group) to have examples of how the seasonal variability is captured in the model estimates.

Reply

A new figure will be added with a time series for each of the four groups.

Referee#1

P8498.L28. It may be of interest to put the correlation figures in the context of other published results, making reference to the values presented in the validation of some of the other methodologies with tower measurements.

Reply

It will be done.

Referee#1

P8500.L3. Jimenez et al. (2010) may be more appropriate.

Reply

Agreed. The reference has been changed.

Referee#1

P8500.L11. Any references to backup this statement? Some researchers argue than is not clear at the moment whether we can estimate better precipitation than ET (even if the precipitation products may seem more mature). Perhaps saying "remains ONE of the biggest" may be less controversial.

Reply

We agree with the reviewer. The text has been changed like he/she suggested.

Referee#1

P8500.L16. While GCM benchmarking if one of the good reasons to provide ET esti-



mates, the characterization of the water and energy cycle from observations for analysis/attribute studies is (on its own) another important objective.

Reply

It will be added.

Referee#1

P8500.L19. A (instead of THE) wide range?

Reply

Changed.

Referee#1

P8500.L23. See previous comments for the abstract. For instance, it could be argued that the proposed herbaceous stress function (driven by a RS estimated optical depth) is not far from a modeled canopy conductance (driven by a RS estimated vegetation index), in the sense of both characterizing observation-driven stress in plant function.

Reply

See first comment.

Referee#1

P8502.L3. Possible references for LandFlux-Eval are Jimenez et al. (2010), and Muller et al., Evaluation of global observations-based evapotranspiration datasets and IPCC AR4 simulations, under review, GRL.

Reply

They have been added.

Referee#1

Figure 3. The figure could be simplified by plotting the 4 lines in one plot, as 2 of the lines are repeated in the 2 individual plots. For consistency, use either “herbaceous” or “short vegetation”.

Reply

We did not think of it, but it is true. The two plots will be combined into one plot. Also, we will use ‘short vegetation’ consistently through the entire manuscript.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 8479, 2010.