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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.

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We would like to thank the referee for his/her comments on our approach. These comments are addressed in the following response and the manuscript will be revised and modified considering them.

Referee#2

The paper makes an implicit assumption, and also expects the reader to infer, that a satellite-based estimate of evaporation is somehow superior or desirable to evaporation estimates obtained by other means. In my opinion, this is wrong. The accuracy of the estimates is intrinsically tied to that of the input data, and satellite data for some of the

key inputs are notoriously poor over land. Precipitation is a prime example. Satellite based precipitation datasets do exist but have monstrous errors over land compared to rain gauge data, at least in the many areas that are gauged.

Response

We agree to a large extent with the reviewer. However we use satellite-based products because these are the only observation-based data sets available that have truly global coverage. It is not our intention to suggest that satellite-based evaporation estimates are superior to model-based estimates or in situ-based approaches. However, we think we are able to show the added value of satellite information to improve the more traditional (FAO-type) model estimates of evaporation.

In regions with dense networks of in situ observations, a global estimate of evaporation based on the up-scaling of those ground observations may have better skill than our fully satellite-based methodology. This is the area where the two approaches are complementary and a future merged product may ultimately prove superior.

We also agree with the reviewer that the non-familiar reader may be unintentionally misled to think that satellite observations have an accuracy that they do not have. We will rewrite the introduction to emphasize that satellite observations are not error-free. In fact, in our assimilation of soil moisture data for instance, we specify these errors explicitly.

Referee#2

From the text on p. 8492, it looks like the authors go out of their way to avoid using rain gauge data, using them only poleward of 60 degrees. Why? Just to say that only remotely sensed data are used? This seems fine for an academic exercise, but not if the goal is to produce the most accurate evaporation rates possible.

Response

We believe the reviewer is also making an incorrect assumption here. We do not use

CMORPH instead of GPCP because it is fully based on satellite data; we use CMORPH because it is overall a better product. GLEAM can work perfectly well with GPCP data. Indeed the first global long-run of GLEAM (from 1984-2005) has actually been done applying interpolated rain gauge data from CPC (the CPC-Unified dataset), due to non-existence of a (better) long record of daily satellite global precipitation (see response to reviewer#1).

We acknowledge that GPCP may be a better product than CMORPH in areas of dense rain gauge observations. However, CMORPH represents a better precipitation product overall (please check CMORPH inter-comparison studies referenced in the manuscript). For instance, CMORPH has 14 times higher resolution, it captures orographic rainfall better and it makes full use of the high quality TRMM observations unlike GPCP (which is still satellite-based to a large extent).

It is not our intention to reduce the performance of our evaporation product by using worse input data just to prove that we can develop evaporation solely from satellites. We are determined to use the best available global inputs. We however believe that in most cases this implies the use of satellite-based inputs due to their observational nature (as opposed to modeled fields) and their ability to provide global spatial estimates (as opposed to in situ measurements). Therefore, despite being designed to combine the existing satellite observations, GLEAM is flexible enough to work with other types of input, and we will use those as long as they are reliable, have global coverage and increase the overall quality of the final product.

Referee#2

Snow water equivalent estimates from space are even worse; at best, sensors can only give accurate estimates of snow cover fraction.

Response

We are aware of the uncertainty in SWE derived from satellites. However, we are not

sure about what better quality global daily estimates of SWE the reviewer is redirecting us to. As we pointed out above, if the reviewer is aware of the existence of different data with a proven overall better quality that the ones we chose for this exercise, we encourage him/her to provide us with that information; we will be pleased to use the data as input for the methodology.

In the mean time, since SWE is only used to distribute the melt water flux in time, and does not influence the cumulative sum of this flux, we feel confident that any errors in SWE are sufficiently constrained in our methodology.

Referee#2

Soil moisture with current (pre-SMOS) sensors does not extend 5 cm into the soil, as the text states, but rather about 5 mm, and these measurements are also subject to great error.

Response

The penetration depth at C-band is a direct function of the soil moisture conditions, resulting in a low penetration during wet/saturated conditions (approximately in the order of a few millimeters) and deeper penetration during dry conditions (approximately in the order of a few centimeters) (Ulaby et al., 1982). To make it not too complicated one often assumes that the penetration depth is about several tenths of a wavelength (Schmugge, 1983). This for C-band (wavelength = 4.3 cm) means one or two centimeters.

Numerous studies have shown that the produced satellite surface soil moisture is strongly related to the 0-5 cm soil layer (e.g. Wagner et al., 2007, De Jeu et al., 2008, Draper et al., 2009, Gruhier et al 2010). Comparisons with modeled data of the first 5 cm also showed good agreement (i.e. Dorigo et al., 2010, Rudiger et al.,2009).

In our approach we normalize the satellite soil moisture data to match the mean and standard deviation of the annual time series of the model estimates. This will bring the satellite soil moisture data in the modeled 0-5 cm range. Such an approach has been conducted successfully in the past by several other studies (i.e. Reichle et al., 2004, 2007; Bisselink et al., 2010) and is a valid approach for soil moisture assimilation.

We will rewrite this part about the penetration depth with some more scientific background in the revised manuscript to guide the readers.

Referee#2

Nowhere does the text point out how poor these inputs are, leaving the uninitiated reader to infer that they are adequate for the task at hand.

Response

Line 18, Page 8493: "The LPRM soil moisture product has been validated in several studies and is estimated to have an average accuracy of $0.06 m3m-3$ (see De Jeu et al., 2008)." Also the section about the data assimilation of soil moisture deals specifically with the uncertainty of soil moisture observations, and the relationship between this uncertainty and the density of vegetation. If additional information is needed, see the response to reviewer#1.

Referee#2

Given the quality of the input data, other approaches for estimating evaporation may be superior and should be evaluated against that described in the paper, if possible. A prime example is the approach used by the Global Soil Wetness Project (GSWP2), in which an array of land surface models is forced globally with (among other things) radiation fields and global fields of precipitation derived from a rain gauge network.

Response

We disagree with the reviewer and we do not believe an inter-product comparison should be included in this paper. This paper deals with the methodology and the validation through in situ data.

We also do not imply that other global products are of worse quality than GLEAM (and we will revise the manuscript to make sure we do not give that impression). We have no reason to believe that GSWP2 is a better product than any of the 10-20 global estimates of evaporation that currently exist. For instance, the precipitation forcing used by the GSWP2 baseline product B0 - the Global Precipitation Climatology Centre (GPCC) precipitation - is known to suffer from several problems (e.g. Decharme and Douville, 2006). We are aware that a considerable effort was made within GSPW2 to test the sensitivity to different precipitation forcings (although not all models took part, precluding an extended multi-model analysis for some of the alternative forcings). By no means we would like to imply here that the choice of precipitation causes GSWP2 to produce unreasonable evaporation estimates, but to highlight that all approaches are exposed to difficulties (such as the uncertainties in the input data).

Interestingly, the GEWEX initiative LandFlux Eval – that compares the existing global evaporation products – has at no point concluded that GSWP2 evaporation is superior to other products. For instance Jimenez et al. (2010) pointed out (and we agree) that at the moment it is difficult to prove (at the global scale) that one product is superior to the others. Model-based approaches show a large variation among each other. These differences between more traditional methods to estimate global evaporation were indeed the starting point for us to decide to derive a more observation-based product.

Referee#2

Unlike the approach outlined in the paper, the GSWP2 approach takes advantage of the concept of energy conservation; in GSWP2, prognostic temperatures are maintained and an energy and water budget is maintained at every time step, an advantage for the evaporation calculation. (Also, with prognostic temperatures, GSWP2 models determine snowmelt based on energy balance considerations; here [p. 8485], snowmelt is apparently partly determined via differencing against poorly estimated snow depths from space.) An obvious test of the approach used in the present paper is to compare somehow the evaporations generated with those produced by GSWP2 or GLDAS.

Response

Again we have no reasons to believe that the approach taken by GSWP2 is not one of the valid alternatives to derive global evaporation. Closing the energy and water budgets is a desirable thing, but not all models participating at GSPW2 achieved that for a variety of reasons, which have implications for the multi-model analysis proposed as best outcome of the exercise (e.g. Dirmeyer et al., 2006). A comparison of GLDAS global evaporation for their three participating land surface models is also displayed in Jimenez et al.(2010), showing again significant differences among them in some regions.

Notice that we do not imply here that more model-based methodologies are wrong (also differences are observed in more satellite-based products in Jimenez et al., 2010), but to highlight the difficulties that all methodologies face at the moment.

As we commented above, this paper only aims to present the methodology of GLEAM and its validation through in situ observations. However, we agree with the reviewer that an inter-product comparison will put the approach in perspective with other existing methodologies. Such a comparison is currently being done by LandFlux.

Referee#2

If I'm reading the paper correctly, much of the skill in the authors' approach stems from capturing either the seasonal cycle (for the site comparisons) or the geographical variations of precipitation and radiation (for the annual comparisons). Assuming the authors can do this, it's not surprising that they get reasonable skill scores – even a much simpler model would show some skill. A truer, more convincing test of the authors' approach would be to demonstrate how well it captures the inter-annual variability of evaporation at a single site. Can the authors comment?

Response

As said before, this paper only aims to present the methodology and validate it for 2005 using FLUXNET data. In this paper, we are very interested in whether we are capturing the magnitude of the fluxes and variations at daily time scale properly. The revised manuscript will include a figure with time series of estimates and observations for some of the stations (as suggested by reviewer#1), to show that it is not only the seasonality what we are capturing.

Once these daily dynamics are validated (in the present paper) and we are confident on the performance of the methodology, we can extend the product to cover a long period. As we mentioned above (and also in the conclusions of the manuscript) the final dataset will cover several years (from 1984-2007). Inter-annual variability will then be tested.

We are confident in the results of the validation we show here, which are of course (and so we recognize it in the manuscript) dependent on the quality of the inputs feeding the methodology. We believe that the reviewer will realize the level of skill of the method once he/she sees the time series figure in the new manuscript - levels of correlation coefficient over 0.9 are not obtained at daily step by only capturing seasonality of evaporation.

Referee#2

Regarding the formulation of the ground heat flux G: does the authors' approach account for the fact that G, averaged over a long time period, is zero? This wasn't clear.

Response

The fraction of Rn that we define as G is based only on the day-time partition of incoming energy. We only consider G as a removal of energy from the system when evaporation takes place. We agree with the reviewer that at annual basis (and nearly-daily basis) the net G will approach zero. However (as the reviewer pointed out) we do not attempt to close the energy balance; we do not consider that the upward G at night time has any effect of the value of daily evaporation.

Referee#2

Summary: Overall, I think the attempt to produce a satellite-based evaporation dataset is laudable; it's a useful and interesting exercise. The biggest problem is that the casual reader would not be aware of the deficiencies of the approach (particularly the low accuracy of many of the satellite-based inputs) or of the fact that other (non-solely remote-sensing) approaches are potentially superior, at least in the areas for which ground data, such as rain gauges, are available.

Response

We will rewrite the parts of the manuscript that may be giving the impression that remote sensing approaches are potentially superior. We acknowledge that the methodology could potentially benefit from the use of in situ data when/where available. The corrected manuscript will give more information about the existing uncertainty of satellite observations so that uninitiated readers are not misled into thinking that satellite observations are error-free.

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