Hydrol. Earth Syst. Sci. Discuss., 10, C3512–C3517, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C3512/2013/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Benchmark products for land evapotranspiration: LandFlux-EVAL multi-dataset synthesis" *by* B. Mueller et al.

B. Mueller et al.

brigitte.mueller@env.ethz.ch

Received and published: 26 July 2013

Reply to short comment by H. Savenije

Thank you very much for your comments on this paper. Hereafter, we include your comments in italic font and our reply in non-italic font.

General comments *The authors of this paper present newly launched synthesis products for evapotranspiration (ET). For the rest of this comment we will simply use the term (terrestrial) evaporation (E) instead, which we perceive as a better term for the change of liquid water to water vapour (e.g. Monteith, 1981; Shuttleworth and Wallace, 1985; Savenije, 2004; Brutsaert, 2005). Anyway, we think that these global benchmark evaporation products could indeed prove useful in many applications and will certainly*

be welcomed by many in the scientific community. Therefore, we highly appreciate this contribution to HESSD. However, we have some concerns we would like to point out to the authors. First of all, the products are not yet made available on the website referred to in the paper: www.iac.ethz.ch/url/LandFlux-EVAL (accessed 08/02/2013), which hopefully will be solved well before the public discussion period is over. It would for example be nice to be able to check the statement about the differences in the Amazon region.

Answer:
The data products have not yet been made available because we wanted to make sure that it is a 'final' product and it is accepted as is by the reviewers. As soon as the paper is accepted, we will provide the product on the mentioned webpage. Currently, people can already contact us for the products but we expect them to wait for possible publication of their analyses until the acceptance of our paper.

That aside, our main concerns in terms of content are 1) consistency in the use of units, both for comparisons within the paper as for comparisons with other global water flux studies, and 2) the lack of a discussion of what would be the best (benchmark synthesis) product for evaporation.

A:
1) Thank you very much for pointing out the inconsistency with units. Upon this comment, we decided to use mm/yr and mm/yr2 for absolute fluxes and trends, respectively, in all tables, figures and the text. We add km3/yr2 in addition to these 'standard' units when the focus is on the total amount of water.
2) We add some discussion of that in the text (see also below).

In the paper, several units are used interchangeably to quantify global water flows: mm per day, mm per year, km3 per year. We feel that the paper would gain readability with a more consistent use of units. In one instance, the unit conversion seems to have gone wrong: the negative trend in evaporation between 1998 and 2005 is reported as 18.9 *km3 yr-2 (Sect. 3.3 and conclusion) and 1.40 mm yr-2 (abstract). These numbers do not correspond with each other assuming a land area of 130,922,000 km2 (see Table 4), whereas in the abstract the obviously wrong value of 130,922 km2 is mentioned). Unfortunately, this leads to some confusion in the discussion related to the true number of the negative trend(s). Also, we feel that it should be explicitly pointed out to which physical area of the Earth this 130,922,000 km2 or 'global' land is referring to, as it seems that Antarctica is excluded, but that is as far as we could see not made explicit in the text. Furthermore in many sentences and tables precipitation and evaporation are given in mm/day, but runoff in km3 per year (3.3), or the trend in evaporation in km3 yr-2 (Table 4). We think that mm yr-1 for all fluxes and mm yr-2 for all trends in fluxes would be most appropriate when comparing fluxes within this study and possibly mentioning km3 yr-1 for comparison with other studies. However, this is up to the authors. The most important thing is that it becomes easier for the reader to readily compare data.*

A:
We agree with the reviewers that the manuscript gains readability with a more consistent use of units. The units mm/year and mm/yr2 are now consistently used throughout the manuscript, also for runoff. The land-area in the abstract is corrected. Table 3 and the corresponding text mentioning the negative trend from 1998-2005 have been corrected as well. Thank you for reading it carefully. In the text, we now mention that Antarctica is missing. For the land-area, we refer to the contour plots in the supplementary material.

Specific comments about unit consistency
773-12: 'Wang et al. (2010b) found an increase in global land ET of 15 mm per year.' Here it should be made clear that not a trend of 15 mm/yr2 is meant (which would be quite a lot), but an increase over 20 years. Or better this number should be converted into a trend in units that are also used in this paper for trends.

A:
We corrected and converted this number into mm/yr2.

780-6 '1.35 mm/day for both...' We are confused here as Fig. 2 clearly shows that the estimate for the short period is lower than that of the long period.

A:
These do not show exactly the same quantity. In Figure 2, the interannual variation of ET is shown, i.e. the median from the datasets at each single year. In the text, we refer to the multi-year mean value, i.e. the median of the multi-year mean. This quantity is shown in Table 4 and Figure 1. We now mentioned this in the caption for Figure 2.

780-6-780-16. Here, the global land evaporation from the benchmark product(s) is compared to other studies. We would also like to take this opportunity to make a general comment, not necessarily confined to this paper. We have noticed that water fluxes are interchangeably reported in L/T or L3/T in different studies. While it seems trivial to convert back and forth, large differences can occur when different land areas are used to make the conversion. In some studies Antarctica, Greenland and the Sahara, all with practically zero evaporation are left out, which does not really make a difference when reporting in L3/T, but which does when reporting in L/T. In this case the land area used for the conversion should be made explicit. Moreover, some studies may include/exclude big lakes, include/exclude other areas, or have other reasons why the conversion is not straightforward. This all makes it hard to compare numbers from different papers. In this case, the authors are perhaps only interested in giving the reader an idea of the range, but a less cautious or time-pressured reader might inaccurately presume a precise and representative comparison, so a warning to the reader would fit here very well.

A:
Different land mask indeed result in large differences in 'global' values. We have added a notice in Section 4.

782-8 '1.5 mm/d' We assume this is evaporation as well? Although the authors explain why this value is higher than the '1.35 mm/day' mentioned before, it does illustrate nicely just how tricky it is to compare numbers from different studies.

A:
We checked the two numbers again carefully. The difference is, as expected, in the land mask. In figure 5, the aim is the comparison of the absolute fluxes between single datasets. Therefore, only pixels that are available in every single dataset can be considered. This means that desert areas (with low ET) are excluded (see Supplementary material Figure B2).

The best product? As we also use global evaporation products (e.g. Van der Ent and Savenije, 2010; Van der Ent et al., 2010; Keys et al., 2012), we are of course interested to know what is the best dataset available for evaporation. However, we are not fully convinced by the processing of the data (Sect. 2.3) and we find that a discussion on which of the four benchmark products (diagnostic, reanalysis, LSMs, or merged) comes closest to the truth, is lacking in the paper. Why is for example each dataset given equal weight? Are LSMs not overrepresented or is this because you trust them the most?
Specific comments about the best product
773-14-773-18 Although (as far as we know) not publicly available, the dataset presented by Jung et al. (2010) could also be considered a benchmark product based on several distinct data sets. Are the new benchmark products better, equally good or is this impossible to say? We feel that the authors should discuss this.

A:
With the absence of direct measurements over the globe, it is not possible to say which dataset is closest to reality. Even measurements have large uncertainties. Since it is not possible to rank the datasets according to their validity, we think that giving equal weight to each of the datasets is the best approach. If the large number of LSMs is a concern, we recommend the users to include the synthesis benchmark product based

on diagnostic datasets in their analysis and compare the results to the product based on all datasets.

777-9-777-11. Sometimes as much as 50 % of the datasets (as can be seen from the movie provided in the article supplement) is excluded in the winter season in Europe. This is done although the authors acknowledge that interception can be larger than radiation (see also Gerrits et al., 2010). A recent review paper (McVicar et al. 2012) also suggests that 'in mid-latitudes (i.e. > 35) in winter, the aerodynamics governs > 80% of the evaporative process', which confirms our own experiences. We feel that the decisions made in data discrimination should be more elaborately motivated and transparently explained. Why are the excluded datasets considered wrong?

A:
We agree that ET can be higher than radiation even for long-term values, especially in winter. We consider ET values which are up to 25% higher than radiation. In winter, a higher exceedance might be possible. A possibility would be to rise this upper constraint of 25% exceedance to a higher value in winter. However, this would mean that the coldest season would have to be determined for every single pixel. The constraint would be difficult to comprehend and reproduce. In most seasons of the year, a maximum exceedance of ET over radiation of 25% (as we used for the synthesis products constraint) is realistic. In addition, ET is relatively small over in winter. With including all values smaller than 0.3mm/d, we accounted for the fact that ET can be larger than radiation in the specific cases where ET is very small.
