Hydrol. Earth Syst. Sci. Discuss., 10, C61–C66, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C61/2013/

Hydrology and Earth System Sciences Discussions

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

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Received and published: 8 February 2013

These comments are submitted on behalf myself and my PhD students Lan Wang and Ruud van der Ent.

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 (776-5 – 776-7). 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.

Consistency in units

In the paper, several units are used interchangeably to quantify global water flows: mm per day, mm per year, km^3 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 km^3 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 km^2 (see Table 4), whereas in the abstract the obviously wrong value of 130,922 km^2 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 km^2 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 km^3 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 km^3 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.

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 yr^{-2} 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.

780-6 “1.35 mm day^{-1} 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.

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 L^3/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 L^3/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.

782-8 “1.5 mm day^{-1}”

We assume this is evaporation as well? Although the authors explain why this value is higher than the “1.35 mm day^{-1}” mentioned before, it does illustrate nicely just how tricky it is to compare numbers from different studies.

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.

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. $> 35^{\circ}$) 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?

Conclusion

The paper contains interesting material that should be published. But because we think that these datasets may indeed become widely used, we hope that the authors will do their best to address our comments in a revised version.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 769, 2013.