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Hydrology and Earth System Sciences Discussions

Interactive comment on "An application of GLEAM to estimating global evaporation" *by* D. G. Miralles et al.

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The manuscript by Miralles et al. presents an application of an interesting methodology to estimate the global distribution of the four major components of evapotranspiration at a daily resolution. The fact that the different components are considered separately sets this study apart from the numerous other recent studies that present gridded interpolated fields of evapotranspiration using earth observation or other data sources. This makes it a potentially interesting paper that could make a significant contribution to the research field. However, I agree with the previous comment by Fischer that the manuscript in its current state is not sufficiently different from the Miralles et al. (2010) paper, and does not present enough new results (especially since it is being presented to the same journal). Upon reading, one gets the feeling that the paper has

been written as an internal progress report rather than with a clear message to the reader in mind. (This can be dealt with during a revision though and should be taken as a motivation). Clear objectives are missing, and some of the most interesting results are listed in tables that are not discussed in the text. Rather than merging the two manuscripts, however, I believe that the current manuscript has sufficient potential to be revised into stand-alone HESS paper if additional analysis are presented and all results are discussed in detail.

General comments

The title suggests that the paper is an application of a method to learn something about a process (i.e., global evaporation), but it remains unclear from the abstract what has been learned exactly. Obviously, the manuscript can make the biggest contribution by focusing more on the different components of evaporation and how they relate, since this is where the manuscript differs most from similar papers. While some analyses have been performed in this direction, many obvious questions remain unanswered. In my opinion, the manuscript would be much stronger if some additional analysis were presented in order to answer the following questions:

- What is the global evaporation for the period 2003–2006 and its possible inter-annual variability? (presented in different units, so volume and depth, and with uncertainty estimates, see comment below, so that the numbers can be easily used by others)
- How does this estimate compare to numbers in other studies? (surely there are more numbers in the literature than the two others mentioned on Page 11)
- How much do certain ecosystem types, continents and processes contribute to the total global evaporation? (presented also as % of E, possibly in a graph or

map)

- What is the dominant contribution to total E in a given region? (i.e., bare soil, transpiration, sublimation, this can be presented in the form of a map)
- What is the average soil moisture stress in each pixel? The authors correctly state that the stress parameterization is of key importance, but without it being presented explicitly there is no way of knowing its regional importance (note that the correlations in Figure 6 only reflect part of this information).
- ...

A general comment related to my previous one deals with the lack of uncertainty estimates for the GLEAM product. Many different gridded ET products exist nowadays, and these products typically differ significantly (Müller et al., 2011). Information on uncertainty is almost indispensable in present-day science in general, and in application and comparison of gridded datasets in particular. Such information can for instance (in the case of GLEAM) be derived from statistics of the data assimilation increments (i.e., the imbalance between the Priestley-Taylor evaporation and the calculated soil water budget), or by creating a GLEAM ensemble with perturbed parameters.

A third general comment concerns the usage of the term "physics-based", also commented on by Fischer, in both the manuscript and the response to Fischer. The authors claim that "physics-based" methods are "data-driven", whereas "model-driven" methods are empirically-based. In my view, it is exactly the opposite. Physics-based methods are based primarily on fundamental laws of physics that don't (or to a very limited degree) depend on parameterization. Data-driven methods are by definition empirical, but they are indispensible in earth system sciences since the world cannot be modeled by fundamental physics alone. Since GLEAM violates the most fundamental physics law for hydrologists, namely the conservation of mass, and uses several empirical relationships, it cannot be called "physics-based". Note that this is not necessarily

a bad thing, since the final product can still be optimal in a statistical sense. My suggestion would be to just describe the method as it is, and avoid any discussion about "physics-based" versus "model-driven".

Specific comments

Page 2, Line 16: "This study gives new insights into the relative importance of precipitation and net radiation in driving evaporation". Can such new insights be obtained by studying signals that are not independent? The fact that precipitation, soil moisture, as well as radiation are at the hart of the GLEAM model, and not independent observations (as was the case in the analysis of Teuling et al., 2009) should be acknowledged.

Page 3, Line 25: "calibration or tuning of new parameters is thus unnecessary". This is a very strong and somewhat strange statement. Surely, the parameters used might result in reasonable estimates of evaporation, but *all* of the parameters in the model will turn out to vary both in space and time when studied in detail. Even the PT-"constant" is known to vary from 1.08 to 1.75 depending on, unfortunately, evaporation itself.

Page 3, Line 26: "explicit coupling between evaporation and soil moisture conditions". This coupling is not explicit since the soil moisture balance is controlled by data assimilation.

Figure 3/4: Figure 3 contains interesting information, but why isn't bare soil evaporation included? Alternatively, Figure 4 could be extended with small panels on the sides of the maps that show the latitudinal distribution of all components.

Figure 6: This data is indeed suitable for plotting with a bivariate color scheme. Such schemes, however, are notoriously difficult to read, and care should be taken that the use of information in color is maximized. In the current color scheme, only a limited subset of all possible colors is used (i.e., the green component seems not to be used),

effectively compromising the interpretation. A wider spectrum of colors is used in Figures 4 and 5, whereas they only use 1D color schemes. A method that makes more effective use of the degrees of freedom offered by the different color components is described in Teuling et al. (2011). A code is available upon request.

In addition, the authors should be aware of and account for possible negative correlations between soil moisture and evaporation in humid regions. These are not artifacts or noise, but an inherent consequence of the non-linear relationship between soil moisture and evaporation.

Detailed comments

Table 2/3: I assume these values are yearly sums, and not the total sum over the period 2003–2007?

References

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