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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.

Anonymous Referee #2

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Review of "Global land-surface evaporation estimated from satellite-based observations", by Miralles, Holmes, De Jeu, Gash, Meesters, and Dolman.

This is an interesting paper, describing a laudable, if perhaps somewhat academic, attempt to establish global evaporation fields through remote sensing data alone. The paper does suffer, however, from a lack of context – the uninitiated reader will not understand just how limited the satellite inputs to the evaporation estimates are. The reader is never told that an approach that combines remotely sensed data with superior ground-based data (e.g., using observed rain gauge measurements) should in principle produce an evaporation data set superior to the one described here. I recommend that the authors qualify their paper according to the following points.

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

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. 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.

Evaporation data for GSWP2 are freely available on the internet.

3. 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 interannual variability of evaporation at a single site. Can the authors comment?

4. 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.

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.

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