Editor Decision: Publish subject to minor revisions (Editor review) (22 Oct 2014) by Paul Stoy

We would like to thank the handling editor Dr. Paul Stoy for the insightful suggestions that helped to improve the manuscript.

Comments to the Author:
The authors have adequately addressed referee comments, but I feel that the manuscript requires further improvement for publication in Biogeosciences. I hesitate to accept a paper that states 'The promise of seamless land-sea observations of surface heat fluxes offered by the approach appears to warrant investing the required effort.' as the last line of the Abstract, and 'preliminary development' at the end of the Introduction. A published manuscript should be complete.

Reply: The stated sentence in omitted from the abstract.

The term 'preliminary' is omitted at the end of introduction.

Much of the manuscript reads like an extensive justification of the simplifications used, with hypothetical explanations for why the approach often fails. One way of adjusting the manuscript emphasize the novelty of the approach is to place it in a framework where the flux estimates are shown to (approximately) agree regardless the (rather substantial) assumptions required by the approach, which simultaneously defends the assumptions while leading to future research in investigating why the approach works or fails. I also feel that some key references, listed below, have been missed and that the manuscript still needs improvements in specific areas.

Reply: The references listed below are cited now in the revised manuscript wherever appropriate.

On line 54, 'appear to fall' is not sufficiently specific. Evaporation routines can be classified with more rigor. I also note a critical new analysis by the Salvucci group (listed in References below) that does not rely on surface parameterization. A table of existing approaches would emphasize the novelty of the current approach.

Reply: The paragraph is now corrected as 'The methods employed thus far can be categorized based on the various approaches followed to determine E. The most common approach centres on assuming a physical model of evaporation given many of the variables required to compute evaporation using these models are available directly as satellite products (e.g., land surface temperature, vegetation index, albedo etc.) (Choudhury and Di Girolamo, 1998; Mu et al., 2007, 2011). The Priestley-Taylor (Priestley and Taylor, 1972) based model for estimating monthly global E relies on constraining the Priestley-Taylor parameter with meteorological and satellite based biophysical variables (fractional vegetation cover, green canopy fraction, vegetation index, etc.). In contrast, a number of studies have also tried to resolve E indirectly by estimating the evaporative fraction from the relationship between satellite derived albedo, vegetation indices, and land surface temperature (Verstraeten et al., 2005; Batra et al., 2006; Mallick et al., 2009). More recently, Salvucci and Gentine (2013) proposed a novel method for determining E based on minimizing the vertical variance of relative humidity while simultaneously estimating water vapor conductance and E.

A list of the widely used global and regional scale satellite based E models is listed in Table 1.'

A table (Table 1) is added now stating the widely used global and regional scale satellite based E estimation approaches.

Salvucci and Gentine (2013) PNAS paper relies on the land surface parameterization to estimate the aerodynamic conductance based on Monin-Obukhov similarity hypothesis. With the sub-model of aerodynamic conductance this approach minimizes the vertical variance of relative humidity to simultaneously determine the water vapor conductance and evapotranspiration.

Equation 5 requires further justification with respect to the dissimilarity between kH and kE. Note Katul et al. 1995 (below) and related references.

Reply: Added now, please see line 145 to 150.

Define 'radically different space' on line 155.

Reply: Defined now, please see line 168-169.

On line 160, advective fluxes occur across a range of scales in space.

Reply: This is now line 174. Correction incorporated.

On line 168 'it is important to appreciate that the landscape heterogeneity is likely to increase with scale': the relationship between landscape heterogeneity and scale can be quantified for the 30 study towers.

Reply: Good point. The role of landscape heterogeneity is now quantified and shown in Figure 6 (a and b). Figure 6 is also explained in the discussion section (line 431 to line 447).

The sentence is modified as 'it is important to appreciate that the landscape heterogeneity is likely to increase with scale. Therefore, although the satellite-based method we are proposing has promise as an observation platform, relating these observations to unique surface characteristics is likely to be problematic [despite an attempt is made (Figure 6) to explain the retrieval errors in light of the vegetation biophysical heterogeneity]'. Please see line 182 to 187.

On line 240 and elsewhere, is there a relationship between spatial heterogeneity or representativeness and lack of fit?

Reply: Yes, the connection between the landscape level spatial heterogeneity and lack of fit is now explored by investigating the relationship between the variance of EVI and land surface temperature with the slope of regression between the observed and estimated E. The results of this analysis is described in the discussion section (line 432 to line 439) and demonstrated in Figure 6.

On line 245, data rejection descriptions best belong in the Methods section.

Reply: There were two data rejection criteria. The first criterion related to the AIRS soundings is discussed in methods section now (line 221 to 224). However the data rejection in relation to the Bowen ratio rejection criterion is retained in the results section.

I disagree with line 408, a relatively straightforward investigation of representativeness and/or spatial variability for thirty towers is likely to add insight into the findings. We performed such an analysis for 173 ecosystems in Stoy et al. (2013, AFM) and it wasn't too onerous.

Reply: Thank you for the suggestion. We have performed this analysis and the results are shown in Figure 6 and described in discussion section in line 432 to line 439. The Reference of Stoy et al. (2013) is also mentioned.

Likewise, it is extremely straightforward to find if there is a relationship between tower height and goodness of fit on line 415.

Reply: Good point again. The relationship between average tower height of the biomes and goodness of fit is investigated in the revised manuscript (Table 3 and Figure 4). Please find the description in the results section (line 320 to 326).

On line 465 and elsewhere, maybe just remove the discussion of non-terrestrial fluxes if there is no basis for comparison.

Reply: The discussion on non-terrestrial fluxes is removed. However, in the conclusion section, we have mentioned this method could potentially be used for interrogating the oceanic latent heat fluxes.

In figure 4, I am assuming that the x axis is month.

Reply: Yes. Figure 4 is now Figure 5. This is now explained in the figure caption.

References

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Sahoo, A.K., Pan, M., Troy, T.J., Vinukollu, R.K., Sheffield, J. and Wood, E.F., 2011. Reconciling the global terrestrial water budget using satellite remote sensing. Remote Sensing of Environment, 115(8): 1850-1865.

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Vinukollu, R.K., Wood, E.F., Ferguson, C.R. and Fisher, J.B., 2011. Global estimates of evapotranspiration for climate studies using multi-sensor remote sensing data: Evaluation of three process-based approaches. Remote Sensing of Environment, 115: 801-823.

Reply: The references are added in appropriate places.