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Interactive comment on "Components of near-surface energy balance derived from satellite soundings – Part 1: Net available energy" *by* K. Mallick et al.

Anonymous Referee #3

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The paper presents a simple promising approach for estimating monthly global fields of net available energy (φ =Rn-G) for exchange of heat between the Earth surface and atmosphere. The method is based on a novel approach whereby φ and the surface heat capacity (c) are solved analytically using inputs of the day-night temperature difference (dT) and Rn which are both estimated from satellite data. The method is directly applicable on a global scale and no empirical relationships between soil heat flux and vegetation indices or assumptions about G-Rn ratios are needed, as is usually the case. However the method is based on a number of other assumptions, and it would strengthen the paper to include a test of the method using higher spatial resolution data (field data or MODIS data) before its application to global low resolution ($\approx$ 100 x 100

km2) AIRS data which is evaluated using FLUXNET data (with much smaller footprint). Currently, it seems like the estimated global radiation (Rs_in) could be the main reason for the (small) biases in Rn and φ, and it would be valuable with further analysis and discussion of this problem.

1) One assumption is that the system is approximately in equilibrium over 24 hours. In practice, all components of the diurnal energy balance are highly dynamic, however the method is applied to assess the mean monthly dynamics in which case the diurnal variability is smoothed out. Thus, it should be clear in title and abstract that the method is applicable only for estimation of average (eg. monthly) fields of φ.

2) Another assumption is that at 01:30 h, $\varphi \approx 0$. Even though this seems plausible, I suggest to use the field data to support this hypothesis. This would strengthen credibility of the method.

3) As already noted by the authors, the assumptions lead to a dependency of daytime G on net long wave radiative energy balance which seems strange. Again, I suggest that the field observations are used to examine this relationship.

4) A MODIS global albedo product is available that could be used directly, but instead the broadband solar reflectance from the 7 MODIS spectral (bidirectional) surface reflectance bands are used for calculated the global albedo. Please explain the reasons for this.

5) A constant albedo for oceans (0.04) is assumed that do not consider sea ice. Why is surface albedo not assessed for oceans from the satellite data as it is for the land surface?

6) For the evaluation of the results in Fig.'s 3 and 4 and Table 3, it is unclear whether the data-based φ is derived as (Rn-G) or (λE+H). It makes sense to use (λE+H) for large-scale evaluation due to the larger foot print of atmospheric fluxes than (Rn-G), as mentioned earlier in the paper, but please clarify whether this is the case in Fig/table

captions. Calculation of data-based φ requires energy balance closure of data which is typically not the case (an analysis for all Fluxnet sites is seen in Stoy et al., 2013). Amiro (2009) show that the energy balance closure is better fulfilled when data are averaged over longer periods. Is this the case for the data used in the current study? Please report on the monthly energy balance closure of the data used.

7) If possible, please provide information about the area extension of the biomes that are represented by flux sites and show a biome map as background for the eddy co-variance site map in Fig. 1.

8) The importance of the bias in Rs_in for Rn and φ estimation can be tested using the field measurements of Rs_in as inputs. I suggest testing of the method using field data and discussion of possibilities to improve the satellite based estimation of Rs_in. A very simple method is used for Rs_in estimation in the paper which is based on a constant global clear-sky atmospheric transmissivity. Since Rs_in seems to be the main issue, what is the potential for improving clear-sky transmissivity and the global Rs_in estimates? (the problem seems to be largest at high Rs_in, so inaccurate clear-sky transmissivity could be an important reason for model bias). Please compare with other studies and include discussion of this.

9) p. 15, l. 365-376. Regarding discussion about energy balance closure and its possible attribution, an extensive analysis of energy balance closure of all (173) FLUXNET sites were recently given by Stoy et al. (2013). Interestingly, the energy balance closure is generally best for savannahs and evergreen deciduous forests, and the results suggest that landscape heterogeneity (in addition to canopy heat storage) could be responsible for lack of energy closure. This hypothesis suggests that lack of energy balance closure is not attributed to systematic errors in Rn, G, H and λE. Amiro (2009) also indicates that energy balance closure is not an issue over longer time scales (only one site studied). In the current paper, it is quite obvious that the underestimation of Rs_in is a problem that will propagate to Rn. The importance of the bias in Rs_in for Rn and φ estimation could be tested using the field measurements of Rs_in as inputs.

References should also be included for comparison with Rs_in estimation results in other studies (as was done for discussion/evaluation of Rn results), and perspectives to improve global Rs_in estimation discussed (see also comment 8).

Minor issues:

p. 7, l. 144. Please include unit for c in parenthesis following its presentation.

P. 13, l. 301. “Table 2” should be changed to “Table 3”.

Fig. 3. In figure caption, reference to Table 2 should be changed to Table 3.

Fig. 3: How were the longwave radiation component RL_in(tower) and RL_out(tower) estimated? Were they measured directly or calculated from air/surface temperature data?

Table 3. Please specify in caption that the statistics are based on monthly values. Please also provide the percentage errors to facilitate comparison with discussion on page 13.

P. 12, l. 295-> . Please include description of results shown in Figures 3c, 3d and 3e (only 3a and 3b are described). In particular, Fig. 3c indicates that underestimation of Rs seems to be the reason for underestimation of Rn.

p. 13, l. 302. It should be specified that the error results of Bischt (for Southern Great Plains) are comparable to results for grassland in the current study. In fact, other biomes show larger errors for Rn.

p. 13, l. 315. What was the surface type (or biome) in the study of Stisen et al. (2008)? And what was the time resolution of their results? Hourly, daily, monthly?

p. 14, l. 335. Even though there seem to be only a marginal difference between measurement and calculations, then the bias becomes important for radiation budgeting which should not be neglected.

References:

Amiro, B. (2009). Measuring boreal forest evapotranspiration using the energy balance residual. Journal of Hydrology 366: 112-118.

Stoy, P.C., Mauder, M., Foken, T., Marcolla, B., Boegh, E., Ibrom, A., Arain, M.A., Arneth, A., Aurela, M. Bernhofer, C., Cescatti, A., Dellwik, E., Duce, P., Gianelle, D., van Gorsel, E., Kiely, G., Knohl, A., Margolis, H., McCaughey, H., Merbold, L., Montagnani, L., Papale, D., Reichstein, M., Serrano-Ortiz, P., Sottocornola, M., Saunders, M., Spano, D., Vaccari, F., Varlagin, A. (2013). A data-driven analysis of energy balance closure across FLUXNET research sites: The role of landscape-scale heterogeneity. Agricultural and Forest Meteorology 171-172: 137-152.

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