Reviewer 3:

General comments:

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 (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.

We thank R3 for the supportive and helpful comments. We accept that to demonstrate the efficacy of the approach at higher spatial resolution would offer more reassurance to the reader (as also raised by R1). We will include a test of the proposed methodology over some representative FLUXNET sites using tower measured Rn and radiometer measured land surface temperature only. However, given ultimately we have to use the AIRS sounding data for the overall method; we are constrained to this scale. As such, it is this scale that has to be ultimately evaluated and hence scaling will remain a recalcitrant issue.

See below for comments on Rs_in.

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.

We agree, the monthly nature of the method needs to be made explicit throughout as also highlighted by R1.

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

We shall add an additional figure to support the assumption of $\Phi \approx 0$ at 1.30 hours (see response to R1).

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.

This is simply a product of the discretisation of the surface energy balance equations and stems directly from the $\Phi \approx 0$ at 1:30 assumption for which we will present an additional figure. In order to evaluate this assumption as suggested we would need independent observations of G and Rn. We do not believe heat flux plate data provide an adequate evaluation data set.

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.

The MODIS global albedo product contains black sky and white sky albedo. Look/up table based atmospheric informations and parameters are needed to convert them into the blue sky albedo. But there are established formulations (Liang et al., 1999, Liang, 2001) to directly convert the narrowband reflectances into the broadband visible albedo that does not depend on any atmospheric variables and look-up tables. This explanation will be included in the revised version of the manuscript.

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?

There is no ocean surface albedo data available in either AIRS or MODIS products and we shall mention this in the revised manuscript. Given the structure of the atmosphere and the very small energy fluxes involved, high latitude Φ and latent heat estimates from this method are likely to be problematic anyhow. We will emphasise this in the revised manuscript.

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.

In the present case Φ is derived as λE+H and this will be stated explicitly in Fig 3, 4 and Table 3 in the revised manuscript. We fully accept the issues around lack of local scale closure in tower data and highlight this in the text. We have used the monthly averages of (AIRS overpass time) 13:30 hours net available energy from the eddy covariance tower. We will point this out that helps improving the robustness of the closure assumption used to derive λE+H.

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 covariance site map in Fig. 1.

The location of the eddy covariance sites and their corresponding biome categories are already given in Table 1. We believe this is sufficient given this is traditionally how such information is communicated.

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.

Good point, as R1 also pointed out. We accept that a more sophisticated treatment of the atmospheric transmissivity would improve the scheme, but rather than include this in the current manuscript (it represents a significant amount of additional work to develop and evaluate a model independent approach!), we would rather expand the discussion on the potential avenues to improve the atmospheric transmissivity retrieval.

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).

We will add the reference of Stoy et al. (2013) and Amiro (2009) in the revised version of the manuscript. The sensitivity analysis presented in Table 2 is already indicating the significant sensitivity of Rn and Φ to cloud cover fraction and atmospheric transmissivity. This shows the method presented in the manuscript to estimate Rs_in needs further improvements and the manuscript is clear in this regard. However, this is non-trivial and, given this is thrown up in the evaluation, as with all 'methods' papers, it is a matter of further work in our opinion. We propose to expand the discussions on Rs_in with suitable references.

Minor issues:

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

OK.

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

OK.

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

OK.

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?

Both the longwave radiation components were measured directly over the tower sites in question. The measurements were available for a limited subset (14) of tower sites and this is already mentioned in the manuscript in line 358 to 360.

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.

OK.

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.

The description of the results in Fig 3c is already given in Line 332 to 344. Similarly the descriptions of Figs 3d and 3e are also given in line 358 to 374. While probing into the detail behind the reasons of Rn and Φ underestimation, we had analysed the shortwave and longwave components. We shall make Figs 3c, 3d and 3d into a separate Figure to clear this confusion.

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

We shall mention this in the revised manuscript.

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?

Stisen et al. (2008) conducted their study on grassland and used 15 minutes geostationary satellite data. This will be mentioned in the revised manuscript.

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.

Point taken.

References:

Liang, S. (2001), Narrowband to broadband conversions of land surface albedo, Remote Sens. Environ., 76, 213–238.

Liang, S., Strahler, A., and Walthall C. (1999), Retrieval of land surface albedo from satellite observations: A simulation study, J. Appl. Meteorol., 38, 712–725.