Reviewer 1:

General comments:

The paper of Mallick et al (2014) presents a simple and potentially interesting approach for estimating midday net radiation and net available energy at the Earth' surface (Rn-G) from satellite sounders. The method has been applied to data acquired twice daily by AIRS and MODIS sounders, obtained or aggregated at 1° resolution and monthly averaged, and extensively evaluated with the help of ground observations of turbulent heat fluxes from 30 FLUXNET stations. This validation allowed the authors to draw conclusions on the quality of the estimates for each biome considered.

The paper is within the scope of BG journal, globally well written, and the results well presented in a concise style that the readers may appreciate. However, the paper intents to address multiple objectives at the same time (a new methodology to compute surface heat capacity, application with satellite data and generation of a dataset for use in an evapotranspiration model presented in a second paper). This makes the paper not very focused on clear objectives, and sometimes do not address fully each of them properly.

The paper would probably gain in focusing on one objective only (the methodology, for example), or merge the two papers intended for the series. From my point of view, the method to derive Rn-G, and in particular surface heat capacity, is particularly interesting as such, although some assumptions need to be checked, as well as the extensive validation at FLUXNET sites. In that perspective, I think that the paper should be more focused on the method, which is the most innovative part of the study. I would therefore strongly recommend to first validate the new method at FLUXNET sites using ground observations of the surface radiation components as input, before applying it with satellite data, as the latter part has already, but partially, been done in other studies with a high degree of success (eg Verstraeten et al (2005) with NOAA-AVHRR for instantaneous net radiation). An alternative would be to integrate Paper 1 (this paper) into Paper 2 (paper on the Bowen ratio), as it would justify better some choices made in the methodology (monthly time scale, combination of AIRS and MODIS data).

We thank R1 for their helpful comments. We understand R1's position with respect to wanting to see a fuller evaluation of some of the various steps taken to produce the net available energy estimates. Although many of the steps have been evaluated elsewhere, novel aspects such as the derivation of G and surface heat capacity do require a fuller evaluation here. As a result, we propose to demonstrate the retrieval of Φ using in-situ surface temperature and day-night Rn observations of FLUXNET eddy covariance towers and evaluating the Φ retrievals against observations at the tower sites.

We have also explored a single merged paper, but it becomes unmanageably long and so our preference is to keep the existing two (joint) paper format. Providing the links between the two are clear and they are published together, we believe this is the most efficient and effective means of presenting the overall framework.

1. Questions on the choice of specific satellite data for this study.

• After reading the paper, it is not clear to me why a combination of AIRS and MODIS data has been chosen. Is there any reason to prefer the use of AIRS data in this context compared to MODIS (as described in Peng et al, 2013)? Certainly, the use of AIRS data make the validation task much more difficult because of the huge difference in spatial footprint, and this choice imposes the results to be concentrated on 1 time slot per day, 13:30 local time. Other satellites could possibly have been used to have more passes per day.

One of the core objectives of the work is to explore the potential of atmospheric sounding data. AIRS is the only dedicated sounder available which can be explored to address the objectives in the paper. Although MODIS has soundings, it was not designed for this and only has low quality air temperature soundings. We fully appreciate this introduces many difficulties when it comes to the evaluation, but the most important aspect of these two joint papers is to introduce the possibility of using sounding data as a means of observing surface energy fluxes. We have restricted the net available energy derivation to (largely) AIRS data (we use MODIS albedo because AIRS does not contain any albedo field) in order to exploit a single platform for the entire framework, something we believe is important.

See comments below on handing the scale mis-match.

• A monthly time scale has been chosen for the study. The perspective of this choice is not exposed. One may wonder why daily or weekly time scale has not been selected, as it could be at least equally or more interesting to study the daily time scale for monitoring.

The daily 1 degree data contains orbital gaps and cloud contamination. In the 8-day data the co-incident land surface temperature in both day and night pass was missing and the atmospheric soundings were also missing in many places. It is the monthly dataset where the soundings as well as both the day-night land surface temperatures were available and the data has complete global coverage. This is the reason for using the monthly datasets. We appreciate that although this is mentioned in the manuscript it needs to be declared throughout, including the title.

2. Questions on the assumptions

• I found the method to derive net available energy interesting, and certainly very promising for further applications. However, the paper would gain in credibility if all the assumptions were checked to be realistic in using ground observations. The assumptions to be checked are: the symmetry of the Ts difference over a month, Rn - G = 0 at 1:30 AM LT (Although there is already an indication of it in the paper, an information of the distribution of the Rn-G at night may be useful).

To address R1's first point we propose showing a figure of the 30 minute samples of Ts for a monthly averaged day highlighting the 13:30 hours and 1:30 hours samples. This will show how well these two samples capture the dynamic range of the day and hence the discretisation is representative of the daily energy balance.

We also propose to show the diurnal distribution of Rn-G to support the hypothesis Rn-G = 0 at 1:30 AM.

• It is assumed that the estimates at 1 degree can be directly compared with point scale observations at the surface, which is an uncertain hypothesis. The authors themselves point out in the paper that the scale mismatch can be a source of discrepancy in their results, but without quantifying this effect. I would suggest to first verify the validity of the new methodology to compute Rn-G with ground observations only (if possible, or at least with satellite data at finer scale), and then to apply it at global scale with satellite soundings. This would certainly help in both acknowledging the accuracy of the new method and better understand the effects of scale mismatch.

Comparing these two very different scales is not a matter of choice, but necessity. It is important to keep in mind that our objective is to use AIRS soundings to estimate the surface latent heat flux and that in using AIRS we are restricted to this scale. That said, we accept that we could derive some higher resolution Φ estimates using tower infrared radiometer measurements of surface temperature along with corresponding net radiation measurements as detailed above and compare these against tower data. We accept that this would improve the manuscript and thank R1 for the suggestion.

3. On the structure and text of the paper.

• The time scale should be mentioned in the title, the abstract and the introduction.

Good point, although it is important to realise the estimates themselves are not monthly averages, but 13:30 and 1:30 values computed from monthly data.

• The satellite datasets should be described first to interpret correctly the methods as they imposes constrains, or the methods should be reformulated to avoid relying on AIRS and MODIS specific products, leaving their description for after.

Necessary corrections will be incorporated in the revised manuscript.

• In the discussion section, the authors give a series of references to support the quality of their estimations. From my point of view, I found difficult to state the quality of the proposed estimations compared to the other studies as such: some references concern instantaneous values, others daily averages, some of them on very limited samples, but all with satellites soundings at finer scale (from MODIS, GOES). If I add three other references I found (Verstraeten et al, 2005; Jin et al, 2011; Peng et al, 2013), it is still difficult to know how to compare to the RMSD found in this study with others (See Table below). The suggestion of point 2 would certainly help the readers to apprehend the effect of scale discrepancy with the error due to the methods, assumptions. In addition, the authors could explain how to compare the different results from other studies with theirs.

We agree our RMSD is not comparable to the previous studies and we shall make it in the text. RMSD is getting impacted in two ways, due to scale mismatch and due to the time integration. This applies to both the spatial scale mismatch and time integration and again we thank R1 for raising this.

• The last paragraph of the discussion section should be moved (and reformulated) to Introduction, as it justifies some choices, and make a link to the second paper.

Good point, we shall include this in the revised version.

• The conclusion section should state the conclusion based on the results obtained, the potential applicability to other sensors and perspectives. A part of the conclusion section (first paragraph) should be moved to the discussion section, as limitations of the method are discussed. A clear conclusion should be stated here. Applicability to other sensors is briefly mentioned by citing other satellite missions, but sometimes sounds a bit far stretched, without the authors giving a clue on how to proceed (for example, how to apply the method initially tested for monthly averages once a day to get 30 min estimations of Rn-G ?). Therefore, I would suggest either to limit the sensors list to which the method can be applied, or give a short explanation on how to proceed (the option I would recommend).

Again, good point. The first paragraph of the conclusion will be moved into the discussion section. Some discussion on the future use of the method will also be stated in the conclusion section.

4. Typos and additional references

• p11828, eq 1: $\Phi = \lambda E + H$

• p11832, eq 8: $\lambda E(t)$

Both the typo errors will be corrected.

• Verstraeten W., Veroustraete, F., and Feyen, J., 2005: Estimating evapotranspiration of European forests from NOAA-imagery at satellite overpass time: Towards an operational processing chain for integrated optical and thermal sensor data products, Remote Sensing of Environment, 96 (2), 256-276.

• Peng, J., Liu, Y., Zhao, X., and Loew, A. (2013): Estimation of evapotranspiration from MODIS TOA radiances in the Poyang Lake basin, China, Hydrol. Earth Syst. Sci., 17, 1431-1444.

• Jin, Y., Randerson, J. T., and Goulden, M. L. (2011): Continental-scale net radiation and evapotranspiration estimated using MODIS satellite observations, Remote Sensing of Environment, 115, 2302-2319.

These three references will be added in the revised manuscript.