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Comment

## ***Interactive comment on “Components of near-surface energy balance derived from satellite soundings – Part 2: Latent heat flux” by K. Mallick et al.***

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Near-surface energy balance derived from satellite soundings – Part 2: Latent heat flux” K. Mallick et al.

Referee 1 (R1) General comments The paper presents a method to partition net radiative fluxes by using atmospheric measurements from the AIRS sensor an algorithm based on the Bowen ratio methodology. Deriving reliable land surface heat dataset is needed to improve climate scale water and energy cycle characterization. Although some aspects of the paper and methodology may be discussed (as acknowledged by the authors, who present the study as a preliminary development) the paper can be a

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relevant contribution to the field and merits to be published in ACPD for discussions. Despite the existence of a large body of work characterizing the land surface heat fluxes from the local to the regional scale, the extension to the global scale requires simplified formulations adapted to the existing global datasets and which are robust against the data uncertainties. On going inter-comparisons of the available global latent heat flux products show still a relatively large spread in their estimates, so there is ground for alternative formulations that could help refining future methodologies for the operational production of a global land surface flux. No methodology is perfect, but the forcing data used (AIRS) to drive the algorithm and (as pointed out by the authors) the relatively simple modeling involved (compared with the more complex parameterizations used in many land surface models) can make this dataset an interesting alternative to more established methods.

1. The relatively simple modeling of this approach also facilitates a better characterization of expected errors, compared with more complex formulations. For instance, a database of realistic atmospheric temperature and pressure profiles (e.g. Chevallier, F., et al., 2000: TIGR-like atmospheric profile databases for accurate radiative flux computation. *Q. J. R. Meteor. Soc.*, 126, 777-785) with AIRS expected errors randomly added (e.g. Eriksson, P., et al., Qpack, a tool for instrument simulation and retrieval work, *Q. J. R. Meteor. Soc.*, Volume 91, Issue 1, 2005, Pages 47-64) could be used to characterize the expected Bowen ratio error, and how that propagates into the estimation of the latent heat flux. An alternative would be to check the sensitivity of the parameterization in an analytical way (see e.g., Wang, K., et al., (2007), A simple method to estimate actual evapotranspiration from a combination of net radiation, vegetation index, and temperature, *J. Geophys. Res.*, 112, D15107, doi:10.1029/2006JD008351, or Fisher, J. B., et al., Global estimates of the land-atmosphere water flux based on monthly AVHRR and ISLSCP-II data, validated at 16 FLUXNET sites, *Remote Sensing of Environment* (2007), doi:10.1016/j.rse.2007.06.025). Adding this analysis to the paper will help to put in perspective the comparison with the tower fluxes (which may be of importance, due to the 3 orders of magnitude spatial scale miss-match in the

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comparison) and will make the paper more complete, especially before a possible final publication in ACP.

Response: A very good suggestion and a notable omission from the original submission(s). We have now conducted this analysis and will include the results in the resubmission.

2. The paper is statistically well presented. However, the related wording describing the results may be a bit confusing. For instance, a correlation of 0.34 (much lower than other “r” discussed in the paper) is qualified as significant (does it mean significant in the sense of a t-test, or just a word qualifying the strength of the correlation?). The other correlations are not qualified as significant, even if the “r” are larger. The relation in Figure 2a is judged as well defined (in statistical terms again, or in a more loose way?). However, other relations showing much less scatter (same figure) are not judged. A more consistent approach may help interpreting the results.

Response: All estimates of  $r$  are presented with their associated estimated standard deviation. As a result the aim was for the reader to use this to judge the significance for themselves (e.g.  $\pm 2\sigma \approx 95\%$  confidence). As a result, the use of the term ‘significant’ was in its strict sense. However, clearly R1 would like this to be handled more explicitly. Therefore, we would like to suggest adding a footnote upon first use, spelling out our use of the estimated uncertainty band for the reader. In addition we have reviewed the use of the term ‘significant’ to ensure it is in keeping with this definition and that this is clear to the reader.

Specific comments:

L153. It may be worth commenting also the validation results in the paper cited and not only the expected resolutions. As this paper concentrates on the land fluxes, the fact that the validations show worst performance over the mid-latitude land site is of importance (1-2 K (T) and 25-35% (water vapor) RMS errors).

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Response: The validation results of AIRS soundings are included now and will appear in the revised submission. The temperature and relative humidity sounding uncertainties quoted on L153 (for some reason R1 is reviewing the author's version?) are those quoted in the AIRS support literature as intended. However, we have included this uncertainty in the sensitivity analysis called for by R1, which we presume is the thrust behind R1's point here.

L221. Why is the evaporative fraction more linearly related to the tower fluxes, compared with the Bowen ratio?

Response:  $\lambda E = (1+\beta)\Delta D_d$

i.e.  $\beta$  is inversely related to the latent heat flux whereas,

$$\lambda E = \Lambda \Delta D_d$$

i.e.  $\Lambda$  is linearly related to the latent heat flux. We will make this more explicit in the text, although we believe it is relatively clear as is.

L222.  $r=0.34$  is described as significant (t-test?) but later on the correlations are not judged (e.g., the  $r=0.75$  in L237). See also the related comment above.

Response: L222 reads " $r = 0.34 \pm 0.06$ " not  $r = 0.34$ " i.e. the standard deviation of the estimate is also provided so that the reader can see that "significance" is intended in a strict sense. We propose inserting a footnote at this point so that it is absolutely clear how we intend the reader to use this information to judge significance for themselves.

L223. What is the number after the +/- symbol? I think an explanation is given in Table 1, calling this standard error of  $r$ . Still, it's not clear to me what this means, standard error of estimate, assuming  $\Delta_{\text{satellite}}$  is an estimate of  $\Delta_{\text{tower}}$ ?

Response: Again, we believe the addition of a footnote at this point will remove any ambiguity.

L228. Relation "well defined", see general comments. Again, what are the numbers in

brackets? According to the table, one standard deviation? What does this mean?

Response: ditto

L237. The correlation for the latent fluxes (0.75) is much higher than for the evaporative fractions (0.34), i.e., the absolute fluxes are better estimated than the flux partitioning. Could this be an indication that the seasonal component of the radiative fluxes contributes to the larger correlation found for the fluxes? A small discussion on this may be interesting.

Response: Correct. The sensitivity analysis we have now conducted spells this out explicitly and the modified text will add a brief discussion of this point.

L249. The figure could be more commented. The seasonality is captured by the AIRS estimates at some stations but not all (e.g., Vielsam, Tsukuba, Skukuza).

Response: We agree and will modify text accordingly

L261. This paragraph is somehow confusing. What does it mean a compensation error? Looking at Figure 3, one can see that it's not only a SAV where the latent fluxes are over-estimated (e.g. at Santarem, far from a dry region, or Puechabon).

Response: By compensation error we mean that overspecification (or error in) of one component ( $\Delta$ ) is offset or 'compensated' for by underspecification (or error in) another component ( $\Delta d$ ). On L261 the overspecification being discussed is in  $\Delta$  not  $\lambda E$ , which appears to be largely specified correctly. We will modify the text to remove any ambiguity.

L295. The differences between H satellite and H tower may be related to measuring different "Hs", but they will not be the only factor explaining the differences. "This explains" may be re-phrased by something like "This may be responsible for part of the differences . . . ." to make this clear.

Response: Changed to "This could partly explain" from "This explains".

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L312. Tower aggregate at 1 km here, at 10 km in L192, change for consistency.

Response: Will change to <10 km scale for consistency.

L313. Tower are installed in relatively homogeneous terrain, but this is normally judged at the tower scale (1-10 km), while the discussion here is for  $\sim 100 \times 100 \text{ km}^2$  satellite estimates (3 orders of magnitude larger scale). The references given discuss these issues but typically for much smaller satellite scales (e.g., MODIS at  $\sim 1 \text{ km}$ ). It may be worth mentioning again the very large scale miss-match here.

Response: Good point. Will change to: “Although towers are often installed in relatively homogenous terrain at the <10 km scale, rarely can this be assumed for scales approaching the 100 km scale of the AIRS data. In addition, characteristics such as surface wetness and temperature can still be highly . . .”

L322. What do you mean by general scaling from tower to satellite? Spatial scaling? What’s the relation with the time averaging?

Response: Will replace “general scaling” with “spatial scaling” as this is what is intended. If the sources of heterogeneity are dynamic (like surface wetness) then time averaging will tend to average out this effect. We will add a sentence on this effect.

L326. Could the better agreement for the more extensive forests being related to larger seasonal cycles (compared with other regions) and the more radiation-driven latent fluxes?

Response: We see no reason why the seasonal range per se should result in agreement between tower and AIRS estimates. The dominance of the radiative forcing of latent heat will obviously improve the correlation but not necessarily the absolute agreement. We will expand on this in the revised paper.

L330. In the same way that the time average of the compared fluxes is given for Mecikalski et al. (daily), it should also be given for the other comparisons in this paragraph to help judging the given errors. Larger errors are expected the shorter the

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integration time, so it is relevant information.

Response: We agree, for the other comparisons the errors in instantaneous and daily latent heat fluxes will be included in the revised submission.

Technical corrections L54.  $\lambda E$  not defined.

Response: Changed to E

L270. "Submitted this issue", but in L258 "to this journal", change for consistency.

Response: Done

Table 1. See above, not sure the reader will understand the descriptions for the bracket numbers.

Response: See above

Figure 1. Having the ocean fluxes is interesting, but the required color scale does not allow to properly see the fluxes over land (the main interest of the paper). Having an extra figure with only land fluxes will help to judge the global annual means by comparing with other reported maps. It may help to have a set of e.g. 10 discrete colors, instead of a continuous color palette. Also, why there is no missing data for the sensible flux over Australia (Bowen ratio not calculated in panel a)?

Response: We would prefer to keep the figure as is as we believe it conveys the global pattern well including the land fluxes. As for the missing data, this was a presentational mistake; we have corrected this now and will be included in the revised submission.

Figure 3. Giving the month in numbers and names seems redundant.

Response: We will remove month numbers.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14417, 2010.

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