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

Anonymous Referee #2

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The authors propose a novel application of an old method. Instead of applying the Bowen Ratio as a micrometeorological method used at the field-scale, they make use of satellite soundings to apply the Bowen Ratio method to large spatial scales. They test the method against flux tower measurements.

1. Given the rather large leap in spatial scale of the application of the Bowen Ratio, it might be informative to more systematically evaluate why the original assumptions (and typical assumptions of most subsequent uses) of the Bowen Ratio Energy Balance method remains valid or nearly valid in this modified use of the method. Typically, there are three basic assumptions layed out for the Bowen Ratio method to work: i. 1-D vertical transport, ii. homogeneous land surface, iii. steady-state conditions (Fritschen

and Simpson, 1989, Journal of Applied Meteorology, 28: 680-689).

2. The dramatic difference in spatial scale between the flux tower footprint and this satellite based method would seem to inherently limit the ability of the flux towers to validate the method (a point made on Lines 350-355). Lack of energy balance closure at the flux tower sites is also an issue. Thus, it could be worthwhile to consider other methods to estimate actual ET that would be more consistent in terms of scale. Namely why not use an annual water balance method (P-Q=ET assuming minimal storage) for gaged watersheds at the approximately 1 ° scale? One would look at differences in ET across multiple years instead of monthly differences within the same year. Given the sometimes questionable relationship between this Bowen Ratio method and flux tower measurements at certain sites (Figure 3), this additional method of validation would provide a further check on the soundness of the method.

3. On Lines 355 to 380, the manuscript reports RMS errors in latent heat fluxes from other studies. It suggests that errors observed in this study are comparable to those in other studies. However, nearly all these other studies use daily fluxes or less. It would seem that the RMS errors for daily values represent something somewhat different, namely the ability to replicate daily variations in fluxes and maybe (but not necessarily) consistent bias in estimates. At a monthly scale, RMS error would seem to more strongly suggest bias since daily variations would cancel out over the longer averaging window. In essence, I might expect that a suitable RMS error for a monthly averaging window would be lower than that for a daily averaging window (much the same way it is often easier to model monthly streamflows relative to daily streamflows). Are there any additional studies that report RMS error for monthly fluxes?

Minor Comments: a. Lines 89 to 91 – Could this statement be explained in greater depth. Not enough context for most readers to understand the exact reasoning. Maybe a citation to something else?

b. Eqn's 4a, 4b, 5 and adjoining text. Inconsistent notation in terms of T, P, and Z.

BGD

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Sometime capitalized, sometimes not.

c. Line 224 – Make clear that the values in this expression came from the relationship shown in the caption of Figure 2. If one does not immediately look at the Figure 2 caption, it is confusing were this expression came from.

d. It would be helpful to repeat a full description of biome types in Figure 2. The abbreviations are not totally obvious and somewhat awkward to flip back to Table 2 depending how paper is layed out.

e. What is N in Table 2? Is this the number of months of observation across all sites within each biome? Please clarify.

Interactive comment on Biogeosciences Discuss., 11, 8085, 2014.