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Biogeosciences Discussions

Interactive comment on "Sensible and latent heat flux from radiometric surface temperatures at the regional scale: methodology and validation" *by* F. Miglietta et al.

Anonymous Referee #2

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This paper describes the results of a comparison between estimates of sensible and latent heat flux from a combined satellite and ground based method with aircraft measurements, for different land cover types. Such a validation of energy flux estimates is very useful, because validation dataset of spatial estimates of heat fluxes are scarce. The paper is well written, the methodology is described in a clear manner, and the results are encouraging. I recommend publication after a minor revision (see the numbered comments below).

1. There is a mistake in equation 6, the last part needs to be: /(Rn-G)

2. The psychrometer constant has a tilde only in Eq 6

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3. Page 1948, line 17: generation of aircraft -> aircrafts

4. Page 1952, line 20. Repeating twice, does it mean the measurements were carried out two times or three times?

5. Page 1953, line 18. The equation only considers incoming and reflected long wave and shortwave radiation. Long wave radiation emitted by the surface is not included here (but Rn in Fig 3b seems ok).

6. Page 1959, line 15, a space is missing between 'fraction' and 'of';

In addition, I would like to comment on two conclusions of the paper (the authors may or may not want to address these comments in this paper: they are not essential for the understanding of the paper, but they are relevant for people who want to use the methodology in the future).

I was interested in the applicability of the method in areas where no aircraft and fewer ground data are available. The authors say that the method can even be applied if no tower or aircraft measurements are available:

On page 1962, lines 11-14, the authors conclude that results for H are similar when aircraft surface temperature is replaced by satellite surface temperature.

On page 1962, line 25 to page 1963, line 2, the authors state that the method can be applied even when no tower data are available, by using models or measurements of stomatal resistance.

Looking at the methodology, if I substitute (the corrected) Eq 6 back into Eq 4, I find the following expression for H:

H = rho*cp*(Ts-Ta)/((Ts-Ta) + (es-ea)/psyc)) * (Rn-G)

In this expression, es is a function of Ts and rc/ra. The energy balance is closed (forced), and thus the distribution of available energy over H and lE is a function of Ts, aerodynamic and stomatal resistances. If we want to apply the method in the absence

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of tower and aircraft measurements, then the sensitivity of H to Ts, ra and rs is relevant.

In the above equation, Ts appears both in the numerator and in the denominator. How sensitive is H to Ts, compared to the sensitivity to the resistances ra and rc? Is the fact that aircraft surface temperature can be replaced by satellite surface temperature caused by (1) the good match between aircraft and satellite Ts (Fig 3a) or (2) a relative insensitivity of the model to Ts?

In this study rc was calibrated from flux measurements of the tower, and thus, the 'rae-estimates' of lE are dependent on the flux tower measurements. The authors say that literature values for rc can be used as well. I wonder whether the authors tested the sensitivity of H to the ratio of rc/ra, since ra and rc may play a key role (Eqs 7 and 8).

Interactive comment on Biogeosciences Discuss., 6, 1945, 2009.