Hydrology and Earth System Sciences Discussions

Interactive comment on "Analysis of the energy balance closure over a FLUXNET boreal forest in Finland" *by* J. M. Sánchez et al.

J. M. Sánchez et al.

juanmanuel.sanchez@uclm.es

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First, we would like to thank the reviewer for his/her comment/suggestion since they have contributed to improve the paper. Appropriate changes have been made following each one of the reviewer's comments/suggestions. In the following, detailed and justified responses, as well as the corresponding modifications into the manuscript (with appropriate reference to particular page and line numbers) are given.

Answer to Comments:

(1) Page 2685, last paragraph (soil heat flux discussion) – Here the authors state that the heat flux plates are placed "at a certain depth in the soil to avoid disturbances,

such C984 as the loss of contact. . .". But the loss of contact is always a possibility regardless of the placement depth of the heat flux plate. In fact, I rather suspect that partial contact is more the norm than the exception. So I would suggest that the authors revise the manuscript to allow for the possibility that the contact is incomplete. More specifically, they could assume that the G' is underestimated by 30% to 50% (which in my experience is not unreasonable) and see what impact this loss of contact may have on the energy closure. The authors also need to provide the mathematical details on how the soil heat storage was estimated.)

According to this referee suggestion, we have included the effect of this possible underestimation in G′ produced by the loss of contact of the plate. A new section 3.2, and a new Table 2 have been added dealing with this issue, page 11, line 1: "Also, an incomplete contact between the heat plate and the soil is possible yielding an underestimation of G′. Some authors suggest that this underestimation might reach 30-50%. In this work, an underestimation of 40% is assumed to analyse the effect of this contact loss in the energy imbalance. Results in Table 2 indicate a significant improvement in the energy closure produced by this hypothetical G′ underestimation." Also, mathematical details on how to estimate the soil heat storage were included, page 8, line 12: "The heat stored, ΔG, in the soil profile above the plate located at a depth "d", was computed from the temporal change in soil temperature (ΔTs) over the output interval "t", soil water content (θv), and ancillary data such as the bulk density (b) or the specific heat of the dry soil (Cd) (Tanaka et al., 2008): $\Delta G=(ãĂŰ\Delta TãĂŮ\_s\ C\_s\ d)/t$ (9) $C\_s=\_b$ $(C\_d+\_w\ \theta\_v\ C\_w)$ (10) Where w is the density of water and Cw is the heat capacity of water."

(2) Page 2686, discussion of storage terms – I think the authors need to provide the details of their method for estimating EC storage-fluxes (SH and SLE). Are they just integrating the time rate of change of the canopy water vapor and temperature profiles?

According to this referee comment, specific equations used to estimate EC storage-fluxes were included in Section 2.2, page 7, line 22: " $S\_(H=)$

$$\int\_\Omega C\_p(dT\_a)/dtdz (7) S\_(LE =) \int\_\Omega$$

$$\lambda$$

dq/dt dz (8) where is the density of moist air, Cp is the specific heat of moist air, and λ is the latent heat of evaporation."

The lack of any biomass heat storage is unfortunate, so I think the authors need to include a statement that they will discuss the significance of heat storage in the biomass in more detail later in the paper (which, in fact, is what they do in the upper half of page 2695).

Following this referee suggestion, a new sentence has been added after equation (3), page 4, line 22: "The significance of neglecting the biomass heat storage will be further discussed."

(3) Page 2686, discussion of ERB – I would recommend dropping ERB1 as a statistic. It is really not that useful of insightful. It might be more useful to include the 24-hour summation ratio as a measure of closure. The 24-hour summation ratio is Σ(LE+H)/ Σ(Rn – G-S) is approximately 0 for a 24-hour cycle, so that the 24-hour summation ratio is reasonably well and simply approximated byP(LE+H)/PRn. This approach obviates the need to include the storage terms and may help provide further insight into the nature of the lack of closure (that is, it is related to either the net radiation measurement or to the EC flux measurement, but is unlikely to be storage related).

The 24-hour summation ratio was already included in the paper. In Section 3.7 the study of the energy balance closure was repeated using different averaging time, including the 24 hour period. As mentioned in Section 3.7, only 10 days were selected for this analysis, those days with no more than 3 gaps in the half-hourly dataset. As shown in Table 4, "A much larger improvement in closure is shown when daily averages are considered to estimate EBR. ....daily scale, at which the effect of including the storage terms is negligible" (page 17, line 14).

Many studies on surface energy fluxes do not take care of the heat storage in the soil. However, as shown in this paper, its influence is significant in the results of the energy balance closure. For this reason the authors prefer continue showing the EBR1 values, if the reviewer agrees.

(4) Pages 2687-2688, discussion of spectral corrections issues – (a) The authors should give some idea of the magnitude of the correction terms, especially if they are using a closed-path system to measure LE.

According to this reviewer comment a new paragraph has been added to Section 2.2, page 7, line 13: "Massman (2000) showed that the magnitude of the corrections for flux loss depends on wind speed and stable/unstable atmospheric conditions. Corrections are less than 6% for stable conditions and also during unstable conditions if wind speeds are greater than 1 m s-1, whereas correction may reach almost 40% for unstable conditions and low wind speeds."

Furthermore, readers should benefit y knowing some of the details concerning the authors' corrections for water vapor fluxes. Massman and Ibrom (2008: Atmospheric Chemistry and Physics 8, 6245-6259) indicate that the closed-path LE flux corrections are likely to be dependent on humidity, as well as specific to a given EC system and to change over time.

A new paragraph and new references have been included in Section 2, dealing with this issue, page 7, line 8: "It has been recently reported by Ibrom et al. (2007) that water flux measurements with closed-path systems may suffer from considerable low-pass filtering (resulting in a flux underestimation) not taken into account by available tube attenuation correction algorithms, as these neglect adsorption/desorption of water vapor to/from the tube walls (Massman and Ibrom, 2008). These authors pointed out that LE flux corrections are also dependent upon humidity. Massman (2000) showed that the magnitude of the corrections for flux loss depends on wind speed and stable/unstable atmospheric conditions. . .."

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(b) Since the authors follow Moore (1986) they should be made aware of (if they are not already) that Moore's aliasing-related correction is an error and should not be made. Do the authors make include the aliasing correction or not?

No, the aliasing correction is not included. As stated in Section 2.2, page 7, line 6: "Note that Moore's (1986) proposed correction for aliasing was not included since it is wrong (Horst, 2000)."

(5) Page 2688, the paragraph beginning with the discussion of REBS Q-7 – My concern here is that all the major results reported by the manuscript are completely dependent on one measurement of Rn made by one type of net radiometer. The authors need to put this instrument and their closure results into the proper context. Specifically they need to answer how their closure results could have been different they used another type of radiometer or method for measuring Rn. There are some well known biases/discrepancies between different radiometers, as well as 4-way methods versus single net radiometers. How would the authors' ERB values and conclusions be affected or even changed had they used a different approach (or instrument) for measuring Rn?

The authors are aware that the net radiometer used in this site is not the most accurate, and we are also aware that several studies have evaluated the performance of different net radiometers showing deviations in the values measured by the Q7. A new paragraph has been inserted, page 8, line 2: "Some experiments have recently shown the low accuracy of this particular sensor when compared to other (Brotzge and Duchon, 2000; Cobos and Baker, 2003; Kohsiek et al. 2007). Kohsiek et al. (2007) suggest the net radiation is preferably to be inferred from its four components, rather than measured directly. Unfortunately, only three of the components were measured during the SIFLEX campaign and net radiation calculation is not possible. The effect of a hypothetical deviation in the measured Rn values will be discussed in section 3.2." Encouraged by this referee comment, we have added a new section to the manuscript, and a new Table 2, in which the effect of this uncertainty in Rn on the energy balance

closure is analyzed. In section 3.2, page 10, line 18: "As mentioned before, some authors have reported biases/discrepancies when using a Q 7 sensor to measure net radiation. Kohsiek et al. (2007) showed an underestimation during the day by 20-40 W m-2 and overestimation at night by 10-20 W m-2, findings in line with those from Broztge and Duchon (2000). In this paper, we used the equation obtained by Cobos and Baker (2003) to recalculate the Rn values to account for these uncertainties. Results included in Table 2 show a significant deterioration in the energy balance closure."

(6) Page 2691, section 3.4 – The authors analyze their results relative to atmospheric stability (i.e., z/L). Here their basic assumption is that z/L is well measured. But if the EC measurements are underestimating H and/or LE, how well or precisely is z/L known? I do not think the closure-related problems are likely to affect the sign of z/L, but I am not sure how meaningful the upper and lower bounds they use for defining atmospheric neutrality (i.e., -0.01 and +0.01) can be. Again I think the authors should discuss this issue and provide some estimates of the uncertainties involved with their numerical estimates for atmospheric neutrality.

A new paragraph has been added to the end of Section 3.5 dealing with this issue, page 14, line 4: "Note that the stability parameter, ξ, was obtained from the Monin-Obukov (MO) length (Garratt, 1992). Thus, an underestimation of H or LE might yield to a consequent underestimation of ξ since a virtual heat flux density is required to estimate this MO length. For instances, a 20% underestimation in the virtual heat flux density would produce another 20% underestimation in ξ. The consequence would be a light lateral displacement of the plots in Figure 4, with no effect on the main discussion above." Also in Section 3.5: "According to the defined bounds, around 10% of the data correspond to neutral atmospheric conditions, showing EBR values ranging between 0.80 and 0.70."

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