

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

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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:

The instrumentation of the station is not state of the art (not even for 2002). The sonic anemometer has some deficits and was later replaced and the net radiometer is of

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low accuracy (Halldin and Lindroth, 1992; Kohsiek et al., 2007; Mauder et al., 2007). Therefore, the residual of the energy balance closure might even be larger.

The CarboEurope network integrates around 25 forested sites where surface fluxes have been collected for the last 10 years. A variety of instrumentation is used depending of the site, and also some differences in flux computations exist between different groups (although there is a tendency towards more democracy-based agreements on commonly accepted rules). All these data are stored in a common dataset after their corresponding quality tests and are available for the entire scientific community. We agree with the referee that the sonic anemometer was later replaced, however the data before the replacement, and particularly those corresponding to the SIFLEX-2002 campaign, are available in the CarboEurope website and the authors trust the experience of the Finnish colleagues in the measuring and data treatment processes. A sentence referring to this replacement has been inserted: page 5, line 6 “Note that the SATI-3 Sx was replaced some time after the experiment by a Metek USA-1 (Aurela et al. 2004).” The authors also agree with the reviewer that the net radiometer used in this site is not the most accurate, and we are 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.” Also, encouraged by another referee, 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

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day by 20-40 W m⁻² and overestimation at night by 10-20 W m⁻², findings in line with those from Brozto 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.”

Furthermore, the data analysis system by McMillen (1988) is not state of the art and a block average is recommended (Finnigan et al., 2003; Lee et al., 2004) as well as a different rotation method (Wilczak et al., 2001). The reviewer is wondering why the authors have not reviewed the papers of the Finnish colleagues, mainly by J.-P. Tuovinen and M. Aurela (for overview see e. g. PhD thesis by M. Aurela, Finnish Meteorological Institute, Contributions No. 51, 2005, available online), where the shortcomings are also discussed. As well, a footprint analysis of the station is available (Göckede et al., 2008).

Thanks to this referee comment, we realized that some aspects had not been exposed or detailed properly at this point. Of course, we reviewed the papers of the Finnish colleagues, but we agree with the reviewer that specific references should be inserted. This has been corrected in the new version of the manuscript, and some new paragraphs and sentences have been inserted dealing with the concerns stated by the referee, page 6, line 11 “The EC data acquisition was carried out by in-house programs using the program by McMillen (1986) as a basis, but introducing some updates.”, “A more recent planar fit rotation method has been proposed by Wilczak et al. (2001). However, no significant difference between the resulting turbulent fluxes was observed when applying both rotation methods (Tuovinen et al., 2005).” Also new references have been inserted: Aurela et al. (1998, 2001, 2002, 2004) where the reader can see all details related to the different correction techniques applied to the flux data. We trust the expertise of the Finnish colleagues in the processing and treatment of the flux data. Regarding the footprint issue, a new Figure 5a has been included showing the results of the footprint calculations for our dataset. A new paragraph has been added

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to section 3.6, page 15, line 14: “Göckede et al. (2008) concluded that a footprint filter is recommended as additional information in the CarboEurope-IP database to indicate level of representativeness of each stored flux measurements. Half of the sites analyzed by Göckede et al. (2008) experienced a significant reduction in EC data quality under certain conditions. We used equations from Schmid (1994) to determine the far end of the source area in the present work. Figure 5a shows the scheme of this fetch distance for the different wind directions.”

The separation of the friction velocity problem from the stratification problem is not very helpful, because both parameters are connected with the Obukhov length, which does not allow an independent analysis.

We agree with the referee that the friction velocity and the stability parameter are coupled (as stated several times in the text). However, we decided to separate the analysis since many papers still treat them separately, and the non-expert reader can check the influence of both parameters according to his/her necessities.

An analysis of very stable and nighttime data requires a careful analysis of the data quality to enable selection of nonturbulent data. Such an analysis (Foken and Wichura, 1996; Foken et al., 2004; Vickers and Mahrt, 1997) is missing. In the case of no turbulence, the energy is balanced between the net radiation, the ground heat flux and the storage term. The turbulent fluxes cannot be taken into account because of the absence of turbulence and because the fluxes are below the detection limit.

The suppression of turbulence during calm conditions mentioned by the referee was treated as indicated in the new paragraph inserted in Section 2.2, page 6, line 21: “Additional manipulations and corrections were applied to the collected data off-line (Aurela et al., 1998). One of them is related to the suppression of turbulence during calm conditions when the surface layer becomes stable. To avoid this night-time problem original data with $u^* < 0.2$ m s⁻¹ was discarded and gaps were then filled by modeled values using the mean diurnal variation method (Falge et al., 2001).”

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Energy balance analyses like the one presented more or less provide an additional remark on another issue, but the authors have already done this in their paper published in *AgForMet* 149 (2009), 1037-1049.

The paper published in *AgForMet* 149 (2009), 1037-1049 shows the application of an energy balance model to the same boreal forest dataset. For the validation of the results it is necessary to re-calculate the measured surface fluxes to account for the energy imbalance. For this reason only two paragraphs were included in that paper referring just to the overall lack of energy balance closure. No analysis was included related to possible reasons for the imbalance or the effect of the different uncertainties in the final closure. Therefore, the analysis, results and conclusions shown in this paper are completely new and original and the referee can check that none of them were included in the paper mentioned above.

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