

## ***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.**

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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 Effect of the flux footprint In their approach the author’s relate wind direction with surface land cover characteristics, based on this they argue out the effect of the flux

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footprint on the energy balance closure. I feel, a better link could be arrived at by determining a 1 or 2-D footprint or the fetch distance. With the fetch distance the author's can then quantitatively determine if the "source" of the flux includes the river and the bare soil patch. As it is the argument seems largely speculative. This makes it difficult to go with the author's conclusion that "An improvement of 5% is detected after removing wind coming from the soil patch located 100m to the northwest". There is no quantitative evidence that the flux 'emanated' from soil patch. A figure similar to Fig 5 but showing the fetch distances would make a stronger argument. Whilst Fig 5 is good, the graphic may thus allow for misinterpretations. For footprint analysis see (Detto et al., 2006; Hsieh et al., 2000; Kormann and Meixner, 2001; Schmid, 1994; Schmid, 1997; Schmid, 2002; Schuepp et al., 1990; Sogachev et al., 2004)

Following this referee suggestion, a new Figure 5a was elaborated to illustrate the calculations of the fetch distance using the Schmid (1994) model, page 15, line 19: "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 impact of this possible mismatch in footprints was evaluated by checking. . ." Looking at this new Figure 5a, we can ensure whether the "source" of the flux includes the river or the bare soil patch and reinforce the conclusions in Section 3.6.

2 Tables and Graphs a) Table 2 presents interesting data but what I did not get clearly is whether or not the filtering is "cumulative"? For instance, when filtering for thermal stratification is done is friction velocity filtering included or the 3 filtering processes start from the entire dataset? Also clarify this in the paper.

Thanks to this referee comment we realized that this point was a bit confusing, and it has been clarified in the new version of the manuscript. See the new caption of figure 3: "Parameters of the linear regression between the turbulent flux (H+LE) and the available energy, as well as the Energy Balance Ratio (EBR), for the entire dataset, after several independent, non-cumulative filtering processes, and after all the data

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filtering superimposed...”. This aspect has been also specified in the text after every single reference to Table 3; at the end of section 3.5, page 14, line 2: “. . .after filtering and selecting only unstable conditions ( $-0.01 < \xi < -1$ ) from the entire dataset.”, at the end of section 3.6, page 16, line 14: “. . .after removing wind directions located in the fourth quadrant from the entire dataset.”

b) Figure 4: I think some of the values for zeta need a negative sign.

Note that variable in x-axis in left-hand plots already contains a negative sign. Thus, the values in x-axis do not need the negative sign.

c) Labeling the subplots a, b, c, d. . . should be done.

This has been corrected in the new version and subplots have been labeled.

3 References Overall, the authors reviewed relevant literature with regards to the energy balance closure problem. However, I felt that the author’s review/interpretation of the paper by Timmermans et al (2009) presented on page 2692, Line 25-27 is not precise. I recommend that the authors re-read the paper and reconsider their assessment of the paper. The author’s should verify: i) if a 2-D or 3-D approach after the work of Soegaard et al (2003) was used? ii) If, as the author’s state, there was comparison with LAS estimates? It appears to me that Timmermans et al. (2009) combined LAS estimates to with a footprint approach to infer spatially averaged fluxes, the relative contribution of contributing surfaces amongst other things. From Line 25-27 (2692) it appears as if the footprint approach was used to determine the H flux and then compared to H<sub>las</sub>.

After re-reading the paper, the authors agree with the referee that some points had been misunderstood. This has been corrected in the new version, in section 3.6, page 15, line 1: “. . .in Timmermans et al. (2009). These authors applied a two-dimensional footprint approach presented by Soegaard et al. (2003) combined with the weighting function of the LAS, following Meijninger et al. (2002)., to produce area-averages of

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fluxes suitable for validating spatially distributed models that estimate surface fluxes from remote sensing.”

Typo errors that need to be corrected and some rephrasing needed include: Page 2684, Line 26 – add “of” between “. . . balance” and “about”. Page 2687, Line 1 – Rephrase. , Line 14 . . . mast was placed at the site not in the site. Page 2688, Line 20 – May add “points” to read, “. . .valid data points were. . .” Page 2689, Line 10 –Rephrase. Page 2690, Line 11 – Couldn’t understand this, “. . .with the inversion of sign of the net radiation. . .” Rephrase. Page 2691, Rephrase Line 11 – 13, “. . . explains why the. . . to be in calm”. Page 2691, Line 23 “add” in between and discussion. Page 2693, Line 5 – spelling of Campaign, Line 27 replace “de” with “the”. Page 2694, Line 9 – you may improve to “. . . not the case with low frequency. . .” Page 2695, Line 11, you can remove the word “itself”.

All these typing mistakes have been corrected in the new version of the manuscript.

The author’s should also try to clearly outline the objectives of the paper in a single paragraph under the introduction subheading. It seems statements that read “with the aim of. . .” are all over the paper, which affects the readability of the paper.

The authors agree with the referee at this point, and all those statements have been removed from the text. A single paragraph in the Introduction section clearly states the objectives of this work, page 3, line 19: “The objective of this paper is to identify and quantify sources of energy imbalance using data registered at one of the FLUXNET sites located in Sodankylä, Finland. In particular, we focus on the dataset collected as part of the Solar Induced Fluorescence Experiment in the summer of 2002 (SIFLEX-2002). With this goal we expect to contribute to the existing literature on the energy balance closure in general, and over forest sites in particular.”

5 General Comments a) In the introduction of your work, you highlight that this paper is important as it studies energy balance closure issues over a boreal forest. However, in your conclusion there seems to be no reference to this and what the ecosystem

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specific contribution that the paper is.

According to this referee comment an additional paragraph has been inserted at the end of the Conclusions section, page 19, line 12: “Reducing the lack of energy balance in boreal forests might further contribute to a better understanding of their role on climate processes at a global scale due to the vast extensions of the planet that this ecosystem occupies.”

b) As a threshold for  $U^*$  you choose  $0.25 \text{ m s}^{-1}$  and some authors use  $0.3$  and  $0.35 \text{ ms}^{-1}$  (Barr et al., 2006; Herbst et al., 2002). How did your chosen threshold affect your results? You may comment about it in the paper.

A new paragraph has been added at the end of Section 3.4 dealing with this concern, page 12, line 24: “Some authors use  $0.3 \text{ m s}^{-1}$  (Oliphant et al. 2004) or  $0.35 \text{ m s}^{-1}$  (Barr et al. 2006) as a threshold for  $u^*$ . Results included in Table 3 do not change significantly when modifying this value (closure improvements lower than 2% are obtained when using 0.35 instead of 0.25 as a threshold value).”

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 2683, 2010.

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