

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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The paper addresses a pertinent unresolved issue in the eddy flux community, that of the energy balance closure. The paper identifies and analyses potential causes of non-closure of the energy balance over a boreal forest FLUXNET site in Finland. Though the paper does not present new concepts and methods, it provides an analysis perspective that is vital for understanding the energy balance closure problem. In this regard the paper contributes to scientific progress on the energy balance closure problem.

I, however, feel that the paper can be improved by making the following clarifications and/or corrections:

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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 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)

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.

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

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

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

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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_{las}.

4 Typo, language and presentation.

The paper is presented in simple English that is easy to follow and understand though some significant editorial work needs to be done. The paper is well structured, with subheadings for key discussion points. 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”.

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.

5 General Comments

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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 specific contribution that the paper is .

b) As a threshold for U* you choose 0.25 m s⁻¹ and some authors use 0.3 and 0.35 m s⁻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.

Overall, I recommend the paper for publication upon making the necessary corrections and improvements.

To the authors, it is possible that I might have misunderstood your work and should you feel that you want to contact me personally, please feel free to e-mail me on drwasoka@gmail.com

RWASOKA D. T.

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