

Interactive comment on “Evaporation from Savanna and Agriculture in Semi-Arid West Africa” by Natalie C. Ceperley et al.

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Dear O. Mamadou,

Thank you for your comments. I am sorry that you did not find my paper well organized and ready for publication. I will try to benefit from your comments after your close reading of my manuscript to improve it. You are right, that most of this work and my detailed literature review is a few years old, so as AR2 already suggested, I am in the midst of updating the comparison with other studies from the region, including your own. I am glad that you and AR2 could help me with this weakness.

Major Comment: Firstly, the paper is poorly organized, with much extensive content, though not exhaustive and too much interpretation in the results.

C1

While the methodology, in particular the eddy covariance data treatment requires a particular attention to have reliable turbulent fluxes, this was partially presented by the authors and the units of keys variables were omitted. In addition, there was a total confusion in the signification of such variables. For example, the available energy is not the sum of turbulent fluxes (H+LE) but rather the difference between the net radiation and the soil heat flux (Rn-G), see L13, L25, p5; and section 3.2.2.

I will try to make my wording and variable reference more precise and detailed.

Secondly, one of the main points of this paper was in the site comparison; however, basic information about the research sites was lacking. Did both sites have similar soil characteristics? The large differences in soil water content may indicate site differences in soil texture.

I will include more information about soil characteristics.

Also, more information is needed about the flux footprint. What was the fetch? Was the vegetation in particular (the rain fed site) within the flux footprint homogenous? The forest site seems to be located in a very complex topography according the map of the site (Figure 6). How this has been taken into account in the analysis of eddy covariance data?

This was also a comment from AR2. We used a planar tilt correction to correct for the positive average wind speed as described by Oldroyd, H. J., Pardyjak, E. R., Huwald, H. and Parlange, M. B.: Adapting Tilt Corrections and the Governing Flow Equations for Steep, Fully Three-Dimensional, Mountainous Terrain, Bound.-Layer Meteorol., 1–27, 2015.

C2

While I think a flux footprint would be interesting, I am saving it for a future analysis due to certain constraints that I discuss in my reply to AR2.

These aspects are important for understanding and interpreting the results.
Finally, what is the value of the slope?

I'll add a calculation of slope for the different wind directions around the EC stations.

Some of the writings throughout the text may be rewritten in more compact and yet concise style without losing the message they want to convey to the readers.

This is a good comment, I will try to improve the writing.

Some conclusions are drawn without the support of data. Details can be found at the specific comments.

I will try to take into account as many of your suggestions as possible. Your specific comments are very helpful and many of them will greatly improve my work. Thank you for taking the time to read everything so closely and I hope we have the opportunity to compare our work in more detail in the future.

Dr. Natalie Ceperley

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C3