

Interactive comment on “Variations in diurnal and seasonal net ecosystem carbon dioxide exchange in a semiarid sandy grassland ecosystem in China’s Horqin Sandy Land” by Yayi Niu et al.

Anonymous Referee #2

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In this paper, the authors examined the ~4.5 year record of carbon exchange, measured using eddy covariance, over a grassland site in China’s Horqin Sandy land. The authors present the fluxes at diurnal, daily, monthly and yearly intervals, and use principal component analysis (PCA) to try and examine associations of the fluxes, NEE, Rec and GEP, with a whole host of hydrometeorological measurements. Their findings were limited to the associations found using the PCA with little interpretation of the PCA results. The paper, unfortunately, contained few results and insights that would be useful to the ecosystem flux community, except for perhaps the flux measurements themselves. The study lacks any hypotheses or expectations that would help guide the subsequent analysis. For example, one obvious one would be that we would expect the

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seasonal to annual scale fluxes to be controlled by water availability. There are many other hypotheses and ways to analyze the data in the literature, that unfortunately, were not well reviewed either. With a lack of any physical interpretation, we instead learn things like, soil heat flux had a major effect on NEE because the authors blindly use the PCA analysis to tell us something meaningful about the grassland. This result comes from correlation between the met variables and not a mechanistic link. Some suggestions for improvement: 1. Improve the introduction and let it lead to hypotheses that you can test with the data. 2. I have included many suggested references that the authors missed. While many of these, I contributed to, they are still very relevant to this study especially because many sites in the southwest US have a similar summer monsoonal climate with similar amount of rainfall and summer temps. There are other places globally, cited in these manuscripts, that are worth looking at. These studies should prove useful to guiding your analysis and discussion and not simply presenting the data at different aggregation levels. 3. The figures only the present the data at different aggregation levels and provide little insight into what controls the seasonal to annual variation in the C fluxes. 4. I would love to see more on how water (precip, ET, soil moisture) may be controlling the warm season fluxes. 5. There is way too much reporting of data in the manuscript . For example, why is it important to know maximum and minimum values of SHF to the hundredths of W m⁻²?

I’ve also included a detailed text-specific set of comments in the attached, marked up, PDF file.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2020-89/bg-2020-89-RC2-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-89, 2020>.

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