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Biogeosciences Discussions

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 #1

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General comments

Niu et al. report on 5-years of CO2 fluxes from a sandy grassland ecosystem in China's Horqin Sandy Land region. While this paper presents important information on the carbon source/sink activity of a degraded, sandy grassland system, I have concerns about the presentation and interpretation of results. Throughout the manuscript, it is unclear how some interpretations and conclusions are drawn from the presented results, and some results critical to the authors' conclusions are only found in the supplemental information. Below I address several specific concerns.

Specific comments

Results

While the results address an important knowledge gap on the carbon dynamics of a degraded sandy grassland, the presentation is unclear. Re-structuring the results may increase the impact and clarify of this manuscript. In its current state, the results begin with information on meteorological conditions (3.1). However, these results do not appear to be a major part of the authors' conclusions, and, from my perspective as a reader, this disrupts the flow of the manuscript. One way to re-structure the results would be to first present information on annual mean fluxes. This would address the authors' first goal: to quantify annual variation in fluxes. After presenting annual fluxes, the authors could examine seasonal then diurnal variation in fluxes. Finally, the authors could present results on meteorological conditions as possible drivers of dynamics in observed carbon fluxes.

Figure 2

In L244, the authors state "Figure 2 suggests the sandy grassland was a net CO2 source." I do not see clear evidence for this in Figure 2 and it is not clear how the authors made this interpretation. Because Figure 2 depicts seasonal variation in daily CO2 fluxes, it is hard to determine the sign and magnitude of annual mean carbon exchange. To make inferences about the annual source/sink activity of this system, I suggest adding a figure showing cumulative fluxes or a table depicting integrated or annual-mean fluxes. Related, the numbers listed in L244-246 show that GPP was greater than Rec, implying carbon sink behavior. However, because the reported NEE is positive, the authors conclude carbon source activity. This is very confusing and must be clarified. Please define the sign convention used for NEE.

Figure 3.

This figure is clear and provides good evidence in support of the study goals and conclusions. One suggestion would be to add another panel or figure representing annual mean fluxes, or annually integrated fluxes. The authors could then cite such a

figure as evidence of carbon source/sink behavior at the annual scale.

Figure 4.

This is a strong figure, but the interpretation in the main text is unclear. The authors report in L262-266 that NEE showed an absorption peak from 7:30 to 16:30 and that "the rest of the day was characterized by weak carbon absorption." There is no evidence for this. Before 7:30 and after 16:30, positive NEE indicates carbon emissions to the atmosphere. Please clarify. Also, I suggest adding a horizontal line to all figure at 0.0 on the y-axis. This would help the reader to quickly infer the sign carbon fluxes.

Tables 1, 2, and 3

Why is precipitation included in Tables 2 and 3 but not Table 1? One of the major study conclusions is that annual precipitation strongly regulated NEE (Section 5). However, precipitation is absent from the PCA for seasonal NEE (Table 1). The authors should explain why precipitation is not included in Table 1.

Discussion

Throughout the discussion, claims are made with no reference to evidence. For example, this happens in L379 and again in L404-405 and L425-428. These claims would be stronger if they were supported with evidence.

What I find absent in the discussion is an explanation for how drought may have influenced the interpretation of results. The authors note that the study was conducted during relatively dry years (L232-235). I appreciate that the authors considered land degradation as a possible cause of carbon source behavior. However, it would be helpful if the authors explained how interactions between land degradation and drought make it hard to attribute the observed low productivity to a single driver.

Throughout the manuscript, the definition and sign convention of NEE is unclear. This happens in the results (L244-246) and in the discussion (L415) when the authors write that NEE increased with increasing light intensity. Is this a typing error? Should this be

GPP instead of NEE?

L413: I do not see evidence of daytime CO2 uptake in autumn (Fig. 4c). Please clarify.

L448-450: The observed dependency of Rec on soil water is consistent with existing theoretical and empirical evidence that episodic rain events drive pulses of soil respiration in semiarid regions (Huxman et al., 2004; Roby et al., 2019; Sponseller, 2007).

Technical corrections

L22: please specify that these are CO2 flux measurements.

L166: Check the alignment of this text.

Supplemental material

L10: What is diurnal-scale mean value? Does this refer to the daily mean value?

Fig. S3. Panel e appears to show daily mean values for each year. Despite similar captions, panel e in Figs. S1 and S2 appear to show daily mean values averaged across years. Please clarify.

References

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