Summary

This manuscript presents 5 years of eddy covariance data to quantify the carbon dynamics of a semiarid grassland ecosystem in China's Horquin Sandy Land. The authors examine variation in NEE, GPP, and Rec at several scales of aggregation, and examine the response of these fluxes to environmental drivers. The revised version now includes key references to the relevant literature. I appreciate that the authors have added hypotheses and goals to guide the analysis. However, these hypotheses are not formulated in a way that is testable given the available data. Moreover, the authors continue to draw inferences that are not readily supported by the evidence. With substantial modification of the study motivation, hypotheses, and the analysis/interpretation of data, these issues could be resolved in a way that advances understanding of carbon-water relations in this ecosystem.

Major comments:

A major aim (Goal 2) of this paper is to "explore the effects of changes in precipitation amount and frequency on seasonal and annual NEE, GPP, and Rec." Throughout the paper, the authors conclude that seasonality in fluxes was related to precipitation event size and frequency (e.g. L 28-29). It is not clear which evidence the authors use to draw this conclusion. Figure 7 shows that spring total precipitation explains some of the variation in spring carbon fluxes, but also shows that the relationships are weak for other months, such as summer, when peak GPP occurs.

Goal 2 is followed by the hypothesis that "an effective precipitation threshold would exist at around 5 mm, which could alter soil moisture in deeper layer and affect carbon fluxes in the sandy grassland ecosystem." Unfortunately, this hypothesis is not tested. In Figure 2, the authors explore how precipitation size translates into dynamics in soil moisture at various depths, but no connection is made to carbon fluxes.

The motivation and rationale for this study mostly relies on the novelty of the dataset. This happens several times in the introduction alone (L 64-66; L91-96; L 127-129). The authors have data to make a significant contribution, but the motivation for the study is not strongly articulated in the introduction.

There are sections in the results where the presentation of data is unclear, which makes it difficult to understand and interpret the study findings. For example, there are inconsistencies in the sign convention of Rec (Figs. 3-5). Additionally, in some places it is unclear how the data were used to generate figures (Figure 6). Below I discuss specific aspects of this.

Specific comments

Abstract

In the abstract, the authors conclude the ecosystem was an annual carbon source, and then present two possible explanations for why: because of drought, or due to a history of land degradation. What I find problematic is that the abstract does not build toward a conclusion that is supported by evidence. Instead, two possible explanations are given that are untestable with the data. There is some evidence to support the conclusion that "drought [as quantified by low annual precipitation] decreased carbon sequestration (Figure 6-7), but there is no evidence in the paper to examine how land degradation influences carbon fluxes.

L 26-27: The statement "Annual precipitation had the strongest effect on annual NEE" is vague. I suggest modifying this sentence or combining it with the next one. For example, "Grassland carbon sequestration increased with increasing precipitation, as indicated by the dependency of NEE on annual precipitation."

L 27-28: The authors write "In the spring, NEE increased with increasing Tsoil and increasing precipitation. Is this a typo? Figure 7 shows that NEE actually decreased with increasing precipitation.

L 28-29: Please provide evidence for this.

L32-33: This was written in line 23.

Introduction

I am glad to see hypotheses in the revised version. However, in its current form, the first hypothesis cannot be tested with the available data. The analysis does not allow the authors to test for the effect of past land degradation on carbon sink activity. One way to rephrase this hypothesis is "we hypothesized that due to the strong dependence of GPP on precipitation in this ecosystem, years with low precipitation will be associated with carbon source activity."

Recommend the introduction be restructured to build toward a knowledge gap or hypothesis that is testable with the available data. Perhaps the introduction could be modified to explain why this study is necessary. Do the authors expect that what has been documented in other water-limited regions will not apply in the Horquin Sandy Land? If so, why?

L 56-61: I appreciate that the authors cite existing literature on carbon-water relations in drylands. However, instead of reporting previous findings, the introduction should synthesize content, identify a clear knowledge gap, and present hypotheses or study goals to address that gap. It is not clear how reporting prior work builds toward your study motivation. For example, what is the purpose of writing "evapotranspiration was a better proxy for the water available for NEE" if the authors then chose to use precipitation instead of ET to examine the response of NEE to water?

L 62-64: This is a good reference to the existing literature. Because drylands show a variety of source/sink behavior, there is need to study the Horquin Sandy Land.

L 101-105: There is a large body of work on how precipitation pulses drive carbon and water fluxes in semiarid regions, see below. See especially Figure 1 in Huxman et al. (2004).

L 105-108: Suggest reading Chen et al. (2009), who examined thresholds in a semiarid steppe ecosystem in Inner Mongolia: “The distinct responses of ecosystem photosynthesis and respiration to increasing pulse sizes led to a threshold in rain pulse size between 10 and 25 mm, above which post wetting responses favored carbon sequestration” (Chen et al., 2009).

L 113: Key reference missing: Noy-Meir (1973).

L 121-124: This statement about summer rainfall wets shallow soil layers seems to conflict with the statement in L147 that rain events greater than 5 mm wet deep layers.

Results

In the revised version, it is good to see that in section 3.1 the authors focus on key variables that drive observed dynamics in carbon fluxes. I appreciate the addition of panel d in Figure 3 to show annual total NEE, GPP, and Rec, and that Figure 4 now includes multiyear means in each panel. These figures now provide evidence to support the conclusion that the ecosystem was a carbon source at the annual scale.

Figures S1-S5: what do the error bars represent?

Figure S3: Why is SHF presented? It is not referenced elsewhere in the paper. Suggest remaking this figure with Tsoil. Diurnal patterns in Tsoil may help explain diurnal patterns in Rec, assuming soil respiration is a major component of Rec in this system.

Table 1: There should be a column header for “number of events” above “Magnitude of precipitation event”. Also, since in L 271 the authors refer to the annual number of events, Table 1 should have a row with the annual amount of rainfall events for each size class.

Figure 2: It is not clear how this figure is used to draw conclusions. It shows that the size of a rain event influences dynamics in soil moisture at various depths. No connection is made to carbon fluxes, which is the main point of this paper. Additionally, the caption for Fig. 2 should define what the dashed line indicates.

Figure 3: Inconsistencies remain in the sign convention of carbon fluxes in figures. In Figure 3a-e, Rec is often negative, but it expressed as a positive cumulative total in panel f. This occurs again in Figures 4 and 5 whereas Rec fluxes are “large” when positive. Additionally, I suggest adding a zero line in Figure 3 (as in Fig. 5) to help the reader see the sign of carbon fluxes. There appear to be periods of negative GPP (e.g. 2015 DOY 250). Is this an error? Please explain.

Figures 4 and 5: what do the error bars represent?

Figure 6: It is unclear which data were used to make this figure. The caption says this is the relationship between annual precipitation and carbon fluxes. There are too many data points for these to be annual values. Also in L 346, if these are annual fluxes, why is precipitation maxing out at 100 mm, when annual precipitation ranged from 212-351 mm (L270)?

Figure 7: Typo for Autumn.

L 280: “Ecological links” is unclear; please explain.

L 283: Please provide a number for annual precipitation during a “normal year” to give context for how dry the experiment period was.

L325. I disagree that the diurnal pattern of Rec in summer was similar to that during spring. The diurnal patterns are essentially opposite. For b, why does peak Rec occur at night, instead of during the day when temperatures are highest? Is this related to the heating issue described in L436? Similarly, in Fig. 5d there are two peaks in NEE, and a minimum during the day. Does this pattern indicate some level of vegetation activity?

Discussion

L411: Specify as before the summer growing season.

L419-420: Please add a reference to figures to provide evidence in support of this statement. For example, Figure 8 shows that that these conditions increased carbon uptake (more negative NEE) because the sensitivity of GPP to Tsoil and moisture was greater than that of Rec (similar to the text in L 459-461).

L 429-430: “NEE decreased with increasing light intensity during the day.” Are you referring to similar diurnal patterns of Rnet and NEE, or a light response of NEE? It is not surprising that NEE tracks the pattern of solar energy. I do not see why this result was included in the discussion.

L 435: Did the authors attempt to correct for these heating effects? How has this potential for sensor error influenced the interpretation of results? I think this warrants more discussion, given the study’s emphasis on source/sink activity.

L 479-483: Instead of offering event size and frequency as a possible explanation for dynamics in NEE, what if you used data to test this idea? This would provide a direct test of the goal and hypothesis stated in the introduction. One way to test this is to calculate the mean or integrated fluxes corresponding to the times during which rain events of various sizes occurred. For example, in Table 1, rain events are grouped by size. Perhaps you could find a way to calculate the corresponding carbon dynamics for each of these rain groups. Such an exercise could inform results statements, such as “springs with a greater amount of 10-15 mm rain events had greater GPP than springs with fewer 10-15 mm rain events.” Alternatively, you could order the seasons by integrated flux (e.g. total GPP) and rank years by number of large rain events, and test for a relationship between the two.

At a minimum, I think the authors should add precipitation to Figure 4. If a total precipitation bar was added to each of the years in the four subpanels, it would show if patterns in seasonal precipitation were related to variation in flux rates.

References

Huxman, T. E., Snyder, K. A., Tissue, D., Leffler, A. J., Ogle, K., Pockman, W. T., et al. (2004a). Precipitation pulses and carbon fluxes in semiarid and arid ecosystems, 254–268. https://doi.org/10.1007/s00442-004-1682-4

Noy-Meir, I. (1973). Desert Ecosystems: Environment and Producers. *Annual Review of Ecology and Systematics*, *4*(1), 25–51. https://doi.org/10.1146/annurev.es.04.110173.000325

Chen, S., Lin, G., Huang, J., & Jenerette, G. D. (2009). Dependence of carbon sequestration on the differential responses of ecosystem photosynthesis and respiration to rain pulses in a semiarid steppe. *Global Change Biology*, *15*(10), 2450–2461. https://doi.org/10.1111/j.1365-2486.2009.01879.x

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