26 October 2020

Dr. Trevor Keenan
Editor-in-Chief
Biogeosciences

RE: Submission of the revised manuscript (No. **bg-2020-89**): Variations in diurnal and seasonal net ecosystem carbon dioxide exchange in a semiarid sandy grassland ecosystem in China's Horqin Sandy Land.

Dear Dr. Trevor:

Thank you very much for your assistance in the review of our manuscript and for your invitation to resubmit our manuscript. We have revised the manuscript carefully according to reviewers' comments. We have also had this revised manuscript edited by Mr. Geoffrey Hart (ghart@videotron.ca/geoff@geoff-hart.com), an English science editor with more than 30 years of experience, to ensure that the quality of the language will be acceptable. Please contact him if necessary to confirm that he has performed this work or if you have any questions about the nature of the work that he has done.
Our detailed responses to comments are presented in the remainder of this letter. All of revisions have been highlighted in red in the manuscript.

Responses to Reviewers

Reviewer #1

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 R_{ec} 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 R_{ec}." 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.

We have revised Goal 2 as follows: "explore the effects of changes in total precipitation and pulse size on NEE, GPP, and R_{ec}" (lines 143-144 in the revision). We have added Figures 2B, 2C, and 4 to provide data that let us test this goal. To further explore the relationship between summer precipitation and NEE and its components, we added the total seasonal precipitation to Figure 4 for each of the years in the four subpanels to show the relationships with precipitation. Figures 4 and 7 show that NEE, R_{ec}, and GPP were strongly controlled by the total summer precipitation (lines 32-35, 362-366 in the revision).

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.
We have added Figures 2B and 2C to provide data that can be used to test the hypothesis (lines 370-385 in the revision).

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.
We have deleted the duplicated descriptions of the novelty of the dataset, and have added our motivation for the study in the Introduction (lines 65-70, 89-93, and 125-127 in the revision).

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 R_{ec} (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.
We have revised the colors used in Figures 3-6 to be consistent with the sign convention for R_{ec} and have confirmed that the NEE and GPP sign conventions are correct throughout the revision. We have clarified that Figure 6 shows the relationship between total monthly precipitation and total monthly NEE, R_{ec}, and GPP for the years with a complete dataset (2015, 2016, and 2018) (line 1074 and Figure 6 in the revision).

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.

We have deleted the description of the effect of land degradation on carbon sink activity of grassland ecosystems. As you noted, our present data cannot support this conclusion.

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."
On the advice of our English editor, we added your suggested sentence, with some minor modifications (lines 28-30 in the revision).

L 27-28: The authors write "In the spring, NEE increased with increasing T_{soil} and increasing precipitation. Is this a typo? Figure 7 shows that NEE actually decreased with increasing precipitation.
We had intended to say that the magnitude of NEE increased, so we have changed "increased" to "decreased" (line 31 in the revision).

L 28-29: Please provide evidence for this.
As we mentioned above, we have revised the major goal and have added evidence that can be used to test it (lines 32-35, 362-366, 379-384, Figures 2B, 2C, and 4).

L32-33: This was written in line 23.
We have deleted the duplicate description.

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."
We have deleted the first hypothesis; as you note, it cannot be tested with the available data. We have then rephrased this hypothesis according to your comment, subject to some revisions by our English editor (lines 136-137 in the revision).

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?
We have revised the Introduction to build toward the knowledge gap we designed the study to fill and describe a hypothesis that is testable with the available data. As noted earlier, we have deleted the description of the impact of land degradation on carbon fluxes, and focused on how the total precipitation amount and pulse size affected carbon fluxes in the Horqin Sandy Land. We have added a description of why this study is necessary (lines 65-70, 89-93, and 125-127 in the revision). Based on the existing research in other water-limited regions, we have proposed the hypothesis that the key

factors will be similar in the Horqin Sandy Land, and have verified this hypothesis using the data we collected (lines 136-137, 140-143, 144-147 in the revision).

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?
Citing previous studies in the Introduction is commonly done to provide context (what is already known, which leads to what is not known). However, we have removed our descriptions of previous findings in response to your comment, and have revised the description based on your comment (lines 59-70 in the revision).

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.
We have carefully read the paper and have cited the relevant results (lines 59-62 in the revision).

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).
We have carefully read the paper and Figure 1, and have revised our description of how precipitation pulses drive carbon and water fluxes in semiarid regions (lines 98-100 in the revision).

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).
We have carefully read the paper. Chen et al. (2009) studied the responses of photosynthesis and soil respiration under different water gradient treatments and compared the differences. However, our study was conducted under natural precipitation, similar to the study of Hao et al. (2010). Therefore, we have referred to the method of Hao et al. (2010) to support our belief that the effective precipitation threshold for changing the C flux in the sandy grassland ecosystem was 5 mm. Figure 2C supports this assumption.

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

Hao, Y. B., Wang, Y. F., Mei, X., and Cui, X. R.: The response of ecosystem CO_2 exchange to small precipitation pulses over a temperate steppe. Plant Ecol., 209, 335-347. https://doi.org/10.1007/s11258-010-9766-1, 2010.

L 113: Key reference missing: Noy-Meir (1973).
We have added the reference (lines 109, 865-866 in the revision).

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.
We have revised the description to clarify our meaning (lines 117-122 in the revision).

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.
Thank you.

Figures S1-S5: what do the error bars represent?
We have added the meaning of the error bars (i.e., standard errors) for each supplemental figure (lines 15-16, 21-22, and 28-29).

Figure S3: Why is SHF presented? It is not referenced elsewhere in the paper. Suggest remaking this figure with T_{soil}. Diurnal patterns in T_{soil} may help explain diurnal patterns in Rec, assuming soil respiration is a major component of R_{ec} in this system.
We have revised this Figure to use T_{soil}, as you suggested (Figure S4 in the supplement).

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.
We have deleted Table 1, because we changed one of our major goals to "explore the effects of changes in total precipitation and pulse size on NEE, GPP, and R_{ec}".

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.
We have added Figures 2B and 2C to illustrate the response of carbon fluxes to precipitation pulses in different seasons (lines 370-385 in the revision), and also have defined the dashed line (lines 1045-1046 in the revision). Figure 2C tests the significance of differences in fluxes before and after an effective pulse to support our conclusion that these pulses were significant.

Figure 3: Inconsistencies remain in the sign convention of carbon fluxes in figures. In Figure 3a-e, R_{ec} is often negative, but it expressed as a positive cumulative total in panel f. This occurs again in Figures 4 and 5 whereas R_{ec} 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.
As we mentioned above, we have revised the colors in Figures 3-6 to be consistent with the sign convention for R_{ec} and have checked that the NEE and GPP sign conventions are correct throughout the revision. We have added a zero line in Figure 3. There were no negative GPP values in Figure 3, although the GPP value gradually decreases to a value near 0 at the end of the growing season. To display the data more clearly, we increased the size of the symbols, and that may have caused part of the symbol to extend below the zero line at GPP values near 0.

Figures 4 and 5: what do the error bars represent?
We have added the meaning of the error bars (i.e., standard errors) in the revision (lines 1062-1063, and 1073 in the revision).

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)?
We have revised the caption as "Relationship between total monthly precipitation (PPT) and monthly net CO_2 flux" (lines 1074 in the revision), because we only had complete observations in 3 years, which was too little data to perform statistical tests. Instead, we have used the monthly data from 3 years to explore the influence of precipitation on carbon fluxes.

Figure 7: Typo for Autumn.
We have corrected the typo.

L 280: "Ecological links" is unclear; please explain.
We have revised "Ecological links" as "the ecosystem's carbon absorption and emission processes" (lines 277-278 in the revision).

L 283: Please provide a number for annual precipitation during a "normal year" to give context for how dry the experiment period was.
We have added the value for annual precipitation during a "normal year" (lines 281-282 in the revision).

L325. I disagree that the diurnal pattern of R_{ec} in summer was similar to that during spring. The diurnal patterns are essentially opposite. For b, why does peak R_{ec} 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?
We have revised the description according to your comment (lines 320-321, 328-329, and 335 in the revision), and have added an explanation for why peak R_{ec} occurs at night in the summer (lines 458-467 in the revision). The reason for the two peaks in NEE (Fig. 5d) may be heating effects in the open-path infrared gas analyzer (lines 469-

487 in the revision).

Discussion

L411: Specify as before the summer growing season.

We have added "summer" in the growing season (line 436 in the revision).

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 T_{soil} and moisture was greater than that of R_{ec} (similar to the text in L 459-461).

We have cited the relevant figures to provide evidence in support of this statement (lines 447-449 in the revision).

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.

What we wanted to express was that NEE responded to light. Based on your comment, we have moved this description to the Results (lines 325-328 in the revision).

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.

According to Goulden et al. (2006) and Burba et al. (2008), yearly estimates of NEE may be significantly biased toward CO_2 uptake in cold-climate ecosystems, so we attempted to correct for these heating effects (lines 216-217 in the revision). We have added a description of how the self-heating effect may have affected the results in the Discussion (lines 470-487 in the revision).

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.

We tried to analyze the mean fluxes corresponding to the times during which rain events of various sizes occurred. However, because of the uneven distribution of precipitation in our semi-arid area, there were overlaps between events with different amounts of precipitation, so the effects of different levels of precipitation size on carbon flux could not be accurately determined based on the data we collected. Therefore, we have modified the main goal to a goal that could be tested in the Introduction: to explore the effects of changes in total precipitation amount and pulse size on NEE, GPP, and R_{ec}. We have revised the description of how precipitation pulses affected the carbon fluxes and have added Figures 2B and 2C to provide the data (lines 32-35, 362-366, 379-384, Figure 2B and 2C, and Figure 4 in the revision).

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.
We have added a bar for total precipitation for each of the years in the four subpanels of Figure 4 to show the relationship between seasonal precipitation and carbon fluxes (Figure 4, lines 362-366 in the revision).

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
Yan, L., Chen, S., Xia, J., & Luo, Y. (2014). Precipitation Regime Shift Enhanced the Rain Pulse Effect on Soil Respiration in a Semi-Arid Steppe. PLoS ONE, 9(8), e104217. https://doi.org/10.1371/journal.pone.0104217.
Thank you for directing our attention to these references. We have carefully read these papers and have cited the most relevant ones (highlighted in red in the revision).

Thanks for your efforts to improve our manuscript. We hope that our replies and the resulting changes will be satisfactory, but we will be happy to work with you to resolve any remaining issues.

Reviewer#3:

General comments:

I reviewed this manuscript for the first time. I realized it has been improved a lot after taking the reviewers' comments in the first round. But I think some further improvements remain required before being publishable. See my advices.

1. L19-21,"Sandy grasslands are sensitive to climate change, yet the magnitudes, patterns, and environmental controls of their CO_2 flows are poorly understood", the expression is not backed up by the current literature. Generally, there are lots of studies of carbon fluxes over sandy grasslands worldwide, but for some specific regions, the expression may hold.

We have revised this to clarify that this is true for some specific regions, such as our study area (lines 23-24 in the revision).

2. L136, 'CO_2 dynamics' -> 'CO_2 fluxes'

We have revised this as "CO_2 fluxes" (line 129 in the revision).

3. L98, 'quantified the temporal variation', you actually quantified the CO_2 fluxes over different timescales.

We have revised "quantified the temporal variation" to "quantified the CO_2 fluxes over different timescales" (line 131 in the revision).

4. L183-187, the description of precision and accuracy are very confusing, μmol/m2/s is the unit of flux, but here the authors are evaluating the raw measurements of IRGA; also, the precision and accuracy should be on the raw measurement of 0.1 s timescale for a given 10 Hz EC. It is strange to discuss the topic for raw measurement but on the timescale (30min) for the averaged flux.

We apologize for not clearly describing the processing interval and measurement interval; we calculated 30-min means using a 10 Hz measurement interval, so we have revised the description to clarify this (lines 198-202 in the revision).

5. L165, the measurement sections were messed up. You could describe meteorological measurements soon after the experimental site, then describe EC measurement and flux calculation, quality control etc. That way, the method section may flow better.

Based on your comment, we have revised the order of the measurement sections (lines 172-191 in the revision).

6. L219, the EC system you used is an open path one, I am not aware of any requirement of lag correction. You need to detail it.

Time lags should be always compensated. The only exception is when an open-path analyzer is located very close to an anemometer or overlapping with it. However, this configuration is not recommended due to the important flow distortion effects that result from the presence of the analyzer. Note that the instruction manual for the EddyPro software supports this response (https://www.licor.com/env/support/EddyPro/home.html).

7. L225, avoid using not so necessary description: you do not need to remove data during power failure as no data can be stored as the data logger is also dead then, right?
We have removed this description.

8. L226, it is more intuitive if you use umol/m2/s as the unit of carbon flux.
We have revised the unit of carbon flux to be "umol m^{-2} s^{-1}" (lines 222-223 in the revision).

9. L229-231, The definition of daytime or nighttime NEE seems not useful and breaks the flow of the paragraph. As I reviewed it progressively, this sentence can be moved to the following paragraph for data gap filling.
Based on your comment, we have moved this sentence to the gap-filling paragraph (lines 228-230 in the revision).

10. L249-251, you may use a graph with Rn-G as x-axis and H+LE as y-axis to show the energy closure, which can be part of the supplementary information.
We have added the graph you requested to show the energy closure as Supplementary Figure S1.

11. Fig.3-5, the y-axis can be named as 'CO_2 fluxes'.
On the advice of our English editor, we have revised the axis to "CO_2 flux". (Fig. 3-5 in the revision).

12. L355, The discussion needs further improvement. For 4.1, a more accurate subsection title is required. This subsection is very long, but the information is very divergent. So is the second part of the discussion. The authors may re-write the discussion. The authors can consider what to discuss before writing, e.g., you can have a subsection with a title like 'comparison with other arid grassland ecosystem', in which you can discuss if your finding is different from others and what new knowledge you can bring. Also, this is a data driven research, a possible limitation of the study can give the readers some knowledge to how much degree the conclusion is subject to some uncertainty, e.g., data quality or data treatment. I suggest the authors articulate the discussion in a clearer way rather than lay them out like a twin of the result section.
We have added subsection titles in the Discussion to divide the descriptions as you suggested (lines 403, 433-434, 492, and 518 in the revision). According to your comment and reviewer 2's comment, we have deleted the descriptions that are not supported by existing data in the Discussion and have added possible explanations for some main results (lines 447-449, 458-467, and 469-487 in the revision).

Thanks for your efforts to improve our manuscript. We hope that our replies and the resulting changes will be satisfactory, but we will be happy to work with you to resolve any remaining issues.

Sincerely,

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