7 August 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 nearly 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 Editor

As you can see from the referee's reviews, both highlight issues with your manuscript that are unlikely to be addressable with a straightforward review. In particular, both referees noted a lack of depth in the analysis, and suggested several points of improvement.

I also agree with reviewer 2's assessment that the field has moved on from the point where a straightforward presentation of the observations is sufficient to merit publication, and instead requires hypotheses, depth and novel insights. This is at least the case for Biogeosciences, with other journals more suited to publishing data papers (see, e.g., https://www.nature.com/articles/s41597-019-0119-1). Your response to reviewer 2's comment that a deeper analysis is needed, where you argue that "it is still important for researchers to provide basic data for regions that have not yet been described well" is therefore a concern.

According to your and reviewers comments, we have added a specific statement of our research hypotheses and goals in the Introduction (lines 139-151 in the revision) and added a deeper analysis on the relationships between environmental factors and CO_2 flux in the Result and Discussion (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

Responses to Reviewers

Reviewer #1

General comments:

Niu et al. report on 5-years of CO_2 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:

1. 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.

Although re-structuring the Results section has some advantages, the meteorological conditions (3.1) provide essential context for understanding our results, as they are the primary factors that drive CO_2 fluxes in the sandy grassland. Therefore, we have retained the original section 3.1, but focus our analysis on the environmental factors that are drivers of the observed dynamics of the carbon fluxes (lines 264-294 in the revision). We then present the annual mean fluxes (lines 296-310 in the revision), then examine the seasonal and diurnal variation of the fluxes (lines 311-341 in the revision). Finally, we analyze the responses of the CO_2 fluxes to changes in meteorological conditions as possible drivers of the observed carbon fluxes (lines 343-376 in the revision).

2. Figure 2

In L244, the authors state "Figure 2 suggests the sandy grassland was a net CO_2 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 CO_2 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.

We have added Figure 3f to present the annual cumulative NEE, GPP, and R_{ec} and to show the net source results more clearly (lines 297-300, Fig. 3f in the revision). We apologize for typing incorrect values for GPP and R_{ec}, which incorrectly caused GPP to be greater than R_{ec}. We have revised the values of the GPP and R_{ec} and have checked all other numbers throughout the revision to prevent other errors (lines 298-299 in the revision). In response to your comments, we have defined the sign convention we used for NEE (lines 246-248 in the revision).

3. 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.
We have added another panel to present the annual mean fluxes (Figure 4 in the revision).

4. 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.
We have revised the description to clarify our meaning (lines 314-315 in the revision). We have also added a horizontal line to all graphs at 0.0 on the y-axis (Fig. 5 in the revision).

5. 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.
In response to Reviewer #2, we have removed the PCA results for seasonal NEE, GPP, and R_{ec}, and focused on the impact of precipitation and soil water content on the CO_2 flux at seasonal and annual scales, because precipitation is the factor that most strongly affects the CO_2 flux in arid and semiarid regions (lines 343-376, 395-403, 449-504 in the revision).

6. 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.
We have added references to support our claims in the Discussion. These are highlighted in red in the revision.

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.
We have added an analyses of the relationship between annual precipitation and the NEE, GPP, and R_{ec} in the Results (Lines 343-350 and Fig. 6 in the revision), and have explained how the precipitation affected the NEE, GPP, and R_{ec} in the Discussion (Lines 449-461 in the revision). We have also noted (lines 406-409 in the revision) that although we did not quantify the degree of degradation of the study site, our results suggest that the site has not yet recovered sufficiently to become a net sink.

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?
We have revised the values of GPP and R_{ec} and checked throughout the revision to ensure that they are correct (lines 298-299 in the revision). We have defined the sign convention used for NEE (lines 246-248 in the revision) and have revised the description in the Discussion to agree with this convention (lines 429-431 in the revision).

L413: I do not see evidence of daytime CO_2 uptake in autumn (Fig. 4c). Please clarify.
We have revised the description to correct this error (lines 433-434 in the revision).

L448-450: The observed dependency of R_{ec} 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).
Thank you for bringing these papers to our attention. We have revised the description to include a citation of these papers (lines 498-500 in the revision).

Technical corrections

L22: please specify that these are CO_2 flux measurements.
We have added that these are CO_2 flux measurements (line 22 in the revision).

L166: Check the alignment of this text.
We have revised the alignment of the text (line 218 in the revision).

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.
We have revised Fig. S1 (e) and Fig. S2 (e) to show the daily mean values for each year in order to more intuitively display the variations in these environmental factors during the whole study period (lines 10-11, and 15 in the supplement).

Reviewer #2

General comments:

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, R_{ec} 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 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.

We have revised the Introduction to include a specific statement of our research hypotheses and goals (lines 139-151 in the revision). Based on your comment, we have also eliminated the PCA analysis and focused on the relationships between environmental factors and our results. We believe that our results are important enough to publish because so little data exists for our study area. We have noted this in lines 127-129 in the revision.

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.

Thank you for your suggested references. We have carefully read the literature that you recommended, and have added citations for the relationships between precipitation, soil water content, and CO_2 flux in different seasons based on these papers (lines 97-134, 139-151, 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

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.

We have added analyses and a discussion of the effects of precipitation and soil moisture on the seasonal and annual CO_2 flux (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

4. I would love to see more on how water (precip, ET, soil moisture) may be controlling the warm season fluxes.
We have added an analysis of the impacts of precipitation and soil moisture on fluxes during the warm seasons (lines 357-358, 368-373, 476-494, Fig. 7-8 in the revision).

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}?
We have removed the least important data and retained only the most important data that explain the fluxes (lines 264-271, 282-294 in the revision).

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

We have made the changes you suggested, subject to revision by our English editor.

Page 2:
Number1: affected implies causation, but the analysis only shows correlation.
We have revised the description (lines 26-27 in the revision).

Number2: Could have it been because precipitation was below normal?
Precipitation was below normal, so you are correct that this may explain why the sandy grassland was a net CO_2 source at an annual scale (lines 26-27, 395-403, 449-461 in the revision).

Page 3:
Number 1: Drylands add up because of their vastness, but by themselves (on a per unit area basis) are not considered a potentially large area for long-term carbon sequestration. From what has been shown, dryland C cycling contributes a lot of the interannual variability of global terrestrial C flux (fast response), but long term sequestration potential has not been borne out (slow response. See Poulter et al. and studies that have followed that up over regions like Australia.
We have revised our description to account for the paper by Poulter et al. and other studies in arid and semiarid areas (lines 47-49 in the revision).

Number 2 and 3: This is overlooking a large pool of work in the last 10 yrs. Some of which should certainly be mentioned and considered throughout this paper. Please see (to name a few):
From Southwest US, which has a very similar summer monsoon climate that should be very apropos to your study.
Biederman, Joel A., et al. "CO_2 exchange and evapotranspiration across dryland ecosystems of southwestern North America." Global Change Biology 23.10 (2017): 4204-4221.
Scott, Russell L., et al. "The carbon balance pivot point of southwestern US semiarid ecosystems: Insights from the 21st century drought." Journal of Geophysical Research:

Biogeosciences 120.12 (2015): 2612-2624.
Scott, Russell L., et al. "Effects of seasonal drought on net carbon dioxide exchange from a woody-plant-encroached semiarid grassland." Journal of Geophysical Research: Biogeosciences 114.G4 (2009).
Biederman, Joel A., et al. "Terrestrial carbon balance in a drier world: the effects of water availability in southwestern North America." Global Change Biology 22.5 (2016): 1867-1879.
Kurc, S. A., & Small, E. E. (2007). Soil moisture variations and ecosystem-scale fluxes of water and carbon in semiarid grassland and shrubland. Water Resources Research, 43(6).
Petrie, M. D., et al. "Grassland to shrubland state transitions enhance carbon sequestration in the northern Chihuahuan Desert." Global Change Biology 21.3 (2015): 1226-1235.
Biederman papers, Scott, Litvak, Bowling, Wagle, Aussie papers From Australia. See papers from Beringer, Cleverly, Eamus...
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). We have added a summary of some of the studies that have been carried out in arid and semiarid regions according to your comments (lines 51-67 in the revision).

Number 4: what is a sandy land?
We have added a definition of the meaning of "sandy land" (lines 78-80 in the revision).

Number 5: citation?
We have added a citation (lines 86 in the revision).

Page 4:
Number 1: can't tell from the reference list which papers these are because there are no years in the listing.
We have added the years in the listings and checked throughout the revision (lines 724-726 in the revision).

Number 2: citations to Scott et al. 2015, also Biederman et al. 2015.
We have cited both papers (lines 97-98 in the revision).

Number 3: Austrailan papers by cleverly, beriinger.
We have cited both papers (lines 98-100 in the revision).

Number 4 and 5: how is this in contrast with "concurrent decreases"? see Scott et al., Biederman et al. 2015
We have removed our description of the sensitivity of GPP and R_{ec} to precipitation and have revised this to focus on the amount of precipitation in terms of its impact on ecological processes in the context of our hypotheses (lines 98-105 in the revision).

Number 6: of the "sandy grassland" in this region or grasslands underlain by sandy soils more globally?

We have revised this as "of the sandy grassland" in this region (lines 129 in the revision).

Number 7: Given these pretty goals limited to mainly data reporting. Ecosystem flux science has moved beyond this type of paper and into data interpretation and contextualization. It would be better to put these results into context of what is known already about the seasonal to interannual sensitivity elsewhere. This could be done either by bringing in these expectation learned from previous studies and testing those, or by pulling in data from other sites or syntheses like Fluxnet 2015.

We agree with your point of view that ecosystem flux science has moved beyond this type of paper, it would be better to integrate and analyze data from multiple sites and which could be used as a research focus for the next paper. This comment is great helpful to our future research. However, it would take a long time to obtain and collate these data, and there is little research on CO_2 fluxes in degraded sandy grassland ecosystems, so the impact mechanisms are not clear. We have therefore used the existing data in deeper analysis to obtain preliminary results for CO_2 fluxes and identify the dominant environmental factors in the sandy grassland of our study area. This will provide an empirical basis for future theoretical research at multiple sites.

Page: 5

Number 1: Given the dominant controls of water in semiarid regions and that these P values and summer temps are very similar to the monsoon region in N. America, there is even more reason to bring in some of these results (found in the papers I listed above) in the Introduction and discussion.

We have added some description of the dominant controls of precipitation in the Introduction and Discussion (lines 97-127, 139-151, 449-504 in the revision).

Number 2: zonal soil？

We have clarified that this is the dominant regional soil type (lines 167 in the revision).

Number 3: You would want the fetch (region upwind of the flux tower that consists of "homogeneous" vegetation type and cover) to be greater than the flux footprint (See Schmid, H. P. "Experimental design for flux measurements: matching scales of observations and fluxes." Agriculturaland Forest Meteorology 87.2-3 (1997): 179-200.)

We have clarified that the fetch is greater than the footprint and cited the Schmid paper (lines 180-182 in the revision).

Page: 7

Number 1: Atmospheric stability depends on more than other factors besides u*.

We have used u* to reflect insufficient turbulent mixing at night, which is a general method (Reichstein et al., 2005; Scott et al., 2009). We have revised the description accordingly (lines 226-229 in the revision).

Number 2: How were these parameters obtained and how often? A moving window of ~1 week should be used. See for example, Reichstein et al.2005.

We have clarified that these parameters were obtained using the SPSS software with a 7-day window (lines 235-237 in the revision).

Page: 8

Number 1: These should be used to estimate G, the soil heat flux at z=0.

According to your comments and the actual situation in our study area, we determined that precipitation was the dominant factor that influenced the flux, so we have focused our analysis and descriptions on precipitation. The other indicators were removed.

Page: 9

Number 1: All of this extreme detailed reporting of numeral results is unnecessary. Use figures and tables to report only what is necessary to your analysis.

We have removed the unnecessary data descriptions and have retained the data most relevant to our analysis (lines 264-271, 282-294 in the revision).

Number 2: This could be why the NEE was positive, not just grassland recovery trajectory as mentioned in the abstract and discussion See Scott et al. 2010, 2015.

We analyzed the relationship between precipitation and CO_2 flux according to your comment, and found that the below-normal precipitation may be one explanation for why the sandy grassland was a net CO_2 source at an annual scale (lines 26-27, 395-403, 449-461 in the revision).

Number 3: Too much reporting of the values already shown in the figures and really the figures all convey essentially the same data, just at different time scales.

We have removed the values that are already shown in the figures in the revision.

Number 4: Here and elsewhere non-significant figures are being reported. Do we really believe that precip is accurate down to the nearest 10th of a mm, fluxes down to the 100th of a g?

We have rounded all values to the nearest integer in the revision.

Page: 10

Number 1: Round off to the nearest 1.

We have modified these values throughout the revision.

Number 2: Unusual...the thing should work right out of the box. Do you really mean this?

What we wanted to express was that we conducted a 1-month pre-experiment to test the stability of the instrument, and did not use the data collected during this period in our study. In order to avoid ambiguity, we have deleted this description.

Page: 11

Number 1: I'd like to see some plots like GEP or Reco vs. ppt (yearly or monthly or seasonal), et, or soil moisture. Water availability should be a dominant control. If it isn't, you need to tell us why.

We have added graphs of the relationships between NEE, GPP, R_{ec}, and the precipitation and soil water content at different scales (yearly, monthly, and daily) (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

Number 2: This is not an in-depth analysis. We already know that the diurnal scale is not only controlled, but also defined, by the fluctuations in energy input.
As we mentioned above, precipitation and soil water content were the main factors that influenced the fluxes and that we focused our descriptions on these factors rather than the energy factors. Specifically, we added an analysis of the relationships between NEE, GPP, and R_{ec} and the soil temperature and soil water content at a daily scale (lines 362-376, and Fig. 8 in the revision).

Page: 12
Number 1: Should use VPD.
As we mentioned above, precipitation and temperature were the main factors that influenced the fluxes, so we have focused on them and removed our description of the effects of relative humidity and atmospheric pressure.

Number 2: This type of analysis isn't getting to the heart of the matter. T is related to the seasonality or phenology at the site because it matches the timing of rainfall input (covariation), but it is water, not energy, which is a first order control on carbon cycling in these regions.
We agree with you suggestion that water is the most important factor and may be the first-order control on carbon cycling in our study region, so we analyzed the relationship between the CO_2 flux and the precipitation and soil water content (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

Number 3: What's the relationship between GPP and ground heat flux?
As we mentioned above, we have removed our discussion of the ground heat flux because it was not the main impact factor in our study region.

Number 4: third PC isn't discussed.
We have removed the PC analysis in the revision, because PCA analysis only shows the relationships between the variables and does not represent a mechanistic (causal) link. Because precipitation was the main influencing factor, we focused our analysis on precipitation and the resulting effects on soil water (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

Number 5: same two comments above apply here.
As we mentioned above, we have removed the PC analysis in the revision.

Number 6: This is a great example of why this analysis is not useful. The diurnal cycle is a dominated by energy. You need to look beyond this first order constraint on the diurnal variability to what is controlling the seasonal strength or weakness of GPP and Rec. Again, if that isn't water, then why not?
We have removed the PC analysis and energy factors according to your comments, and have focused our analysis on the relationships between CO_2 flux and the precipitation

and soil water content (lines 343-376, 395-403, 449-504 and Fig. 6-8 in the revision).

These results of within season empirical PC analysis provides no useful insight into the controls on grassland C flux that the rest of the community could find useful.
As we mentioned above, we have deleted the PC analysis.

Page: 13
Number 1: This is not a publishable result.
We have deleted the result.

Number 2: Recommend comparing your results to comparable ecosystems in comparable climates. Missing Scott et al., Biederman 2018, Pietrie et al. Studies from Australia or S. Africa.
We have added comparisons with these references (lines 381-388 in the revision).

Page: 14
Number 1: Certainly not a proven result and there are contradictory, site-specific results. See Scott et al. 2015, Kurc and Small 2007, Pietrie et al.
We have deleted the description and revised this to focus on how carbon sequestration of the ecosystem decreased with decreasing annual precipitation (lines 395-403 in the revision).

Number 2: What about below-average precipitation? Again, our expectation is that water is the dominant control so this should be examined.
As we mentioned above, we analyzed the relationship between precipitation and the CO_2 fluxes according to your comment, and showed that the below-normal precipitation may be one reason why the sandy grassland was a net CO_2 source at an annual scale (lines 26-27, 395-403, 449-461 in the revision).

Number 3: This doesn't convey any useful information.
We have deleted the description in the revision.

Page: 16
Number 1: This is the main result?
Yes, this is one of the main results.

Number 2: The daily scale is by definition regulated by radiation input.
As we mentioned above, we have deleted energy factors because they were not the dominant factors in the semiarid area.

Number 3: I would expect to see WATER, Because your study relies upon simple empirical correlation analysis to tell you what's going on without any input from what is already know from the science, your result here is overly simplistic with no links to physical processes. For example, why would SHF ever be relevant to GPP?
We have deleted the PC analysis and added a description of how water affected the CO_2 fluxes (lines 465-504 in the revision).

Page: 18

Number 1: Where is this result shown?

We have added the result (lines 343-350 and Fig 6 in the revision).

Number 2: This dataset appears to have all the data to remake the figures. What would really be useful is to know where the community can access the 30 min met and flux data? This "raw" data is exactly what others could use to generate comparisons and include with future studies. I strongly suggest this data is shared in a flux archive like ChinaFlux/Fluxnet. This will only increase the use of this data and benefit this studies authors.

Once our results are published, we will share our 30-min data in a flux archive such as ChinaFlux/Fluxnet to support comparisons and include this data in future studies.

Page: 35

Number 1: Labels are too small on all axes of all figures.

We have modified the text on the axes of all figures.

Page: 37

Number 1: The convention for these types of diurnal plots is to use micromol CO_2 m^{-2} sec^{-1}

We have revised the diurnal plots to use μmol CO_2 m^{-2} s^{-1}.

Thank you for your comments and for your work to improve the quality of our paper. We hope that with these changes, it will now be acceptable for publication, but we will be happy to work with you to resolve any remaining issues.

Sincerely,

Yuqiang Li, Ph.D
Northwest Institute of Eco-Environment and Resources
Chinese Academy of Sciences
320 Donggang West Road, Lanzhou, 730000, China
Phone/Fax: 86-931-496-7219
E-mail: liyq@lzb.ac.cn