Authors' Response to Anonymous Referee #1's Interactive comment on "Environmental controls on carbon fluxes over three grassland ecosystems in China" by Y. Fu et al.
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Anonymous Referee #1

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The paper by Fu et al., "Environmental controls on carbon fluxes over three grassland ecosystems in China," presents eddy covariance data from two full years of measurements at each of three sites, with some analysis of environmental drivers related to the net and gross CO2 fluxes. There is a need for additional flux data from native grassland systems, as they have not been studied as much as forests, and the present study does a good job of demonstrating how moisture seems to be the most important influence over CO2 fluxes. Data such as those presented in this manuscript can be used to improve models of C cycling to evaluate potential changes in C sequestration over large areas of steppe vegetation.

(1) Some questions and concerns came to mind while reading this manuscript. First, it seems like much more could be done with the data. Some basic relationships were obtained, but the environmental drivers controlling carbon fluxes are acting together in multivariate climate space, and the authors should strongly consider this in their data analysis. While they did obtain some significant relationships with single environmental drivers, it would be interesting to know how moisture and temperature, or LAI and cumulative precipitation, interacted to govern the fluxes. Refer to papers such as Kwon et al. (2008, AFM 148: 381-391) as an example.

A: Many thanks for your valuable comment. We have adopted your suggestion and used the stepwise multiple regression analysis to examine how air temperature, soil moisture, precipitation, PAR and LAI interacted to control the CO_2 fluxes within and among sites. The results have been added in Results section as *"3.5 Relevant controls on seasonal variation in R_{eco}, GEP and NEE"* (Line279-290) and *"3.6 Controlling factors for inter-annual and inter-site variation in CO_2 fluxes"*(292-300). The statistic information of the multiple analyses was also given in *Table 3* and *Table 4* in the revised manuscript.

(2) Second, in several places the text refers to severe drought stress, but this was not quantified physiologically. The precipitation was below average in one of the two years studied, but no measurements of plant water potential or even water use efficiency were presented. Without a set of parameters to backup the claims of drought stress (or citations of other papers that measured them), it would be better to remove these inferences.

A: We have removed the description of "severe drought stress" throughout our text, replaced by less precipitation or lower soil moisture (e.g. Line 198). As an indicator of dry climate at the temperate steppe in 2005, the values of mean annual water use efficiency (WUE) for the three sites in the two study years were added into Table 2. These values were cited from a previous published paper by Hu et al. (2008), which found that WUE for temperate steppe in 2005 was significantly lower than that in 2004.

(3) Third, the relationships of C fluxes with growing season length are interesting but circular. If growing season is defined as the sum of days when net C uptake was observed, it's not too surprising that correlations with GPP will be found. Unfortunately the grass phenology wasn't measured to allow for an independent estimate of growing season length. Was there a relationship between LAI and NEE? How was LAI determined?

A: Since you and other two referees all thought it inappropriate to define the growing season length (GLS) as consecutive negative NEE. Furthermore, it would be circular to relate GPP with GSL if using GPP to define GLS. Therefore, we gave up the idea of relating GEP or NEE to GLS in the revised manuscript and the definition of GLS was also removed from our manuscript. According to your suggestion, we found a significant role of LAI in controlling the variation in GEP and R_{eco} fluxes across the three grasslands using a multiple regression analysis (Line 292-300). The estimation of LAI was described in revised *Materials and Methods* section "*2.4 Vegetation Measurements*".

(4) Biomass clipping was cited in the methods but it wasn't clear if a subset of leaves were scanned for leaf area.

A: We have rewritten the section of LAI measurement and estimation in the revised manuscript (Line 165-175). After biomass clipping, a subset of the green leaves were scanned for LAI calculation. In the original manuscript, we mentioned that LAI was measured with clipping method at NMG and DX site, and with LI3100 at HB site. Li et al (2007) carried out an experiment at HB site and compared the two results of LAI measured with clipping and LI3100 methods. They found a significant agreements between these two methods (R^2=0.98, P>0.01), which positively supported the data reliability of our study.

[**Reference:** Li, Z.Q., Yu, G.R., Zhao, F.H., Fu, Y.L., Li, Y.N.: Spatial distribution measurement of leaf area index in flux contribution source of eddy covariance flux tower. Chinese Journal of Eco-Agriculture, 15: 131-134, 2007]

(5) The moisture and temperature relationships with P_{max} were interesting. It would be useful if parameters could be developed that were not so site specific (if they exist). Is there a difference in the relationships in Fig. 7, between TS and AMS sites? Presenting the data on similar scales would help the reader compare the lines, and it would also be useful to include the statistics for the relationships and even provide a test to determine if they are different.

A: According to your suggestion, we have presented the relationship between P_{max} and soil moisture at the temperate steppe and the alpime meadow-steppe in one Plot (Fig. 6b in revised manuscript). The statistic information was also given in Fig. 7b. We found that the temperate steppe was much more sensitive to the variation in soil moisture than the alpine meadow-steppe in terms of increasing GEP and gaining carbon (line 275-278).

(6) Although the manuscript is generally well written, the English could still be improved by careful proofreading by a native speaker. The organization of some sections was hard to follow and some suggestions were made to improve the flow of logic.

A: We are sorry for our poor English writing in previous manuscript. We have asked a native English speaker to help improve the English writing of the revised manuscript.

(7) Specific comments 8010: 3-7, be more specific about the climate changes and hypothesized effects on carbon cycling.

A: The paragraph has been revised. We cited some previous studies to address the potential effects of climatic change on carbon cycling in these grassland ecosystems. (Line 57-69).

(8) 8010: 12, "conducted to address"

A: This sentence was change to "Several studies have addressed carbon fluxes over the grasslands in China." (Line 73).

(9) 8010: 26, check latin names; *P. fruticosa* was changed recently; what species of Kobresia?

A: We are sorry that we failed to find the newly changed Latin name for *Potentilla fruticisa*. We would appreciate very much if you could tell us. The species of *Kobresia* are *Kobresia capillifolia* and *Kobresia humilis*, which have been given in Table 1.

(10) 8011: Provide the period of record for all climate data; what is the extent (area) of the exclosures relative to the tower footprints?

A: The record period of all climate data for each site were presented in Table 1. The extent of enclosure area at the temperate steppe was larger than 200s in each direction from the flux tower. The flux tower footprint at NMG varied between 19m (under atmospheric stability) and 195m (under atmospheric instability) from the south and northwest (Mi et al., 2006).

(11) 8012: Better to have a standard set of climate measurements for each site. Present these background data in a more organized way.

A: We have re-organized the section of "2.1 site description". The background data of the three sites were presented in Table 1 in revised manuscript. The meteorological measurements at the three sites were mostly same except that soil moisture was measured at different depths at alpine shrub-meadow at HB, which has been explained in Table 1 and in the text (Line 113-115).

(12)8013: Report the statistics on the relationships used for gap filling so readers can get an idea of the uncertainty associated with the 52% of the data that was modeled.

A: Major revision was made in *Section 2.3* about flux data processing, especially about the methods of gap-fillings (Line 128-143). The equations or models used for gap-filling were described in details, and we also added the window size and periods of those nonlinear regressions in the text (Line 130-158). We totally agree that gap-filling method is a very important issue for flux measurement and research. However, our study is mainly focusing on the comparison of carbon fluxes and their controlling factors among different grassland ecosystems over two years. It seems likely somewhat deviation from the main topic of our study to the methodology of eddy covariance flux measurement if all the detailed information on gap-filling methods and results were listed in the text. To show the reliability of our gap-filling results, the statistic information on the nonlinear regression fittings were given in following tables (Table A.1-A.4). We hope these results could be accepted by you.

Table A.1. Nonlinear regression results of daytime gap-fillings during growing seasons with *Michaelis–Menten* equation (Eq.(1)) during different period of growing seasons at three sites.

Site	**Year**	**Period (DOY)**	**α (mg CO_2 µmol quantum^{-1})**	**P_{max} (mg CO_2 m^{-2} s^{-1})**	**$R_{eco,day}$ (mg CO_2 m^{-2} s^{-1})**	**R^2**
HB	2004	DOY 121-290	0.00025<α <0.0018	0.11<P_{max} <0.84	0.051<$R_{eco,day}$ <0.17	0.49< R^2<0.84
	2005	DOY 121-290	0.00026<α <0.0026	0.098<P_{max} <0.88	0.068<$R_{eco,day}$ <0.18	0.44< R^2<0.81
NMG	2004	DOY 131~270	0.00072<α <0.0051	0.18<P_{max} <0.51	0.05<$R_{eco,day}$ <0.20	0.46< R^2<0.69
	2005	DOY 131~270	0.00024<α <0.0013	0.016<P_{max} <0.17	0.0013<$R_{eco,day}$ <0.065	0.31< R^2<0.56
DX	2004	DOY 120~280	0.000026<α <0.0026	0.027<P_{max} <0.24	0.0012<$R_{eco,day}$ <0.055	0.42< R^2<0.74
	2005	DOY 130~280	0.000021<α <0.00097	0.0227<P_{max} <0.21	0.0042<$R_{eco,day}$ <0.019	0.46< R^2<0.71

Table A.2. The nonlinear regression results between nighttme R_{eco} and soil temperature (at 5cm depth) with Lloyd & Taylor equation (Eq. (2)) for gap-filling of missing R_{eco} at HB and DX with the available data within each entire year.

Site	Year	R_{10} (mg CO_2 m^{-2} s^{-1})	T_0 (K)	R^2	n
HB	2004	0.097	232.4	0.71	1748
	2005	0.11	232.9	0.79	1593
DX	2004	0.046	219.8	0.42	1523
	2005	0.036	217.8	0.36	1601

Table A.3. The nonlinear regression results between R_{eco} and soil temperature and soil moisture (Eq.(3) and (4)) at NMG site during the non-frozen season (from April to October) in 2004 and 2005.

Year	Period	R_{10} (mg CO_2 m^{-2} s^{-1})	*a*	*b*	*c*	*d*	R^2	*n*
2004	April~October	0.051	2.33	0.098	41.23	-99.5	0.63	1230
2005	April~October	0.023	-0.49	0.17	149.5	-722.2	0.57	1028

Table A.4. The nonlinear regression results between R_{eco} and soil temperature with Lloyd and Taylor equation (Eq. (2)) at NMG site during the frozen season in 2004 and 2005.

Year	Period	R_{10} (mg CO_2 m^{-2} s^{-1})	T_0 (K)	R^2	n
2004	Jan.~Mar.	0.019	143.33	0.31	186
	Nov.~Dec.	0.026	222.2	0.37	174
2005	Jan.~Mar.	0.017	226.1	0.32	159
	Nov.~Dec.	0.0093	210.5	0.27	196

(13) 8014: How was LAI determined from the clipping?

A: The description on measurement and estimation of LAI at the three sites were rewritten in the revised manuscript (Line 151-159). Clipping-based LAI is measured on a sub-sample of leaves and related to dry mass, i.e. specific leaf area (*SLA*). Ten the total dry mass of leaves collected within the 0.25 m^2 sampling plot is converted into *LAI* by multiplying by the *SLA* (Line 165-169).

(14) 8015: 25 and Fig 3: This figure does not show anything about drought stress, just LAI. Also on line 26 it appears you mean AMS rather than DX.

A: The comparison on dynamics of LAI among the three sites has been revised (Line 208-216). This sentence has been modified as "its LAI declined significantly in 2005…." (line 214-215).

(15) 8016: Section 3.3 needs to be completely reorganized so the flux results from each site are presented in the same order. It is difficult to follow in the current presentation. In particular, better to first describe (briefly) the seasonal flux patterns observed, and then make some inferences about the environmental drivers later, when you present Table 2 and Figs 5-7.

A: According to your suggestion, we have reorganized Section 3.3 and 3.4 of our previous manuscript. In the revised manuscript, we firstly presented the seasonal patterns and annual carbon budgets in Section 3.3 and 3.4 (Line 216-256), and then analyzed the environmental drivers on seasonal, inter-annual and inter-site variation in CO2 fluxes of the three grassland sites in Section 3.5 and 3.6, respectively (Line 257-299). We hope that the revised manuscript has a better logic organization and is easier for following to read.

(16) 8018:9, change to Table 1 (not 2). Line 20, be more specific about "vegetation development," as you have LAI data. How about soil organic matter content and root biomass at the sites? Line 24, don't start sentences with And.

A: Sorry for such mistake. We've changed it, but Table 1 in previous manuscript is referring to Table 2 in the revised manuscript. The section of annual carbon budget has been rewritten and the sentence of "This could be ascribed to 20 the poor vegetation development and shallow soil with low nutrient content and low soil water retention" has been removed in the revised manuscript (Line 254-256).

(17) 8019: 3-8, this comparison should go into the discussion. Line 11: change to ": : :fluxes in AMS, the annual sums of carbon fluxes were unrelated..." Discussion section could benefit from better organization.

A: This section has been rewritten and the Discussion section was also rewritten. The comparison sentence has been removed in the revised manuscript.

(18) 8020: 15-16, use this as the topic sentence of the paragraph.

A: This sentence has been modified into "Although many studies have examined grassland CO_2 fluxes based on eddy covariance measurements, large variability exists in the findings due to the diverse vegetation and climate types present in grassland ecosystems.", and has been used as the topic sentence of Section 4.1 in revised manuscript. (Line 302-304)

(19) 8021: The P_{max} results would be better presented in the results section. Line 5, you mean non-light limited conditions.

A: We have moved this results into Results section "*3.5 Relevant controls on seasonal variation in R_{eco}, GEP and NEE*" in revised manuscript (Line 270-277).

(20) 8022: 29, replace "Averagely" with "On average: : :"

A: This sentence has been removed in revised manuscript.

(21) 8023-8024, GSL discussion, using the net C uptake to define GSL is an interesting idea but maybe using non-zero GEP would be better. If an entire growing season occurred with Reco > GPP would you say that there was no growing season (GSL = 0 days)? It would make more sense to define GSL as days with measurable GEP. Since the control of NEE by GSL is one of your main conclusions it's important to consider a non-circular way of estimating GSL. Why not just use the MODIS data directly?

A: Since we were lack of the phenology data, and all three referees thought it inappropriate to define the growing season length (GLS) as consecutive negative NEE. Furthermore, it would be circular to relate GPP with GSL if using GPP to define GLS. Therefore, we gave up the idea of relating GEP or NEE to GLS in the revised manuscript and the definition of GLS was also removed from our manuscript. Instead, we found an important role of leaf area index in controlling the variation in CO_2 fluxes cross the three grasslands using a multiple regression analysis. (Line 294-299)

(22) Figure 4, dots for GEP are too light Figure 5, note that R_{eco} is presented as negative values.

A: We have modified Fig. 4 with darker line for GEP. We are sorry for this mistake and have made the change to present R_{eco} as positive data in both Fig. 4 and Fig. 7 in the revised manuscript.

(23) Figure 9, is there a significant relationship between GSL and AP without the low outlier?

A: Since we gave up the use of growing season length (GLS) in the revised manuscript, Fig. 9 (relationship between GSL and annual precipitation) in previous manuscript was also removed.