

***Interactive comment on* “Environmental controls on carbon fluxes over three grassland ecosystems in China” by Y. Fu et al.**

Y. Fu et al.

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Received and published: 20 November 2009

Response to Anonymous Referee #3’s Interactive comment on “Environmental controls on carbon fluxes over three grassland ecosystems in China” by Y. Fu et al.

Anonymous Referee #3 Received and published: 13 October 2009

General comments and overall evaluation: The paper certainly contains the interesting data concerning seasonal and inter-annual variability in carbon fluxes and their constraining factors at rarely published grasslands in China. They could contribute the understandings of regional carbon dynamics across the sub-continental scale area, leading to the understandings of global carbon circulation. However, the paper suffered significantly from less-organized structure of Results & Discussion parts, a shortage of

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solid discussion for annual carbon budgets and inter-annual variability and the effect by the drought on carbon fluxes. Structure of Results sub-sections 3.2, 3.3 and 3.4 is really confusing me. The authors mentioned the discussion of results in Results section. Those should be in Discussion section definitely. The authors should concentrate on the description of results in Results section, and move the discussion of mechanisms of constraining factors for carbon fluxes and LAIs to the Discussion section. Or the author should merge two sections, i.e. Results and Discussion, into one section as Results & Discussion. In Discussion sections (including discussions in Results section), there are so many insufficient explanations, misunderstanding parts and contradictions, which make the value of this study reduced largely. I could not organize them here. Please see details in below minor comments. As the authors mentioned that 46, 48 and 50% data were qualified for each sites, more than half data were eliminated by data quality control and filled by gap-filling methods, which base on regression curves against temperature and water availability conditions. Those gap-filling approaches to calculate the daily, monthly and annually accumulated values of carbon fluxes are widely used, however, such high missing rate values may bring the reliability of discussions down definitely. This is because the behavior of carbon dynamics must be affected by the data regression. Totally, I recommend the authors to modify the structure of Results & Discussion sections thoroughly, and to do stead discussion on modified or added figures and tables. If the authors could make a modification, this paper might be of worth for recognizing as a “re-reviewing”.

A: Thank you very much for your directive comments on our manuscript. We have made major revision to our manuscript according to yours and the other Referee’s comments. The major revisions are addressed as followings:

âŠă. We have rewritten the Section “2.1 Sites description”, which is now presented as a better organized way in Table 1 and displays a clear summarization and comparison among the three grassland sites (Line 89-98, Table 1 in revised manuscript).

âŠă. Major revision was made in Section “2.3 Eddy covariance flux data processing”,

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especially about the methods 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).

âŠć. According to Referees' suggestion, a stepwise multiple regression analysis was performed to investigate the relationships of GEP, Reco, or NEE with concurrent changes in environmental variables (Ta, Sw, PAR, P) and LAI at monthly and annual time scales. Both single factor effect and confounding effects of multiple factors were analyzed with the stepwise multiple regression analysis. As a result, two new tables (Table 3 and Table 4) were added in the revised manuscript to present the statistic information of the stepwise multiple regression analysis.

âŠć. Since we were lack of the phenology data, and all three referees thought it inappropriate to define the growing season length (GLS) based on 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. The definition of GLS and the discussion on the effect of GLS on ecosystem annual carbon budget were also removed from our manuscript.

âŠć'. We are sorry for such confusion due to less-organization on structure of Results & Discussion parts. We have reorganized and rewritten the Sections of 3. Results and 4. Discussion. The Section 3.3 and 3.4 in previous manuscript were reorganized into 3.3 ~ 3.6, by separating the sub-sections of seasonal and inter-annual variation in CO₂ fluxes with the analysis of their environmental controls. Those discussion sentences in Results sections were all moved to Discussion sections, and the relationship between P_{max} and air temperature and soil moisture was moved into Section 3.5 as a part of Result. We hope the revised Results & Discussion parts have a better organization of structure.

âŠć. After giving up the discussion of growing season length (GLS), we found an important role of leaf area index and soil moisture in controlling the variation in CO₂ fluxes

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cross the three grasslands. Therefore, the Discussion section was also changed to focusing on the environmental and LAI controls on seasonal, inter-annual and inter-site variations in ecosystem carbon budgets. The Discussion on effects of growing season length on ecosystem carbon exchange was removed from our revised manuscript.

âŞş. As a result after the above revisions, the conclusion of our study was also changed into “The available soil moisture remains the primary factor influencing the spatial variation in net carbon exchange in grassland ecosystems.”(Line 415-417)

âŞğ. Three new Tables were added in the revised manuscript (Table 1, 4, 5) and the original Table 1 and 2 in the previous manuscript were revised into Table 2 and 3 in the revised manuscript.

âŞí. Major revision and reorganizations were made to the Figures in the revised manuscript. Figure 1 and Figure 3 were revised in a better reorganized way. A new figure for the relationships between Reco and soil temperature at the three sites was added (As shown in new Figure 5). Previous Figure 5 was deleted. Previous Figure 6 was modified into new Figure 7. Previous Figure 8 and 9 were also replaced by new ones. Please review the revised manuscript for details.

Minor comments: (1) P8012, Line 26-27 and Fig.2: Soil water during mid-winter is supposed to be frozen, as air temperature shows less than minus 5 degree C. Thus, the values of Sw derived by TDR are not reliable, at least, during the periods where soil temperature is less than 0 degree C. Please indicate the unreliability of Sw in the caption of Figure 2, and the authors should not use Sw for the calculation of Reco during mid-winter. Another point is that Fig. 2 indicate 0.05 and 0.5 m depth Sw in TS and AMS although the manuscript says 0.05, 0.2, 0.4 m depth Sw were taken in both sites. Please unify them.

A: It is true that the soil water was frozen with soil temperature less than 0 oC during mid-winter from late November to mid March across the three sites in our study. Therefore, we used both soil temperature and soil moisture to calculate Reco

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the temperate steppe (i.e. NMG site in the revised manuscript) during non-frozen period (i.e. from April to October), whereas we only used soil temperature to calculate Reco of the temperate steppe during frozen period. We have added this description in section “2.3 Eddy covariance flux data processing” in the revised manuscript (Line 137-143). We apologize that there was a mistake about soil moisture measurement depths in previous Figure 2. We have mended this mistake in the revised Figure 2, which plotted soil moisture at 5cm and 20cm depths at NMG and DX site. However, the soil moisture were measured at 20cm and 40cm depths at HB. We have modified this section in the text (113-115)

(2) P8013, Line 16-24: Those methods to fill the gaps are widely used and have no problem in themselves. However, the authors should show details in data processing, which are strongly related to accuracy of time-averaged carbon fluxes, more than now. At first, the authors should indicate how often the regression curves are fitted. Monthly or Biweekly or Weekly? At second, the authors should show the regression coefficient and determinant coefficient and significance of regression curves of NEE and Reco as in Table for every period. That table could be in appendix.

A: Thanks for your suggestion. We have made a detail description of data gap-filling as presented in Appendix A. The details in regression coefficient and determinant coefficient of for each site and year were presented in Appendix A, cause there are many windows for the three sites and two study years.

We also have modified the section of data gap-fillings in the revised manuscript with description on the size of gap-filling windows and the equations used. “The missing daytime CO₂ flux (net ecosystem exchange of CO₂, NEE) during the growing season was estimated as a function of PAR using the Michaelis–Menten equation with a 10-days moving window”. (Line 131-133)

The missing daytime CO₂ flux during dormant season and the missing nighttime CO₂ flux (indicated as ecosystem respiration, Reco) were estimated with the empirical re-

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relationships between Reco and soil temperature throughout the whole year at HB and DX. (Line134-137).

The missing Reco at NMG were during growing season (from April to October) were estimated with a Q10 model, which took both soil temperature and soil moisture into account. The missing Reco data at NMG during the frozen period (from January to March and from November to December) were estimated with the Lloyd and Taylor equation. (Line 139-143).

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 the tables (Shown in the attached Spplement). We hope these results could be accepted by you.

(3) P8014, Line 5-8: LAI only in ASM is derived by LI3100A. That difference in LAI measurement is crucial. Have the authors taken the calibration between clipping and LI3100A? Otherwise, the author should address the difference in accuracy of LAI measurement here. P8014, Line 8-12: MODIS NDVI were used for the interpolation of LAI values. Are they applied for all three sites? Or only in ASM? Please clarify it. Moreover, the authors should address what the determinant coefficient ($R^2 > 0.94$) covers. Is this value for only ASM site only for 2005, when the LAI values were taken in the field? Or does this show averaged R^2 of three sites for the period of field data existing? This sentence is really confusing me.

A: We have rewritten the section of LAI measurement a in the revised manuscript (Line 165-175). In the original manuscript, we mentioned that LAI was measured with

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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 effectively supported the data reliability of our study. MODIS NDVI data were used for the interpolation of LAI at all three sites and good agreements between NDVI and measured LAI were found at each site. The determinant coefficient ($R^2> 0.94$) were averaged R^2 of three sites for the period of field data available (Hu et al., 2008).

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

Hu, Z.M., Yu, G.R., Fu, Y.L., Sun, X.M., Li, Y.N., Shi, P.L., Wang, Y.F., Zheng, Z.M.: Effects of vegetation control on ecosystem water use efficiency within and among four grassland ecosystems in China, Global Change Biol., 14, 1609-1619, 2008.

(4) P8014, Line 21-25: Fig. 1 is not helpful for me to find out the difference in time averaged values of PAR and growing season T_a . Please add those values in Table 1.

A: we have modified Figure 1 in a more organized way (See Figure 1 in revised manuscript). We also added the values of average air temperature, averaged PAR and accumulated LAI during growing season (from May to September) at the three sites during two years in Table 2 in the revised manuscript.

(5) P8014, Line 23-25: the description that the mean annual T_{as} were comparable among three sites, seems inadequate. T_a of ASM is 2 degree C less than those of other two sites, and this fact makes the ASM site humid, resulting in being meadow, even though annual precipitation is comparably low as that in AMS site. The authors should modify this description.

A: We have modified this paragraph and this sentence has been removed from the text (Line 193-195).

(6) P8015, Line 1-2: Table 1 says Ta in TS is higher in 2004 than that in 2005. It is not collect.

A: We are sorry for this mistake. The fact is that Ta in TS is higher in 2005 than that in 2004 during the growing season. This sentence has been modified as “The mean annual Ta in 2005 was higher than that in 2004 at HB and DX. A common point among the three sites is that it was warmer during the growing season in 2005 than in 2004 (Table 2)” (Line 194-195).

(7) P8015, Line 9 and 13-14: “no water stress was detected” and “low water holding capability of sandy soil and high surface evaporation”; if the authors would like to say such deterministic facts for characteristics in climate, please show the data to prove them or quote the literatures on those studies.

A: Thanks for your suggestion. We have paid attention to avoiding using the conjecture words in our revised manuscript. This phrase of “no water stress was detected” was deleted, and this sentence has been modified as “the abundant precipitation at HB resulted in relative high soil moisture content throughout the growing seasons, although the relatively less rain in the spring of 2004 caused decrease in soil moisture, compared to 2005” (Line 201-203). The sentence for DX site was also modified as “the soil moisture at DX was generally lower than that at HB (Fig. 2c), with higher seasonal fluctuation due to low vegetation coverage and high surface evaporation induced by strong radiation (Shi et al., 2006; Hu et al., 2008).” The reference literatures were also added” (Line 203-206).

(8) P8015, Line 17: “soil drying out”: how much was it in Sw? Please clarify it. A: This sentence has been modified as “However, it was much drier in 2005 at NMG, with precipitation significantly below the average that led to continuous decrease in soil water content from May throughout the whole growth season (Fig. 2a)” (Line 198-200).

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(9) P8015, Line 21-23: This could not be believed. For me, the spring LAI in ASM grew one or two month faster in 2005 than that in 2004, although it is quite hard to know exact dates of onset from Figure 3 because of unclear x-axis ruler. Please check it again.

A: This paragraph has been rewritten. This sentence has been changed into “The LAI at HB in 2005 grew half month faster in the spring and also peaked half month earlier in summer than those in 2004 (Fig. 3b). Although the peak value of LAI at HB in 2005 was 0.15 m² m⁻² lower than in 2004, the accumulated LAI (LAIsum) during the growing season (May~September) in 2005 was higher than that in 2004 (Table 2)”. (Line 209-212)

(10) P8015, Line 26: What is “DX”?

A: Sorry for this mistake. DX is the abbreviation of the site name of alpine meadow-steppe, which was used in the first draft and then replaced by AMS in the previous submitted manuscript. In the revised manuscript, we again used DX to refer to the alpine meadow-steppe (Line 95).

(11) P8015, Line 27- P8016, Line 2: The determining way of GSL in this study is not adequate. I suppose that, even if NEE is positive, negative GEP should mean plant growing. For example, as I mentioned in above comment, onset of LAI growth is one to two months earlier in 2005 than in 2004 in ASM site, and the duration with LAI of more than 0.5 or 1.0 seems longer in 2005. These facts would indicate longer GSL in 2005 and be against the shorter GSL in 2005 as shown in Table 1. Indeed, larger annual negative GEP and higher annual mean Ta might support longer GSL in 2005. The clearest standard should be positive NPP for the growing season of plant bodies. If NPP is unavailable, otherwise, negative GEP could be another standard for it.

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

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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 CO₂ fluxes cross the three grasslands using a multiple regression analysis.(Line 161-168)

(12) P8016, Line 25 – P8017, Line 1: What is the mechanism of positive net ecosystem carbon sink before senescence in 2004? How did decreasing temperature relate to it, even though Fig. 5 shows less significant relationships between NEE and Ta in TS based on monthly average? Please explain the effect of low temperature properly.

A: We are sorry for the misunderstanding due to the long sentence. We are trying to explain that the positive net ecosystem carbon sink in early September before senescence was mainly resulted from favorable soil moisture. Whereas, the temperate steppe shifted into net carbon release in late September with the onset of grass senescence due to decreasing temperature in autumn. We have modified this sentence in the revised manuscript (Line 230-233).

(13) P8017, Line 19-25 and Fig. 5: The authors address the effect of radiation on carbon fluxes. However, there is no plot of carbon fluxes with radiation. Indeed, in AMS, monthly carbon fluxes are plotted against soil water content, whereas they in other two sites are plotted against precipitation. The authors must show every plots or statistical values of plots, which you address in the manuscript. Otherwise, you could be suspected to have any inconvenient problems for you on showing such hid plots? Totally, the authors should show the plots of carbon fluxes with radiation, temperature, precipitation, soil water content in Fig. 5 and statistical values of them in Table 2. It is also recommended to add the plot of carbon fluxes with LAI, which could be a base of photosynthesis. Moreover, precipitation is not adequate measure to evaluate water availability. It is just a potential value of water availability. The difference between precipitation and evapotranspiration (P-ET) or between precipitation and evapotranspiration plus infiltration plus runoff (P-ET-I-R) should be the proper measures of water availability; i.e. soil water content. The authors could apply the plots with soil water

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content for three sites instead of them with precipitation in Fig. 5. Finally, why are signs of Reco values negative only in Fig. 5, although they are treated as positive values in all other places? Never do that.

A: We were sorry for the incomplete information in previous manuscript. We just wanted to show those plots with significant relationships between CO₂ fluxes and environmental factors. We were not trying to hide any problems with our results. We have accepted your suggestion in our revised manuscript. We used the stepwise multiple regression analysis to investigate the relationships of GEP, Reco, or NEE with changes in air temperature (Ta), soil moisture (Sw), PAR, precipitation (P), and LAI with monthly data during growing seasons (May to September) in SPSS. The results of Figure 5 in previous manuscript were replaced by Table 3 in revised manuscript. The Statistical information (regression coefficient, partial correlation coefficient and significance probability (P)) of the regression analysis between GEP, Reco, or NEE with Ta, Sw, PAR, P and LAI were all given in Table 3. The corresponding results were also described in text of Section 3.5 in revised manuscript (Line 278-289). Our results confirmed your suggestion that soil moisture is a more proper measure of water availability than precipitation and LAI plays a Key role in the variation in GEP (Line 294-299).

The mistake of negative sign for Reco values was modified.

(14) P8018, Line 9: Table 2 seems to be “Table 1”. A: This mistake has been changed.

(15) P8018, Line 10: “local carbon sink”; I could not imagine anything from it. What does “local” mean? Please clarify and use other words for it.

A: We have modified this sentence and the word of “local” was deleted in revised manuscript. This sentence has been modified as “The alpine shrub-meadow at HB was a net carbon sink, with a total accumulation of -137 g C m⁻² over the two study years.” (Line 245-226) (16) P8018, Line 16-18: Non-grazing system might result in larger litter fall in TS in the comparison with other two sites. However, litter production in 2005 is supposed to be much smaller due to low productivity, which could be assumed from

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extremely low GPP, and that makes Reco largely lower, although litter fall in 2004 is large as much as usual and last and affect Reco until spring in 2005. Mineral soil respiration could be the primary factor for the relatively larger Reco compared to GPP in 2005.

A: Thanks for your comment. This sentence has been modified as “The substantial ground litter fall from previous years due to non-grazing at NMG effectively contributed to Reco in the spring of 2005. Besides, the mineral soil respiration under dry soil environment may also account for the relatively larger Reco compared to GEP in 2005 at NMG, resulting in more carbon release in 2005 than in 2004.” (Line 250-254).

(17) P8018, Line 18-21: What is the magnitude of carbon fluxes? If the authors try to say about the absolute values of annual carbon fluxes in Table 1, the values in TS are smallest in 2005 and this discussion is not collect. Another thing is that the authors mention shallow soil and low nutrient content and low soil water retention in AMS as the reason for possibly small magnitude of annual carbon fluxes. However, that makes me confused. There never be any explanation of those characteristic of AMS so far. Sub-subsection 2.1.3 shows the site data as the depth of soil is 0.3 – 0.5 m, with 30% of gravel content and 0.9% - 2.97%, but no any apparent explanation of shallow soil and low nutrient content and low soil water retention compared to other two sites. Indeed, the site description of other two sites does not have any absolute values of those soil depth and water retention and nutrient. We could not assume anything about soil characteristics in AMS from such little or insufficient information of site description.

A: We have rewritten the Results and Discussion Sections in the revised manuscript. This sentence has been modified as “The magnitude of annual sums of CO₂ fluxes at DX was much smaller than those at HB, although the precipitation at these two sites was comparable (Table 2). The GEP and Reco at DX were reduced in 2005, which was mostly resulted from the less spring precipitation. ” (Line 254~256)

(18) P8019, Line 1: The author should add the plot with mean annual air temperature

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and radiation in Fig. 6. Never hide the plots or statistical values when the authors try to address something on those relationships between annual carbon fluxes and two factors, even if they are not statistically significant.

A: This section has been rewritten and Figure 6 in previous manuscript has been revised and presented in Figure 7 in revised manuscript. The statistic information (no matter significant or not) on the relationships of annual GEP, Reco or NEE with accumulated LAI, soil moisture and annual precipitation were all shown in Figure 7. The multiple regression models were also given in Table 4 in the revised manuscript. The revised texts were given in Line 291-299.

(19) P8019, Line 15: Attach “with other ecosystems” or something like them after the title of subsection 4.1. Otherwise, we cannot imagine immediately what the authors are going to compare the data with.

A: The title of 4.1 Section has been changed into “Comparison of ecosystem carbon budget with other ecosystems”. (Line 301)

(20) P8020, Line 5: The authors definitely compared the flux values of this study with other ecosystems in the first paragraph of subsection 4.1. However, there is no description characterizing three sites based on these comparisons. I don't like to know so much about whether if the values are larger or smaller than those of other ecosystems, but, for ex., how different or similar the characteristics are between in the fact and in the expectation when assumed by climatic zones and by biome types. Thus, the authors should add some concluding remarks after this first paragraph to show the characteristics of three sites in terms of carbon fluxes here.

A: This section has been rewritten and the comparison on daily values of CO₂ fluxes among different ecosystems was deleted. In the revised manuscript, we focused on comparing the net annual carbon budget of the three grasslands in our study with others that have similar vegetation or climate types with our sites. Therefore, we'd like to notice you that the revised Section 4.1 was much different with the previous

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manuscript. (Line 302-332)

(21) P8020, Line 17-24: Those sentences are quite ambiguous. I don't know what the authors are going to say here. Indeed, quoting Novick et al. and Gilmanov et al., which might address the geographical patterns of annual NEE in grassland ecosystems, makes confusion when the authors try to say about interannual changes in NEE soon after above two citations with quoting Flanagan et al. and Ma et al. Those should be discussed separately. Finally, the discussion on the alternation of sign in annual NEE in ASM sites are ought to be come definitely with the explanation of possible mechanisms, and those in other two sites should also be discussed.

A: This section has been rewritten and these sentences were deleted from our text in order to avoid confusion with the original objective of this section. (Line 315-332)

(22) P8021, Line 7-13: The authors should add the values of statistics in regression curves in Table 2 seems to be "Table 1".

A: We have accepted your suggestion. Table 2 was revised according to your suggestion and is presented as Table 3 in our revised manuscript. The stepwise multiple regression analysis was used to investigate the relationships of GEP, Reco, or NEE with changes in air temperature (T_a), soil moisture (Sw), PAR, precipitation (P), and LAI with monthly data during growing seasons (May to September). The Statistical information (regression coefficient, partial correlation coefficient and significance probability (P)) of the regression analysis between GEP, Reco, or NEE with T_a , Sw , PAR, P and LAI were all given in Table 3. The corresponding results were also described in text of Section 3.5 in revised manuscript. (Line 278-289)

Remarks: A more complete Response was attached as Supplement, which included the Tables of Statistic information on Gap-fillings as a repl to Question (2).

Please also note the Supplement to this comment.

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