Dear Dr. Wohlfahrt,

We sincerely thank you for handling the reviewing of our manuscript submitted to Biogeosciences “bg-2009-143”. We also want to thank you and the other two anonymous revirwer for your valuable and constructive comments on our manuscript, and appreciate you giving us a chance to revise it. We cherish this opportunity very much and tried our best to improve our manuscript according to yours and the other two Referee’s comments.

Enclosed please find our replies and explanations to all of your comments and questions. We also would like to explain the several major revisions we’ve made in our revised manuscripts to you before you go through the one-bye-one response to your specific comments. The major revisions in our manuscript include the following aspects:

①. 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).

②. Major revision was made in Section *“2.3 Eddy covariance flux data processing”*, 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).

③. According to Referees’ suggestion, a stepwise multiple regression analysis was performed to investigate the relationships of GEP, R_{eco}, or NEE with concurrent changes in environmental variables (T_a, S_w, 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.

④. 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.

⑤. 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 sub-sections 3.3 ~ 3.6, by separating the description of seasonal and inter-annual variation in CO_2 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. Therefore, the Results and Discussion Sections are much different from the previous manuscript.

⑥. 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 CO_2 fluxes 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.

⑦. 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)

⑧. 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.

⑨. 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 R_{eco} 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.

We hope the above summarization would be helpful for you to look through our revised manuscript more clearly. The enclosure is our responses to all of yours and the other two Referee's comments.

Thanks again for your efforts on our manuscript. Please let us know your comments for further revision on our manuscript.

Yours Sincerely,

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Response to Dr. G. Wohlfahrt's Interactive comment on "Environmental controls on carbon fluxes over three grassland ecosystems in China" by Y. Fu et al.

Referee: G. Wohlfahrt

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Due to a shortage of reviews I am in my function as a handling editor of this paper providing a second review by myself.

Fu et al. compare the NEE and its component processes the environmental controls on this processes among 3 grassland ecosystems in China. As the authors point out, there is a shortage of data on grassland ecosystems, in general and in particular for this part of the globe - thus there is surely merit in trying to synthesis data on grassland carbon cycling for this area. However, the paper is fairly standard in the way data are analyzed and the discussion overall is very descriptive so that the contribution by this paper to the scientific field is not very significant. I thus believe that major revisions, as detailed below, will be necessary before the paper becomes acceptable for publication.

General comments:

(1) As mentioned above, the analysis carried out in this paper is pretty standard and the discussion of their findings remains largely descriptive. To change this I first suggest the authors formulate hypothesis about what they anticipate their study will reveal - based on the available introduction this should be straightforward. Second, the authors should sit together and reflect about what makes their data novel and how to tease out this novelty. The differential controls by temperature and precipitation at the three sites is a really good starting point, but the analysis needs to be more convincing. In this context I agree with reviewer #1 in that a more thorough statistical analysis needs to be presented - currently my impression is that the authors just

present those bits and pieces which help to undermine what they intend to show. What are the confounding controls of the other factors potentially driving carbon cycling? What is the role of biotic controls, e.g. LAI? In my view, because of the standard analysis, data are underexploited, e.g. what about the role of temperature and moisture in controlling RECO across the sites - how does RECO differ at similar temperature and moisture? What about the role of LAI in determining GPP? and so forth ...

A: Thank you very much for your general and directive comments on our manuscript. The major revisions according to your above general comments are addressed as followings:

①. We added our hypothesis of this study in the last paragraph of *Introduction Section*. We hypothesized that (1) precipitation dominates the CO_2 fluxes in temperate steppe, and temperature constrains CO_2 fluxes in alpine meadows; (2) at regional scale, precipitation is the first primary factor that controls the spatial variation in grassland carbon budget (Line 80-82).

②. According to Referees' suggestion, a stepwise multiple regression analysis was performed to investigate the relationships of GEP, R_{eco}, or NEE with concurrent changes in environmental variables (T_a, S_w, PAR, P) and LAI at monthly and annual scales. Both single factor effect and confounding controls 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.

③. 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. The definition of GLS and the discussion on the effect of GLS on ecosystem annual carbon budget were also removed from the revised manuscript.

④. 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

CO_2 fluxes cross the three grasslands using a multiple regression analysis. (Line 279-287). 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.

⑤. In the revised manuscript, a comparison on the response of R_{eco} to variation in soil temperature among the three grassland sites was presented (Line 259-270). A new figure for the relationships between R_{eco} and soil temperature at the three sites was added (Figure 5).

(2) Drought stress: as indicated by reviewer #1 I would like to see signs of drought stress, not necessarily in terms of some ecophysiological data, but e.g. as flux data vs. moisture.

A: We have deleted the terms of "severe drought stress" throughout our text. As an indicator of dry climate at the temperate steppe in 2005, the values of mean annual water use efficiency (WUE) at the three sites in the two years were listed in revised Table 2, cited from a previous published paper by Hu et al. (2008). The WUE for temperate steppe in 2005 was significantly lower than that in 2004.

(3) The English is generally sufficient, but sometimes mistakes have sneaked into the paper which needs to be ironed out in the revision. Because these mistakes are fairly abundant I do not specifically refer to these below.

A: We are sorry for our poor English writing. We have asked a native English speaker to help improve the English writing of the revised manuscript. We hope the revised manuscript is much more readable.

Detailed comments:

(1) Ttile: wouldn't be "Environmental controls on CO2 fluxes OF three grassland ..." be more appropriate; please do not use carbon where you mean CO2 - there are a lot of other carbon fluxes aside from CO2 (e.g. CH4, VOCs, ...); the only case I can

imagine carbon to be appropriate would be "carbon assimilation".

A: Thanks for your suggestion. We have changed the title into “Environmental influences on carbon dioxide fluxes over three grassland ecosystems in China”. “carbon flux” was also changed into “CO_2 fluxes” throughout the manuscript text.

(2) p. 8010, l. 27: please provide more details on the regional significance of these three grassland ecosystems as opposed to other ecosystems.

A: We have revised this section and the regional significance of these grasslands has been addressed. We added the following sentences:

- “The temperate steppe represents one of the typical vegetation types on the Eurasian continent, and half of its total area (about 84 million ha.) is located in China (Li et al., 1998)” (Line 60~61).
- “Meanwhile, the alpine meadow ecosystems on the Qinghai-Tibet Plateau, covering an area of approximately 2.5×10^6 km^2 (Zhao et al., 2006) and with higher soil carbon density than those savannas and temperate grasslands (Adams et al., 1990; Ni, 2002), may have played an important role in global carbon cycles.” (Line 66-69)

(3) Site description general: I find the use of the abbreviations ASM and AMS very unfortunate because they may be easily mixed - maybe the authors can find a different abbreviation for these two sites

A: We are sorry for this confusion. We have rewritten the Section *“2.1 Sites description”*, which is now presented as a better organized way in Table 1 (Line 89-98). The previous abbreviations of the three sites (TS, ASM and AMS) were derived from their vegetation type. In order to avoid the confusion of ASM and AMS, we have changed the abbreviation of three sites according to their local placename throughout the revised manuscript. Therefore, TS, ASM and AMS have been replaced by NMG, HB and DX, respectively. Please refer to the *“2.1. Site description”* section (Line 89, 92, 95) and Table 1 in the revised manuscript.

(4) p. 8012, l. 20: the 3d-coordinate rotation aligns the anemometers coordinate system with the mean streamlines

A: We have accepted your suggestion and this sentence have been modified as “we applied three-dimensional rotation to align the anemometers coordinate system with the mean wind (Wilczak et al., 2001)” (Line 117-118).

(5) p. 8012, l. 23: the main issue with the density correction, in particular for these ecosystems, is density fluctuations because of the sensible heat flux, which is likely to dominate over the latent heat flux.

A: We have accepted your suggestion and this sentence have been modified as “The WPL method was applied to adjust density changes resulting from fluctuations in heat and water vapor” (Line 118-119).

(6) p. 8012, bottom: why just give details about the TDR instrument and not about the others?

A: We have revised the section of meteorological measurements, and the details about all relevant meteorological sensors were described in revised manuscript (Line 107~116).

(7) p. 8014, l. 5: how many replicate measurements?

A: We have revised the section of LAI measurements. This sentence had been modified into “The sampling plot was 50 cm × 50cm in size and 5 replicates were taken on each measurement day”. (Line 166-167).

(8) p. 8015, l. 9: due to the differing soil physical properties it would be very helpful to scale the original soil water content measurements between field capacity and wilting point (as plant available water) or at least as a fraction of saturation water content; this will improve the comparability between sites.

A: Thanks for your suggestion. We tried to scale the original soil water content measurements as a fraction of saturation water content, but it was a pity that we only

accessed to the field data from NMG and HB site and failed to get such data from DX site. Besides, the difference in measuring soil bulk density among different site would likely produce considerable uncertainty in calculating saturation soil water content. Therefore, we think it is acceptable to use the original soil water content measured with same method and same TDR sensors across three sites. We hope this could be acceptable by you.

(9) p. 8015, 8016: what the authors refer to as growing period is essentially the net carbon uptake period; relating this to NEE is circular, as reviewer #1 pointed out; the growing period is the time during which plants grow - this may or may not be related to the net carbon uptake period; as suggested by reviewer #1 better use GPP to delineate this period, but also in this case refrain from relating to GPP or NEE because this would be circular; alternatively use some independent measurement to delineate the growing period - this may then be compared again carbon cycle metrics.

A: Since we were lack of the phenology data, and all three Referees thought it inappropriate to define the growing season length (GLS) from 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 the revised 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.

(10) p. 8019, l. 3-5: this should go into the discussion.

A: We have made major revision to the *Results* and *Discussion* sections. This sentence has been removed in the revised manuscript.

(11) p. 8019, 8020: the discussion here is very confusing - I suggest using a table to summaries the results from literature and referring to the table in a concise fashion.

A: By summarizing your comment and Anonymous Referee # 3's comments on this

section, we have given up the comparison of daily CO_2 flux values among different grassland ecosystems. This section was re-written and Table 5 has been added to compare the annual carbon budget of the three sites in our study with other studies assumed by similar climatic zones and biome types (Table 5). Therefore, the revised Section 4.1 was much different from before in previous manuscript (Line 303-337).

(12) p. 8020, l. 22-24: this sentence will hold for a lot of ecosystems – remove.

A: Thanks for your suggestion. This sentence has been removed in the revised manuscript.

(13) p. 8021, l. 1-13: these are new results - move into the results section; this applies to all original material.

A: We are sorry for mixing the results and discussion together. We have moved this results into Results section "*3.5 Relevant controls on seasonal variation in* R_{eco}, *GEP and NEE*" (Line 256-264).

(14) Fig. 6: use different symbols for different sites

A: Fig. 6 was revised into Fig. 7 in the revised manuscript. Different symbols were used for different sites in the revised Figure 7.