

Interactive
Comment

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

Anonymous Referee #3

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General comments and overall evaluation:

The paper certainly contains the interesting data concerning seasonal and interannual 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 solid discussion for annual carbon budgets and interannual 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

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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”.

Minor comments:

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 Sws in TS and AMS although the manuscript says 0.05, 0.2, 0.4 m depth Sws were taken in both sites. Please unify them.

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 Bi-weekly 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.

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.

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.

P8014, Line 23-25: the description that the mean annual $T_{a,s}$ 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.

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

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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.

P8015, Line 17: “soil drying out”: how much was it in Sw? Please clarify it.

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.

P8015, Line 26: What is “DX”?

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 T_a 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.

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 T_a in TS based on monthly average? Please explain the effect of low temperature properly.

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

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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 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.

P8018, Line 9: Table 2 seems to be “Table 1”.

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.

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 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.

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 author mention shallow soil and low nutrient content and low soil water retention in AMS as

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the reason for possibly small magnitude of annual carbon fluxes. However, that make 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.

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

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.

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.

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

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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.

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

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