Biogeosciences Discuss., 6, C2217–C2220, 2009
www.biogeosciences-discuss.net/6/C2217/2009/

Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 26 September 2009

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.

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.

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.

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

The moisture and temperature relationships with Pmax 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.

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.

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

8010: 12, “conducted to address”

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

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

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

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.

8014: How was LAI determined from the clipping?

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.

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.

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.

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.

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

8021: The Pmax results would be better presented in the results section. Line 5, you mean non-light limited conditions.

8022: 29, replace “Averagely” with “On average. . .”

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?

Figure 4, dots for GEP are too light Figure 5, note that Reco is presented as negative values. Figure 9, is there a significant relationship between GSL and AP without the low outlier?

Interactive comment on Biogeosciences Discuss., 6, 8007, 2009.