

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

G. Wohlfahrt (Referee)

georg.wohlfahrt@uibk.ac.at

Received and published: 12 October 2009

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 synthesise data on grassland carbon cycling for this area. However, the paper is fairly standard in the way data are analysed 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.

C2463

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

Detailed comments: (1) Title: 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" (2) p. 8010, l. 27: please provide more details on the regional significance of these three grassland ecosystems as opposed to other ecosystems (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 (4) p. 8012, l. 20: the 3d-coordinate rotation aligns the anemometers coordinate

C2464

system with the mean streamlines (5) p. 8012, l. 23: the main issue with the density correction, in particular for these ecosystems, are density fluctuations because of the sensible heat flux, which is likely to dominate over the latent heat flux (6) p. 8012, bottom: why just give details about the TDR instrument and not about the others? (7) p. 8014, l. 5: how many replicate measurements? (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 (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 (10) p. 8019, l. 3-5: this should go into the discussion (11) p. 8019, 8020: the discussion here is very confusing - I suggest to use a table to summarise the results from literature and refer to the table in a concise fashion (12) p. 8020, l. 22-24: this sentence will hold for a lot of ecosystems - remove (13) p. 8021, l. 1-13: these are new results - move into the results section; this applies to all original material (14) Fig. 6: use different symbols for different sites

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

C2465