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Biogeosciences Discussions

Interactive comment on "Land use affects the net ecosystem CO_2 exchange and its components in mountain grasslands" *by* M. Schmitt et al.

Anonymous Referee #1

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General comments:

The authors investigated how net ecosystem carbon (C) exchange (NEE), gross primary productivity (GPP), ecosystem respiration (R) differ between mountainous grassland ecosystems of different land-use as well as how these parameters are associated with leaf area index (LAI), plant biomass, and light use efficiency (LUE) in the different systems. Given the large changes mountainous ecosystems are currently facing in many European countries (ceasing of agricultural land use practices) this topic is of great relevance and studies such as this one can help to better understand the carbon (C) sink-source capacity of mountainous ecosystems. However, to make a contribution in that context, I suggest the authors to look at their data differently from how they currently do. The main results presented in the current version of the manuscript are

relationships between LAI, LUE, PFD and NEE, GPP, R etc (some questions remain though on the data presented eg. in Fig 1 → see comment below). In my opinion, this is nothing really novel and has been published many times by various authors for grassland ecosystems, even at high elevations (Gu et al. 2003, Kato et al. 2004a,b). Given the large amount of data the authors have available from all their different grasslands on similar soils I would have expected them to much more use this data to show how land use affects the fluxes. In Figure 3 they do present differences between the land use types. However, when reading the captions they took data from May 2002 for the valley bottom meadow, August 2002 for mountain meadow, August 2002 for the pastures, August 2003 for the nutrient poor abandoned grassland, March 2002 for the nutrient rich abandoned grassland (according to 11443, line 22 "seasonal peak values"). To me it is not a feasible comparison as the environmental conditions etc must have differed considerably, when measuring the fluxes. If the authors think that this is the only approach to show what they intended to show I think they have to provide some explanations for this.

The next major comment concerns the methods: The authors state that they measured the fluxes in the different vegetation types "between 2002 and 2008 in episodic campaigns every three to four weeks". Did they measure all the vegetation types on the same day or in the same week or within the same months? If the week/month is true, I think they have to provide the environmental conditions during with the measurements were taken. Otherwise the fluxes obtained from one site are not comparable to the ones from the others. I know that it is difficult to hit all the sites on the same day with the chamber system, but since this likely was not possible, the results should somehow be adjusted to account for this disadvantage. Please also clarify how often and when exactly you took your measurements.

The third major comment concerns Figure 1. First of all I wonder why the authors did not use all the data they have collected. They mention in the abstract and intro that they measured the fluxes between 2002 and 2008, but the figure contains only results

from 2002-2004. Also, would it not be more meaningful to show NEE measured on a specific date over time (e.g., from 2002 to 2008) and then show when the pastures were mown/grazed and how the fluxes developed thereafter? Having said this: how did the authors assess the effect of grazing given that they mention that grazing took place from May to mid-September? Are all the filled symbols for the pastures presented in figure 1 from May to mid-September, the open ones for March/April and mid-September till November? If so, wouldn't the differences in fluxes be due to differences in temp, light etc., during the different seasons and not due to grazing? Please clarify this.

The last major comment concerns the discussion. I suggest that the authors revamp the discussion to put more emphasis on what is known from other studies on how land use (moving, grazing, fertilizing) affects different fluxes, instead of putting the main focus on how environmental parameters, LAI and biomass are affecting them. They do provide Figure 5, which gives some general insight on potential overall patterns. However, from the figure it is not clear whether the differences in values are significant. Please add. Why have natural grasslands lower fluxes than the managed ones? Were these studies conducted with eddy covariance or chambers, which would affect the values measured as you mention in your methods? Could you clarify this too? Wouldn't it be more meaningful to plot the values of all these studies eg. against elevation, air temp or precipitation, indicate which data points are M, P, A or N and then discuss how the systems differ along such gradients (maybe no difference at low elevation, but much more at higher ones?). With just the bars as in Figure 5 it is difficult to really assess what is going on in terms of land use change.

Summarizing the points made above, I think the authors should much more address what they announce in the title within the manuscript, i.e., how land use affects ecosystem CO2 fluxes.

Specific comments:

Abstract:

11436, line 10: You mention physiology. To me it is not clear from the methods how you assessed this? Can you add this?

11436, line 12: "parameters of light response curves were generally closely coupled" to what? One another? Please clarify

Introduction: 11438, line 9: you mention that you have flux measurements from 2002-08, but you never present all the data. Can you clarify?

11438, line 14-19: the hypotheses do not represent anything really novel in my opinion. Could you refocus them so they deal much more with the effects of land use change you are trying to assess?

Methods: Study sites: 11438, line 24: from table 1 it does not look as if you measured the fluxes at all the sites from 2002-2008 (see also comment above). Please clarify this in the text

Assessment of the net ecosystem CO2 exchange: 11439, line 14-15: did you sample all the sites at the same date of the year, or a week/month apart? Please clarify. If not at the same day, how did you define the order of your measurements. Valley to top? Random? Please add the exact measurement schedule to the manuscript.

11439, line 15-16: What do you mean by a "diurnal course" for each site? When did you start your measurements (time) and when did they usually end? Did you also measure throughout the night? Did you measure your plots more than once at a site? If so, how often over the course of the day/night? Did you only have one chamber system so you measured your three plots one after another or did you have three chamber systems and measured all three plots at the same time? If only one chamber, did your randomly choose with which plot to begin etc. Please add this information for clarification!

11439, line 23: since it is not clear what times of the day you measured the fluxes it is not clear why there should have been no light. Did you measure at night, or did you use shade cloths? Please clarify.

11440, line 19 – 11441, line 7: is this needed? The chamber method is a recognized method, so I don't think you have to list how they differ from the eddy flux tower measurements. Maybe it would be meaningful to mention this if the values they obtained from other papers to come up with figure 5 were adjusted.

11441, line 22 – 24: could you add some information on how you calculated LUE exactly? This is not clear from the text provided. Could you also add at the end of the paragraph what negative and positive fluxes represent in your study?

Results:

11442, 15-16: You mention that after mowing or grazing the systems released CO2 for approximately 6-10 days. I think this is a really interesting result and should be much more prominent in the manuscript since the authors want to address how land use affects the fluxes. Please see my suggestion of how to address that under "general comments". Please also explain how grazing can have an instantaneous effect on the fluxes when the systems are grazed from May – mid-September (also see general comments).

11442, 21-24: you mention that "the nutrient-rich abandoned grassland showed much higher values of NEE at any given temperature and light intensity, as compared to the nutrient-poor abandoned grassland". This is – given Fig 1 – probably true in 2003 when you measured the fluxes in both systems. However, that is the only year you have results for both grasslands. Given that 2003 was – as mentioned several times – an exception in terms of air temperatures, the statement above should probably be softened somehow.

11443, line 7 – 10: you mention that 68% of NEE, 75% of GPP and 60% of R, respectively, were explained by

PFD Air temp Soil temp Aboveground biomass LAI Grassland type Year of measurement Time of the season

Given all these parameters were used in the model – what explains the remaining 25% to 40% variability in fluxes then? I think you should discuss this at least to some extent.

11443, line 22-11444, line5: I do not understand why you use seasonal peak values from different years for this comparison (see also general comments). To me this is as if you are comparing apples with oranges. Maybe this approach is valid but then it needs some explanation why this was done and why it is okay to do this. Also, what is optimum LAI and how was it determined?

11443, line 29 – 11444, line 1: You mention that for the "ratio R/GPP there was no significant trend across the sites". However, looking at Figure 3f there are significant differences between the different land use types. E.g., the R/GPP of Mm is significantly lower than the one of An-p. Please clarify this in the revised manuscript.

Discussion: It would be nice to incorporate what is known from other studies on how mowing, grazing, fertilization affects ecosystem CO2 fluxes into the discussion, which would allow to better assess whether these mountain systems react differently to different land use than other systems (see general comments). At present this is not really done.

11444, line 26 ff: As mentioned before I suggest that you give the effects of mowing and grazing much more space in this manuscript and then also compared their findings with the ones of other studies.

Tables: Table 1: - are your MAT and MAP values measured at a nearby weather station? If not, why are the values exactly the same for all the higher elevation plots? If measured within the ecosystems I would expect that there is a difference in MAT and MAP between 1850 and 2000 meters in elevation? - what are your aboveground biomass values for spring/summer/autum that you present? Are these ranges of your different plots or years? Please clarify.

Table 2: - maybe it would be meaningful to include the information from this table into

figure 2. Also, please add what regression functions you used.

Figures: Figure 1: See suggestion in general comments Figure 2: As mentioned above maybe it would be meaningful to include table 2 into this figure and also add the regression functions. Figure 3: see suggestions in general comments Figure 5: please provide information on whether the values are different or not.

Minor comments: 11436, line 11: Exchange "(GPP)" with "(PFD)" 11460 Figure caption 3: change Pn to P (second last line)

Literature Gu., S., Tang, Y.H., Du, M.Y., Kato, T., Li, Y.N., Cui, X.Y., and Zhao, XA. (2003) Short-term variation of CO2 flux in relation to environmental controls in an alpine meadow on the Qinghai-Tibetan Plateau. Journal of Geophysical Research-Atmospheres. Volume 108, No. 4670 Kato, T., Y. Tang, S. Gu, X. Cui, M. Hirota, M. Du, Y. Li, X. Zhao, and T. Oikawa. (2004a). Carbon dioxide exchange between the atmosphere and an alpine meadow ecosystem on the Qinghai-Tibetan Plateau, China. Agricultural and Forest Meteorology 124:121- 134. Kato, T., Tang, Y.H., Gu, S., Hirota, M., Cui, X.Y., Du, M.Y., Li, Y.N., Zhao, X.Q., and Oikawa, T. (2004b) Seasonal patterns of gross primary production and ecosystem respiration in an alpine meadow ecosystem on the Qinghai-Tibetan Plateau. Journal of Geopyhsical Research-Atmospheres. Volume 109, No. D12109.

Interactive comment on Biogeosciences Discuss., 6, 11435, 2009.