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Biogeosciences Discussions

Interactive comment on "Response of temperate grasslands at different altitudes to simulated summer drought differed but scaled with annual precipitation" *by* A. K. Gilgen and N. Buchmann

A. K. Gilgen and N. Buchmann

anna.gilgen@ips.unibe.ch

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Response to the Editor

We thank the editor and the two referees for their valuable comments on our manuscript. The comments of the referees were answered separately. We hope to have responded adequately to their concerns. The indicated changes will be included in the revised version of the manuscript.

1 General comments

"The reversed response to the drought treatment is really disturbing and I was won-

dering whether the authors see a possibility to rule out the experimental artefacts suspected by reviewer #2. Maybe the authors can check soil moisture at greater depths and across the shelters in order to get a better idea."

We agree with the editor and the reviewers that it would help to check soil moisture at deeper depths to respond to this critique. However, this is impossible in retrospect, but will be kept in mind in the continuation of the experiment. Nevertheless, we think that we can exclude experimental artefacts for two reasons: (1) Plant water potentials and gas exchange displayed a water stress response, as shown in a companion study (Signarbieux and Feller, 2008; Signarbieux, 2009). We now cite the original paper and PhD thesis in the manuscript. (2) The same experimental setup was already successfully used in an earlier experiment in Central Germany (Kahmen et al., 2005).

"Alternatively I was thinking of transplating soil monoliths from Fruebuehl to the laboratory and to subject these to a drying cycle in order to confirm the "beneficial" effect of drought."

This is an interesting suggestion, but would constitute a new experiment. Since we see the beneficial response at Früebüel in three consecutive years we are confident that the reported results are no artefact. It seems rather unlikely that an artefact would occur in three consecutive years with very different weather. Actually, at the GfÖ conference in Bayreuth (September 2009), several talks and posters were presented from the EVENT experiment in Germany (http://www.old.uni-bayreuth.de/departments/biogeo/de/forschung/klimafolgen/index.htm) and a Belgium study showing no drought response in NPP of grasslands although the drought was applied during three years (75-year consecutive occurrence of drought).

"In retrospective a pre-treatment year would have been really good in order to rule out differences between the treatments not related to the treatment."

We agree that a pre-treatment year is always useful but this was not possible within this project. However, as the plots were arranged close to each other and all plots were

managed for an agronomic purpose (thus homogeneously) prior to our experiment, the possibility of a systematic difference between treatments before the experiment can safely be ruled out.

"I was also wondering about the effect of excluding fertilisation during the study period. In particular at Chamau a lot of nutrients are exported during the six cuts and according to my experience not replenishing these may quickly affect productivity. In particular since excluding precip also excludes part of the nutrient inputs via wet deposition. So there is also a difference in atmospheric nitrogen input associated with the experimental design (in addition to changes in nitrogen fixation resulting from shifts in the legume fraction)."

We agree that a lot of nutrients are exported due to the cuts. However, for several reasons we think this was no problem for this study: (1) The annual biomass produced actually increased during the course of the experiment from 2005 to 2007, showing that plant growth was not limited by lack of nutrients (Fig. 2). (2) Total soil N did not differ between treatments end of 2007 at Chamau (no data available for the other sites). Thus, despite the regular cuts, no depletion of soil N was found in our experiment. (3) This observation is supported by results from a long-term biodiversity experiment (The Jena Experiment, Jena, Germany) where grassland biomass production did not indicate any nutrient deficiency after six years (Marquard et al., in press) or now even after eight years of harvesting without fertilisation. (4) The exclusion of additional N deposition during the shelter period is implicit with all shelter experiments. However, N concentrations in the vegetation did not decrease in response to the drought treatment, thus no negative effect of this exclusion of wet N deposition was detected. We added this information in the discussion (section 4.1).

"Finally, I would like to see the questions posed in the intro being more clearly addressed in the discussion."

We have revised the discussion section accordingly.

2 Detailed comments

"p. 5220, l. 9-20: I am not sure that LAI is worth a question of its own at it is related to above-ground phytomass unless large changes in the bulk SLA occur"

We agree with the editor that, in general, LAI and above-ground phytomass are related. SLA of several species has been measured in 2006 at Chamau and Früebüel and indeed, no difference was found in response to drought (Stohler, 2006). We have therefore omitted the question for vegetation structure in the introduction but still present some results for LAI and vegetation height in a separate sub-chapter.

"p. 5222, l. 17: here and on many other occasions in the MS you are referring to productivity but actually you are reporting and discussing biomass not changes of biomass over time; when you then sum all the harvested material of each year this is rather the total harvest than productivity in an ecological sense (where you would have to quantify litter fall, herbivory losses and so forth);"

We don't understand this statement. Surely, biomass grown after one harvest until the other is growth over this time period. The term productivity is used in the ecological sense for the development of standing biomass over time (as for example also used by Weigelt et al., 2009 in this journal). Thus the quantification of all loss fluxes is not necessary since biomass harvested already represents net primary productivity (in contrast e.g. to eddy flux measurements partitioned into gross primary productivity and ecosystem respiration). To clarify this point, we rephrased the legend for Fig. 2 and the text accordingly.

"another definition issue is biomass, which to the best of my knowledge is living plant matter; attached dead plant matter thus usually is referred to as necromass and the sum of (living) biomass and necromass gives phytomass"

Unfortunately, there are contradicting definitions used of "phytomass" vs. "above-ground biomass". In our manuscript, we now define the usage of the terms relating

to biomass much better. We clarified the meaning throughout the manuscript (including Tables and Figures) and also point out the use of the term phytomass. However, in order to avoid an inflation of different terms, we do not use the terms necro- or phytomass throughout the manuscript.

"p. 5225, l. 1: Statiscal analysis"

Done.

"p. 5226, l. 4: what about temperatur maxima and minima?"

In general, the differences in temperature maxima during shelter installation were slightly higher than the differences in daily average temperature shown in Fig. 1 while no shelter effect on temperature minima was found. We added this information in the text.

"p. 5231, l. 17-26: these are original data and should be presented in the results section"

Done.

"p. 5232, l. 20: the statement regarding fertilization seems to contradict the methods section, where you state that no fertilisation occurred"

We agree that the wording was somehow unclear. We meant that due to the high fertilisation level in the years prior to the experiment there was still enough N available. We rephrased this section.

"Table 3: units are missing"

This is true, we had forgotten to indicate that all data shown in Table 3 are in g m-2. We have added this information in the table header.

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