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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

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Response to Anonymous Referee #2

1 General comments

"[. . .] However, the experiment suffered from a critical flaw in design that probably created misleading results. The rain shelters were only 3x3.5 m in area and no subsurface barriers were installed at the chambers periphery. Thus, it is highly probably that rainfall and subsequent infiltration of water in the area next to the chambers resulted in horizontal water flow under the shelters. The authors only monitored soil moisture from 5 to 30 cm so there is no way to determine if roots below 30 cm were absorbing

water that came from outside the shelter. The encroachment of water at lower depths is especially likely at sites with greater rainfall and less probable at drier sites. This is probably why the Fruebuel site always showed greater productivity under the "drought imposing" shelter (lower evaporative demand by plenty of subsurface soil water). That is, at Fruebuel, the shelters where not imposing a drought but actually creating more favorable plant water relations. The potential for subsurface horizontal water flow was less likely at the drier sites – locations that showed lower productivity under the shelter. Thus, the main conclusion of the paper is likely the result of the relative effectiveness of the shelters at sites with different precipitation. The findings are probably not an ecophysiological response to drought, but an artifact of the flawed experimental design. If different rain shelters had been used, the results might have been exactly opposite of what the authors observed."

See responses to the Editor. For easier reference, we copy/paste parts of them here:

"[. . .] 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)."

"[. . .] 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)."

See responses to the Referee #1. For easier reference, we copy/paste parts of them here:

“Indeed, we believe that too much water can limit plant productivity (e.g., oxygen deficit, redox conditions affecting nutrient availability and microbial activities, etc.). The experience of the local farmer confirms that drier years are better years at Früebüel (Fritz Grunder, pers. comm.). We now included further details on potential mechanisms. In addition, we have replaced ‘drought’ with ‘decreased precipitation input’ in the statement mentioned above.”

“We excluded similar fractions of annual rainfall for all three sites. However, the different responses among sites could only be predicted well by their total annual precipitation as shown in Fig. 6 and not by the total amount of rain excluded as suggested by the reviewer. In addition, with three sites being 200 km apart, including a very remote site, feasibility issues needed to be considered as well.”

In addition:

Within each plot, we had a buffer strip of 75–100 cm since we only sampled the inner core area of 1 $\times$ 2 m. This buffer strip was sufficient as seen in the plant physiological measurements performed in our joint project by Signarbieux (2009). This work is now cited where appropriate.

2 Specific comments

"P 5220, L 23-24. The lack of grazing or mowing of the plots would seem to have important consequences on the results and interpretation. Are not these grasslands normally grazed or used for hay production. Please explain the potential implications of not removing any aboveground biomass over the course of the study."

There seems to be a misunderstanding. Although cattle/sheep grazing on the plots was not allowed to avoid disturbance of the experiment, the biomass was harvested and removed during the study period at approximately the same dates as on the sur-

rounding meadows.

"P5221 and Table 1. More discussion of the soils and vegetation among sites is needed. Most important is the soil profiles characteristics (water holding capacity, % clay, etc). Also, it would be nice to know the historical max LAI at these sites. Please add information on rooting depth at each site."

No long-term LAI data sets are available for these sites. We added more information on the soil in Table 1.

"Table 2. Please add another column with the long-term historical avg. precipitation at each site."

There are no long-term precipitation data available for the sites. We have therefore added the information from nearby MeteoSwiss (Federal Office of Meteorology and Climatology) stations in Table 2.

"P5221. The design of the rainfall shelters appears to be inadequate. The shelters are small (3 x 3.5 m). Rain outside the shelter could have infiltrated and moved horizontally under the shelter by unsaturated or saturated flow (saturated flow is especially likely if the site had even a mild slope). The investigators only used the 1 x 2m interior for the sampling to avoid edge effects. However, it is likely that water moved horizontally under the entire sheltered area at lower portions of the profile. We have conducted experiments of this type and found that its very difficult to prevent horizontal water movement at deeper depth of the profile – even when horizontal barriers were used to depth of 1 m. The researchers indicate that soil water was only measured to depth of 30 cm so they do not know if rood below 30 cm were tapping water lower in the profile – water that could have be transported from outside the shelter. Thus, plants in the 1x2 m sampling area may not have been water stressed, especially at the higher rainfall sites. This weakness in the shelter design could have greatly confounded the results."

We agree that the shelters seem rather small. However, our personal observations and

also the measurements in this study and earlier (Kahmen et al., 2005) show that the buffers on either side of the core area are large enough. Often a clear treatment effect could be detected by eye and the ecophysiological measurements of gas exchange and water potentials showed the adequateness of the shelters to reduce precipitation input and impose a drought. For further information: see above and responses to the Editor and Referee #1. We added information from a companion study in the revised version of the manuscript.

"P5223. What was the rooting depth among sites."

We do not have spatially representative data on rooting depths but the C-horizon starts at around 120 cm at Chamau, lower than 120 cm at Früebüel (Roth, 2006) and at around 30 cm at Alp Weissenstein (pers. obs.). We added this information in Table 1.

"P5223. Did you calibrate the LI-2000 using areas outside the shelter (make LI-2000 measurements and then harvest). This is very important when comparing sites with different canopy structure and size."

No, we did not calibrate the LAI-2000 in such a way. But when taking measurement, the first value was always measured above the vegetation. Since the LAI-2000 measures light attenuation to calculate the LAI, such a calibration is not necessary in stands as measured here and typically in other ecological studies (Asner et al., 2003; Niklaus and Körner, 2004; Spehn et al., 2005). Furthermore, we did not use these measurements to infer leaf area (m2) or specific leaf area (m2/g) but to estimate leaf area index (m2/m2).

"Fig. 1. The precipitation data is very hard to view in the plots. Likewise, Tair, Tsoil, and PAR don't provide much information and not integral to the discussion. Perhaps only precipitation and soil moisture should be shown at larger scales."

We agree that the data in the figure was hard to view. We wanted to keep this figure because we believe it is important to show the microclimatic impact of the shelters but we now present the difference between sheltered and control plots for Tair and Tsoil

and the fraction of PAR reaching sheltered plots. This makes it easier to read the figure.

"P5226, L14-15. This statement seems illogical – would you not expect greater impact of the shelter in wet climates. The use of "% reductions" to describe the results is misleading, especially on a variable like soil moisture that is bounded by zero. For example, if the shelter had be used in a desert, perhaps the water content would have be changed from 10 to 5 m3/m3 – a reduction of 50 % but the effect on desert vegation would have likely been trivial. In a mesic ecosystem, the rainout shelter might have reduced water content from 30 to 15 m3/m3 – again a 50 % reduction but the effect on vegetation would have been dramatic. It would have been better to describe the drought treatment as -5 m3/m3 in the desert and -15 m3/m3 in the mesic zone. In the study, it would be better to describe the reductions in soil water in terms of mass and/or volume of the water."

We understand the concerns of the reviewer but it is a question of pools vs. fluxes. At a wet site, the soil has a large water pool within the profile and a shelter for some weeks does only reduce additional inputs into a large pool. In contrast, at a rather dry site with low water pools in the soil profile, a reduction in water input will have larger effects. In Fig. 1, we are mainly interested in the comparison between treatments and not in a full-fledged water budget model. We therefore still present relative treatment effects.

"P5227, L3. At the Fruebuel site the biomass under the shelter was always greater. It is likely that lower radiation and perhaps lower wind speeds reduced evaporative demand but undocumented horizontal movement of water under the shelter kept the plants well watered – especially those species with deeper roots. So the plants under the shelter actually had less water stress and greater productivity – opposite of the desired treatment effect."

The reviewer assumes that deep water was available to plants at Früebüel. However, plant water potential measurements showed that this was not the case (see above and

responses to the Editor and Referee #1). We have added this information in the revised version of the manuscript.

"Fig. 4, P5230. Why was the root depth pooled with the aboveground data – or does the area under zero line represent belowground biomass. If so, the number on the y-axis label shoud still be positive, not 200 to -800. Please explain."

There is no root depth included in this graph but indeed, below-zero data represent root biomass. Negative numbers were used to symbolise the growing direction. However, we understand that this was unclear and have adapted the sign of the y-axis.

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