

Interactive
Comment

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 #1

1 General comments

"The varying sampling strategies, implementation of treatments, and site management policies across each of the 3 sites made it very confusing and difficult to keep track of appropriate comparisons and responses. As a field ecologist myself, I sympathize with the perils and pitfalls of site-dependent fieldwork, but the inconsistent (or site-specific) methods and inconsistent results (or lack of treatment results) across sites made it difficult to recognize general conclusions from this research."

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Interactive Discussion

Discussion Paper



We do not agree with the reviewer. Methods and sampling strategies employed were identical at the three sites. What actually differed were the harvest dates that were set/given by the local management. However, we believe that this is a strength of our experiment rather than a problem. Cutting an extensively managed meadow (e.g. Frübüel) more often (similar to the intensively managed site Chamau) would have created a biased picture and put the whole ecosystem into a transient stage of development, something that is clearly unwanted. We do not understand the comment of the reviewer on the inconsistency since the relationship we show in Fig. 6 for all three sites and all three years is a rather good one ($R^2=0.85$) and can be used to scale our results. The 'inconsistency or lack of results' is actually part of our findings that different sites respond differently to a reduction of precipitation during summer.

"In general, the Results section needs to be condensed and presented more succinctly."

We shortened the Results section in the revised version of the manuscript where appropriate.

"What mechanism do you propose for the positive biomass response of grasses at Fruebuel during drought? The statement on line 21, page 5231 seems odd. Are you suggesting that water-logged soils and anaerobic conditions (or some other mechanism) are somehow reducing productivity?"

We think that the reviewer meant the positive community above-ground biomass response and not the response of grasses at Frübüel (since grass biomass was unaffected by drought at Frübüel). 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 Frübüel (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.

"In the second paragraph of the Discussion, root depths for the grasses and forbs are used as potential mechanisms to interpret varying community responses to drought. Grasses and forbs can have similar maximum rooting depths, but the proportion of roots at depth and the utilization of water at depths can differ markedly. It has been shown that the presence of roots at depth in grasslands is not a good predictor of water use at depth, even during dry periods (Nippert and Knapp, 2007 *Oikos* V116 1017-1029; *Oecologia* V153:261-272). Thus, it is a possibility that the forb and grass communities were utilizing water from different portions of the soil profile during the drought treatments, regardless of maximum rooting depth. Without water isotope data, it is difficult to tell."

We agree with the reviewer that the distribution of roots in the soil profile is not necessarily identical to the water uptake at different depths. We have collected preliminary water isotope data and indeed, there was no clear indication for differences in water sources of grasses and forbs. However, the replication of these isotope measurements is too low to make reliable statements on rooting patterns and especially water uptake. We removed the respective sentence in the revised manuscript.

2 Specific comments

"1. Page 5219, line 26, you missed some key citations from Alan Knapp (Colorado State), John Blair (Kansas State) and Philip Fay (USDA-ARS) who are the pioneers in large-scale precipitation manipulation experiments in mesic grasslands. Their work has shown large ecosystem level responses to manipulations in precipitation timing, independent of changes in precip amount. Check their webpages and cite accordingly."

We are well aware of the work mentioned by the reviewer. However, although Knapp, Blair and Fay work in mesic grasslands, their prairie systems are dominated by C4 plants that show a very different gas exchange response to drought and have a very different phenology compared to the C3 grasses we work with. We now cite their work in the introduction more explicitly to stress the need to also better understand the

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response of C3 dominated grasslands to reduced precipitation amounts (and not to differences in timing as mentioned by the reviewer).

"2. Page 5220, line 4: What is altitude a surrogate for environmentally (of interest)? I would assume 'dryness', and as such, yes this question has been done before (Heisler-White et al. 2008). As you've currently phrased this question, the uniqueness doesn't really appear to be that unique. I would suggest you rephrase this sentence to highlight more of the ecology and predicted response. Additionally, responses are not replicated by altitude and thus it is hard to draw general 'altitude' conclusions from this research since there is only 1 site at each altitude. Presently, it is hard to tell if the responses measured are simply a low precip. response, an altitude response, or some interaction of the two."

We agree that there were no replicates for altitude but the sites were chosen as case examples for Switzerland because Swiss agriculture covers all elevations. However, the altitudinal gradient itself was not the focus of the study and does not explain our results (see Fig. 6 where altitude is not the sorting factor). Since the whole paragraph, including the mentioned statement, was rephrased (see previous answer), there is no more mentioning of the altitudinal gradient.

"3. Why did you choose to exclude all rain from the sites for the treatment periods? Extended droughts are generally produced by reducing the amount of rainfall per event, or lengthening the periods of time between events (or both). In your simulations, the target reduction was simply a long absence of rainfall, but not a 'season-long' drought. The lack of an effect at the two lower-elevation sites may reflect the fact that the treatments and target reductions were not season-long, but in fact were only for a portion of the growing season after which rainfall amounts and timing were back at ambient. At the higher elevation site, it presumably has a shorter growing season, and the exclusion of rainfall for 6-8 weeks was sufficient to impact ecosystem responses."

There seems to be a misunderstanding. Our experimental design is based on climate

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scenarios for Switzerland that project a strong reduction of summer precipitation. Thus, we reduced "season-long" precipitation by a large fraction but did not exclude all rain during the entire growing season (see Table 2). We excluded similar fractions of annual rainfall at all three sites. However, the different responses among sites could well be predicted by their total annual precipitation as shown in Fig. 6. In addition, with three sites being 200 km apart, including a very remote site, feasibility issues needed to be considered as well.

"4. How many control plots were present at each site? Five? (the same as the number of shelters per site)?"

Yes, there was the same number of control plots present. We added this information more explicitly in the methods description.

"5. How did you keep water from moving laterally through the soil into the drought treatments? Was flashing buried around the plot boundaries to restrict water movement overland and through the soil?"

We did not prevent lateral water movement by mechanical barriers. Although in theory water might have flown into our plots horizontally, the physiological measurements of gas exchange and plant water potentials do not support this hypothesis (Signarbieux, 2009). We cite this work now where appropriate.

"6. ECH2O soil probes are notoriously finicky and problematic. How did you calibrate your probes? The data produced by these probes varies greatly by soil texture, and is sensitive for a fairly limited range of volumetric contents (not too wet, not too dry). The comparison of the vol. and grav. soil moisture data in Fig. 1 doesn't show much agreement between the two measurements. Perhaps this is just because the figure is small and one variable is continuous, but I don't see much consistency (and validation for interpreting the data from the Ech2O probes)."

We used the ECH2O probes without changing the factory calibration since we used the

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sensors for pairwise treatment comparisons to follow the development of soil moisture and not for absolute values in for example an ecosystem water budget model. The gravimetric measurements are just shifted on the absolute axis but confirm the temporal development of volumetric soil moisture. To avoid misinterpretation, we now present volumetric soil moisture as relative instead of absolute values to clarify that we were more interested in the shape of the soil moisture curve than in the absolute values.

"Also, why did the dashed lines in panel E (drought treatment) drop to <10 before the treatments were initiated (shaded section)?"

There was no rain in the weeks before the drought treatment which affected soil moisture in both, drought and control plots. We added a sentence in the results section.

"7. In section 2.3 (Aboveground productivity), did you record the number of species per plot / per site? Did richness vary over time as a function of the treatments?"

Since biomass was separated into species, information about the number of species is available at Chamau and Frübüel. We found that drought tended to reduce the number of species. However, this was only significant at Chamau in 2006 and 2007. For Alp Weissenstein, where biomass was only separated into plant functional types, a tentative list of species is available. Drought reduced the number of species significantly in 2006 but not in 2007. This information has been added to the results section.

"8. I don't understand the first sentence on page 5225. Do you mean that you used all data within a plot as an independent replicate rather than placing the statistical inference at the plot scale? If so, this is pseudo-replication and your inference is now only these plots at these few grassland sites and not the larger altitudinal gradient in Switzerland. Please elaborate on how the data was used in the analysis."

What we wanted to say was that no outliers were excluded. There was only one value per plot that entered the statistical analysis. We have clarified this sentence.

"9. A common consequence of rain-out shelters in close proximity to the ground is they

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can alter the boundary layer dynamics above grasslands. How did RH and/or VPD vary as a consequence of a “shelter-effect”? If RH was higher in sheltered plots, this may have mitigated some of the soil water losses during the treatment period.”

Unfortunately, the RH measurements failed due to technical trouble with two different approaches. However, our rain shelters were not closed down to the ground but were open all around for approximately 30 cm. Towards the main wind direction, the cover started only ca. 1.7 m above the ground. This setup, also used in an earlier experiment (Kahmen et al., 2005), ensures that the shelter-effect is minimised. We added a sentence in the methods section.

“10. On page 5227 line 9/10, is the reduction in productivity really from the drought treatment, or from the increased productivity of Rumex? This is a difficult effect to tease out and I’m not convinced that simply excluding Rumex biomass from the total sample allows you to infer a treatment response for the remainder of the productivity sample.”

Both, the lower biomass of grasses and the higher biomass of Rumex are consequences of the drought treatment. There are two different mechanisms at play under drought conditions: (1) competition among species and (2) stress due to environmental conditions at Chamau in 2006. Both are linked to each other since dominant Rumex can outcompete the grasses (Gilgen et al., 2009). However, in a companion study, plant water potentials and gas exchange of grasses were measured. These measurements showed that grasses were clearly negatively affected by the drought treatment. We added this information to the revised manuscript.

“11. What do the values in the column ‘duration’ mean in table 2?”

We meant the period of the drought treatments. We rephrased this in Table 2, now using the term ‘treatment period’.

“12. What year do the data in Fig. 4 come from?”

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The data are from 2007. We added this information in the figure.

"13. In the discussion, heavier $\delta^{13}\text{C}$ in the drought treatment are used to infer water-savings from greater stomatal regulation. It is somewhat difficult to interpret data from Alp Weissenstein since there was only 1 sampling each year. However, since this was the only site to illustrate a reduction in biomass from the treatment effects, it may be interesting to discuss the somewhat marginal differences in $\delta^{13}\text{C}$ between treatments at this site. Perhaps stomatal regulation is lower at this site?"

We did not dive more deeply into these issues due to space constraints in the manuscript and because it was not the main focus of the study. We tested the resource use explicitly with *Rumex* at Chamau (Gilgen et al., 2009), ecophysiological variables were tested within our companion project (Signarbieux, 2009).

"14. In Fig. 6, what is the difference in the solid versus dashed lines?"

The dashed line shows the regression without *R. obtusifolius* at Chamau. This is explained in the legend.

"Are you proposing a causal response between reductions in precip and changes in biomass?"

In Fig. 6, we show a strong relationship ($R^2 > 0.73$) between the total annual precipitation at a site and the relative change in biomass production caused by a reduction in precipitation. The relationship with the amount of reduced precipitation is not shown in Fig. 6. Nevertheless, we added this information in the text, despite the fact that this relationship is not significant ($R^2 = 0.33$, $P = 0.08$).

"The data in Fig. 4 doesn't suggest such mechanism exists. As such, is Fig. 6 simply showing that a precipitation*biomass gradient exists along the altitudinal gradient? If so, is that response worth a figure in this manuscript?"

We do not understand the comment of the reviewer. Fig. 4 shows the response in 2007 only while Fig. 6 presents all three years at all sites. Furthermore, Fig. 6 shows the

annual relative drought response (as % change), while Fig. 4 gives the absolute values in 2007. Fig. 6 does not show an altitudinal gradient, since the site at the intermediate elevation shows the largest annual precipitation and a beneficial response to reduced precipitation inputs. We decided to keep Fig. 6 since we think it offers a simplified summary of the wealth of data presented in the manuscript and might be useful for the modelling community.

3 Technical corrections

"15. First sentence page 5218, replace 'live' with 'life'."

Done.

"16. Line 24 page 5218 and line 27, page 5234: what is an 'adaptation measures'? A management strategy?"

Yes, adaptation measures means changes in management strategy here. We replaced 'measures' with 'management strategies'.

"17. Page 5219 line 1, use of the word 'probably' is weak and needs to be rephrased."

We now say 'among the' instead of 'probably'.

"18. page 5220, line 7. Focused is misspelled."

Corrected.

"19. page 5220 line 9: replace "react in accordance" with the word 'scale'."

This sentence was removed.

"20. page 5223, line 3: replace "an own. . ." with "its own"."

Done.

"21. Page 5227, line 21, 26, focused is misspelled."

Corrected.

"22. Focusing is misspelled on page 5228, line 10"

Corrected.

"23. Line 26, page 5232. Starting a paragraph with "But..." is very awkward."

We agree with the referee and have rephrased the sentence.

"24. Line 15, page 5233 add the word "integrated" before WUE."

Done.

"25. Line 24, page 5233, delete 'also'."

Done.

References

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