Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-100-RC2, 2020

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Timing of drought in the growing season and strong legacy effects determine the annual productivity of temperate grasses in a changing climate" *by* Claudia Hahn et al.

Anonymous Referee #2

Received and published: 24 May 2020

The article presents the results of a seasonal drought manipulation experiment in Swiss grasses (six species) carried out in the growing seasons of 2014 and 2015. Specifically, results from three different rainfall exclusion strategies are presented: spring, summer, and fall rainfall exclusion subdivided in periods of 10 weeks each, as grass is harvested 6 times per year resulting in 6 growth periods. Nutrients were added to control and experimental plots. Beyond aboveground biomass harvest, root biomass, soil water potential, and meteorological conditions were also measured. The results show relatively minor difference across grass species. In relative terms, drought effects are

Printer-friendly version

Discussion paper

more pronounced for summer and fall treatments, while aboveground biomass is less affected by drought treatment during spring and root biomass is overall not affected. The study also shows that positive legacy effects can largely compensate for the reduction in aboveground biomass production during dry periods, leading to similar annual total aboveground biomass production between control and treatment scenarios.

The presented topic is interesting as there are not many seasonal drought studies, the experiment and results are clearly explained, and the manuscript is well organized. The fact that grass in treatment plots after the drought treatment outperformed the growth rates of the grasses in the controls for extended periods of time, suggesting a considerable resilience, is definitely an important result. However, while results are interesting, it is difficult to go beyond what has been observed and learn specific mechanisms (e.g., Line 378-380), as not many physiological variables are measured, e.g., the effects of drought on photosynthesis and stomatal conductance are not reported or maybe not observed (even though a mention to a manuscript in preparation is made). Additional physiological observations could have been useful to enter the debate of carbon source vs sink limitations in growth, which is very much active (e.g., Körner 2015). Potential explanation for the physiological mechanisms (e.g., osmoregulation) explaining the higher drought resistance of the investigated grasslands in spring and the capacity to compensate for growth after drought treatments could not be investigated in the article and are only speculated. Considering that any field or numerical experiments comes with limitations, I might be satisfied with these speculations.

What it is much less satisfying, is that the key question coming from data is left unanswered. Using the data in the article (see Fig. R1), we can clearly see that the ANPP sensitivity to growing season precipitation in the control scenarios is much, much larger than during drought treatments. This is not the first time, I see such type of “mechanistically unexplainable” behavior in field manipulation experiments. Now, the question is what is happening in “nature” that is not happening in the drought treatments? If the authors will add data from similar ecosystems (from literature) – something I would rec-

Printer-friendly version

Discussion paper

ommend to increase the outreach of the article - to the two observations, they will likely find a considerable sensitivity of grassland ANPP to precipitation for the natural rainfall regime. However, the sensitivity is very different in the treatments, even though at a lower “rainfall amount” sensitivity would be expected to even increase further rather than decrease (e.g., Huxman et al 2004). This result is somehow embedded in Fig. 9 and partially explained/discussed in 4.4 as a positive legacy effect. However, it is never presently as clearly as in Fig. R1 and of course, it leaves a big question mark on the representativeness of the entire study for real conditions. My explanation in such cases, it is typically that rainfall manipulation experiments have scale issues (lateral/vertical) that leads to such type of behavior. The authors have surely done their best to avoid any artifacts, but it remains the fact that the sensitivity they observe is completely different from the real sensitivity (but of course more years will be needed for a proper conclusion). This poses serious challenges on the extrapolation of the results to the real world. Some of the variability of ANPP can be ascribed to conditions other than precipitation, but it is difficult to find any convincing mechanistic explanation why ANPP sensitivity should be so different, and as this is unlikely what one observes in natural conditions, I am left with more doubts than answers.

Minor Comments

Line 66-77. There have been a number of publication from a drought experiment in a grassland in a similar environment near Innsbruck (e.g., Fuchslueger et al 2014; 2016, etc.), which can be relevant for this article.

Line 50. See also Paschalis et al 2020 for a recent analysis of model performance compared to rainfall manipulation experiments.

Line 83, 140-150 181-182. I know that it is very common to refer to grassland ANPP to the sum of harvested biomass throughout the year or the growing season. However, strictly speaking ANPP should be computed based on the continuous (flux) productivity allocated aboveground, i.e.., including also any turnover of biomass that might occur

Printer-friendly version

Discussion paper

between two harvests and also the change in biomass below the 7cm cut height. I think for grassland in Switzerland the difference might not be very significant but if the drought lead to some grass wilting and litter production, there could be some difference. Overall, I think it would be good to clearly mention that what is referred to as ANPP is not the “flux ANPP” but an estimated based on harvested biomass.

Line 131. Evapotranspiration is not a variable which is directly observed. How did you get the estimate? Which equation/method has been used to derive evapotranspiration?

Line 134-135. How many sensors were installed? How they were distributed? Could you be a bit more precise?

Line 136-137. While from a practical point of view, I agree with the authors, theoretically if transpiration among species differ also the soil water potential will differ especially in prolonged dry periods.

Line 227. Each different plant species or sometime even different individual of the same species will have a different “wilting point”. I know that -1.5 MPa is (wrongly) a textbook reference number, but I would strongly suggest avoiding to indicate a “single” wilting point value.

Figure 4, 5 and 6. Maybe, all this information can be combined in a single Figure, especially Fig. 4 and 6.

Line 296-301. Please use (or not use) consistently the minus for a reduction in biomass. Now sometime is positive and sometime is negative.

Line 416-417. See also De Boeck et al 2018, who studied a not too dissimilar ecosystem even though at higher elevation.

Figure 1. I think this figure can be clearly improved adding a temporal axis with the proper dates and spacing between the harvests. Now, it is very conceptual and there is no reason as this is not a proposal but an experiment, which has been already carried out.

References

Fuchslueger L, Bahn M, Fritz K, Hasibeder R, Richter A. (2014). Experimental drought reduces the transfer of recently fixed plant carbon to soil microbes and alters the bacterial community composition in a mountain meadow. New Phytologist 201: 916–927 Fuchslueger, L., Bahn, M., Hasibeder, R., Kienzl, S., Fritz, K., Schmitt, M., Watzka, M. and Richter, A. (2016), Drought history affects grassland plant and microbial carbon turnover during and after a subsequent drought event. J Ecol, 104: 1453-1465. doi:10.1111/1365-2745.12593

Paschalis A., et al. (2020). Rainfall-manipulation experiments as simulated by terrestrial biosphere models: where do we stand? Global Change Biology 26(6), 3336-3355, doi.org/10.1111/gcb.15024

Huxman, T. E., et al. (2004), Convergence across biomes to a common rain use efficiency, Nature, 429, 651–654.

De Boeck HJ et al. (2018). Legacy Effects of Climate Extremes in Alpine Grassland. Front. Plant Sci., 30 October 2018 | https://doi.org/10.3389/fpls.2018.01586

Körner C. 2015. Paradigm shift in plant growth control. Current Opinion in Plant Biology 25: 107–114.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-100, 2020.

Printer-friendly version

Discussion paper

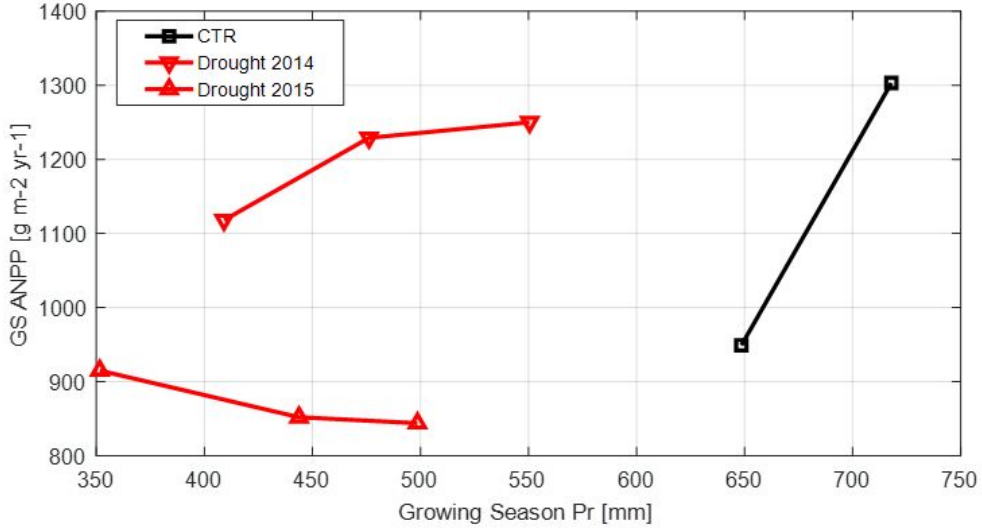

Fig. 1. Fig R1. Growing season ANPP vs growing season precipitation for the two control years (2014, 2015) and the six seasonal drought treatment carried out by the authors.

Printer-friendly version

Discussion paper