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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.

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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 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.

Response: We thank the referee for this overall very positive evaluation. We have assessed ecophysiological variables in four out of the six species/cultivars (conductance, pre-dawn and midday water potential). These data will be presented in a different manuscript that is currently in the final stages of preparation. It was a strategic decision not to include physiological data in the current manuscript but to focus on the reported biomass patterns. We agree, however, that the reported patterns alone only

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allow to speculate about the mechanisms. These will then be discussed in the second manuscript. Given the wealth of data that we present (biomass data from six harvests from 192 plots from two growing seasons), we did not want to overload this paper and are convinced that the reported patterns are yet interesting and valuable.

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 recommend 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.

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Response: The referee raises an important point. We did, however, consider this when planning the experiment. The main reasons for this discrepancy most probably are:

(i) Between the two years not only precipitation differs, but potentially a lot of other abiotic (e.g. temperature, frost events) and biotic (e.g. diseases, soil microbial activity, age of the sward) differ. This is the reason why experiments to study drought effects need to compare drought stress treatments with a rainfed control under otherwise exactly the same conditions.

(ii) The timing of a lack of precipitation is crucial. This can nicely be demonstrated by the response of the grasses to 100

Consequently, we are convinced that such a comparison of whole growing season precipitation differences among years have only very limited validity to explain drought response.

We are also convinced that our treatments did not induce important artefacts. The shelters were open on all four sides and on top to guarantee good airflow. Gutters guiding the water away from the plots and not harvested plot borders of 75cm width can guarantee, that lateral water flow did not affect the studied centre of the plots (as do the soil water potential measurements).

We can help to explain the reason why the response to the whole growth precipitation difference looks so big in the figure of the reviewer. Firstly, during the 10 weeks of the spring treatment in 2015 precipitation was exceptionally high +130 mm higher than in 2014 (Table 1). In contrast, the summer and fall periods 2015 were exceptionally dry with -195 mm less precipitation than in 2014. This had a huge effect on growth as it was in a crucial time period and because the soil water deficits lasted very long (Figure 2). There are now two effects that make the annual comparison so impressively responsive: First, the difference between the +130 mm and the -195 mm looks like a very small difference in growing season precipitation and second, the effect on plant growth was huge because the soil water deficit lasted so long (about 20 weeks), what

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is much longer than the second 5 weeks of our drought treatments.

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.

Response: We now include these references.

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

Response: We now include this reference.

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 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.

Response: Thank you for this advice. We now define the reported ANPP values as "standing above-ground 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?

Response: We now include a reference in the text to clearly indicate the origin of these data.

Line 134-135. How many sensors were installed? How they were distributed? Could

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you be a bit more precise?

Response: We installed 32 sensors that were randomly distributed among the plots. We now clarify this in the text.

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.

Response: We agree. Nevertheless, in a previous study we assessed soil moisture decline at the same site in monocultures of the same species assessed here and found no differences. We therefore feel that the transpiration is comparable across plots with different species. We also compared the soil water potential values obtained from 32 plots in this study and found no species-specific effects suggesting mostly identical transpiration rates.

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.

Response: This comment is correct. Please be aware, however, that we use the permanent wilting point to assess from where onwards a treatment is experiencing critically low levels of soil moisture. While using a single threshold for all species/cultivars might add some uncertainty for across species comparisons, we would like to emphasize that our main focus is on the across season comparison of drought effects. A slight under or over estimation of the permanent wilting point would thus merely introduce a systematic effort that should not influence the overall outcome of our analysis.

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

Response: We actually had larger figures with more panels in a previous version of

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Discussion paper

the manuscript. In the end we decided against this as the figures as they are right now already contain quite a lot of data (already 6 and 12 panels). We are afraid that expanding the figures further would make them more difficult to comprehend. Figure 5 was deleted to be more concise. In addition, we do not see how figures 4 and 6 could be combined, because figure 4 gives all harvest (but averages the six grasses) while figure 6 gives all six grasses but only one single harvest.

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

Response: We followed this suggestion and now consistently use the minus symbol for negative changes.

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

Response: We included De Boeck et al 2018 in the text.

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.

Response: As the time span between the harvests was always five weeks the spacing in the figure is actually the proper temporal spacing during the experiment. We feel that a time axis and dates would not add substantial information as this is given in figure 2.

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