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Discussions

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 #1

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GENERAL COMMENTS

The authors performed a very interesting drought experiment on temperate grass monocultures, in which they assessed the effects of timing of drought on resistance, recovery and overall, annual (A)NPP. They found the lowest drought sensitivity in spring, and overcompensating post-drought growth which to a substantial extent cancelled out immediate impacts of drought on overall ANPP. By unravelling global change effects (drought) on components of ecosystem function (ANPP), the study matches the scope of the journal.

Overall, I am very enthusiastic about the design of the experiment with consideration of resistance, recovery and overall perturbation, the details and reporting of measurements performed, and the consideration of both absolute and relative effects. I found the text also very well structured and well written.

Despite my positive evaluation of the manuscript, I recommend major revisions at this stage because of three main issues to be resolved, and further comments below:

âĂć Based on limited analyses on precipitation data and discussion on the course of soil water potential, the authors suggest that differences in spring vs summer vs fall soil moisture were likely not the main reason for the lower drought sensitivity in spring. While I tend to agree with the authors, they actually have all the necessary data to provide stronger, formal evidence that soil moisture stress was not particularly higher in summer and autumn than in spring. Based on the daily soil water potential, the field capacity and permanent wilting point, I recommend the authors to estimate daily soil moisture stress (Is), as explained in for example Vicca et al. (2012). In brief, with the approach proposed in that letter, plants or soil biota experience drought stress when soil moisture drops below a certain threshold, e.g. relative extractable water below 0.4. The amount below that threshold determines the severity of stress, and stress values for multiple days can be summed so you can get an idea of integrated soil moisture stress (e.g. for spring vs summer vs fall). I recommend the authors to calculate Is and report on their findings in the manuscript to strengthen their message, if confirmed. I also suggest to use Is in Table 2 and Figs. 4 and 9b. See also my references to this point in some of the specific comments below. Please avoid adding more display items, since four tables and nine figures is already at the higher end.

âĂć While the design and procedures done in the experiment itself were well explained, the study still lacks reproducibility in the sense that no data nor R-script were provided in a supplement or link along with the manuscript. Ideally, both the data and a comprehensive script with the main code for statistical tests are uploaded. If it is not possible to make data publicly accessible, the authors need to explain that in a section "Data

availability" at the end of the manuscript. See also BG's data policy.

âĂć Test statistics (F, df, P, ...) were not always presented along with the results in the text/tables/figures. If there is no place in figures to provide such information, please place tables in a supplement and refer to these in figure captions and in the Results section.

SPECIFIC COMMENTS

Line 14 – Here and throughout the paper, the authors refer to "resilience" to refer to post-drought recovery. In line with a proposal for standardized nomenclature and quantification of resilience proposed by Ingrisch & Bahn (2018), I suggest to replace "resilience" by "recovery" when specifically post-drought growth was meant. The overall "resilience", or the opposite, "perturbation", then combines both the "resistance" and "recovery" phases, resulting in e.g. the annual outcomes (see also Ingrisch et al., 2017 for an example).

Line 25 – From your experiment, you found that "(i) the resistance of growth rates in grasses to drought varies across the season and is positively correlated with growth rates in the control". While I agree that the first part of this claim will often be correct, I think there may in practice be many cases where drought resistance of growth – expressed either as absolute or relative values – will not correlate positively with control growth rates. For example, in an agricultural setting, N addition can promote plant growth under sufficient water supply (drought control), while it can exacerbate impacts of drought and thus reduce resistance (Wang et al., 2020). I suggest you either remove the second part of the sentence, or emphasize that it cannot be generalized.

Line 30 – Maybe the emphasis on Europe only is not needed in this paragraph. What about climate projections and ecosystem services of temperate grasslands elsewhere?

Line 65, 89, 90, ... – Suggestion to replace "resilience" by "recovery", see above.

Line 97 – Actually there were different cultivars of four grass species in total.

Line 166 – Were temperature sums calculated based on treatment-specific temperature measurements, as referred to in Table 2 and Line 130? So in other words, Tsum was slightly higher for the drought treatments than the controls? Please add this information.

Line 188 – Does PPT(ctr) include the few +20 mm watering events?

Line 188 – Please also quantify and compare S per unit change in soil moisture stress (Is) – see general comments.

Line 192 – You explain here the statistical analyses carried out. However, to improve reproducibility, I highly recommend you to (i) upload the data in the supplement/provide a link to the data (if allowed to share open-access), and (ii) provide a simple R-script with the code for the main analyses.

Line 194 – The word "regression" suggests that curves were fitted, while in fact only differences among levels in factors were assessed (i.e. ANOVA). While regression and ANOVA are statistically equivalent, I propose to replace "regression" by "models".

Line 198 – Plot was used as a random factor to take into account that the very same control plots were used for contrasting against different treatment plots in spring, summer and autumn. Should plot also be nested in grass and/or treatment? I did not think this through though, maybe it is redundant. Please comment.

Line 206 – Did R^2 refer to marginal or conditional R^2? If it does not apply here, please explain. Otherwise please provide both.

Line 212 – Unclear. You refer here to one-way ANOVA, after which two factors ($\sim$ two-way ANOVA) are mentioned. Please resolve.

Line 217 – Please mention and cite all R-packages used, e.g. for calculating and analyzing mixed-effects models, ...

Line 290 – Did you average first and then perform one-way ANOVA? See also my

comment on line 212. Please clarify.

Line 302 – “Drought (severity)” was defined here as precipitation reduction. Please check and report this also when expressing drought as a soil moisture index (Is). See general comments.

Line 305 – The statistical significance of the results (e.g. F- and P-values with df) is not given, here but also for other figures. If you think adding such details, even * symbols, would make figures confusing, then provide tables with statistics in a supplement and refer to those, mentioning on the significance in the Results section.

Line 308 – Suggestion to replace “resilience” by “resistance”.

Line 320 – Not sure if you now have enough BNPP data to here and elsewhere claim that all changes in NPP will be equal to changes in ANPP. Perhaps it is safer to consistently refer to ANPP. As you mentioned in the section on root biomass (which I nevertheless recommend you to leave in the manuscript!), only the 0-14 cm layer was sampled. Maybe below more root biomass was produced during/after drought. Or would you suggest that this would in any case be negligible in magnitude compared to ANPP? Please comment.

Line 322 – See my comment on the Abstract about the positive correlation resistance $\sim$ control plot growth.

Line 327 – Please replace “climatic” by “meteorological” or “environmental”. Climate rather refers to long-term statistics of the weather, not weather and soil moisture differences between two years.

Line 341 – Also here, it would be interesting to report on an integrated soil moisture stress index, besides/instead of median soil water potential. I then would expect Is to be significantly higher for 2015 than 2014.

Line 341 – I assume the median was taken because of the non-normal distribution of soil water potential data. However, to what extent is the median informative for any

reduction in growth? Or is it rather water potential values below a certain threshold that will affect growth?

Line 382 – Whether you consider differential soil moisture depletion among seasons as an artefact or not will depend on your point of view on the research questions. On the one hand, slower soil moisture depletion in spring than summer is something realistic that could be expected in many situations. On the other hand, it makes the unravelling of the mechanisms underlying lower drought sensitivity in spring more complicated. Please rephrase the “artefact” part.

Line 386 – Replace “herbs” by “forbs”. Herbs include both forbs and graminoids. De Boeck et al. (2011) included only forbs in their experiment.

Line 388 – Here, I want to see reference to a formal test of differences in soil moisture stress. See also in the section with general comments. Note: it may be that soil moisture stress was significantly higher in summer than spring, but was still in the same order of magnitude. So this would not necessarily invalidate your suggestion that soil moisture alone could not explain the observations.

Line 397 – It seems that nowhere summarized data nor statistics were shown for root biomass per species/cultivar. Please provide such information in a supplement, and briefly refer to it in the Results section as well.

Line 426 – Besides N, also the availability of other nutrients like P and K can increase substantially after drought (see e.g. Van Sundert et al., 2020). These may have played a role as important as N, especially since N was added multiple times a year to minimize N limitation, whereas P, K and Mn were only added at the beginning of the growing seasons. Related to that, we could even speculate that P, K, ... were depleted because of harvests over the year, and perhaps a suboptimal P/K status contributed also to the increased drought sensitivity in summer and autumn. This last part is just a thought, I do not expect you to elaborate on this extensively in the manuscript, but please incorporate briefly the role and release of other nutrients in the text.

Line 453 – Please replace “resilience” by “recovery”, see other comments.

Line 456 – Do not show statistics in the text of the Discussion section, unless absolutely necessary. Also, when P = n.s., I still prefer to see the actual P-value.

Line 484 – Refer here to more formal analyses, showing there was (almost) no soil moisture stress during this first growth period.

Line 710 – Better write “precipitation” instead of “rainfall”. Maybe sometimes precipitation fell as snow or hail?

Table 2 – It would be interesting to see integrated soil moisture stress added to this table, or instead of median soil water potential.

Figure 4 – For this and other figures: I am not sure how easy or difficult it would be for a color-blind person to distinguish between the red and green. Consider using another color code.

Figure 4 – I am somewhat surprised to see that the + and - error bars in panel a have the same length, while the Y-axis was transformed. Is it because the transformation of the Y-axis was the same as the Y-variable in the analysis (e.g. ln)? And this was not the case for panel b then? Please explain or correct if necessary.

Figure 4 – Could you make this graph also for soil moisture stress, and then discuss whether change in growth followed change in stress.

Figure 5 – As indicated elsewhere, I am not a huge fan of this graph because correlation does not imply causation. While it is true that in your study, drought sensitivity of growth was highest when control growth was high, we cannot conclude in general that, where/when growth without water limitation is high, also drought resistance will be maximal.

Figure 7 – So did you first average the four plots per species, and then calculated mean plus se by combining the four species and taking n as 4? Or are these mixed model

outputs? This also applies to some other figures where multiple species were pooled. Please explain.

Figure 9 – Am I correctly interpreting that sensitivity did not significantly differ among seasons (no statistics shown)?

Figure 9 – I would like to see the sensitivity expressed per unit soil moisture drought stress, not only per mm of precipitation.

TECHNICAL CORRECTIONS Line 15 – Replace “, thus,” by “eventually” or alike.

Line 91 – drought-stressed

Line 114 – Please remove “see”.

Line 309 – Replace “, thus,” by “eventually” or alike.

Line 309 – drought-induced reductions?

Line 445 – “Both could have contributed to increased growth rates (...)”

Line 459 – There is twice “the fact that” in this sentence. Please rewrite.

Line 500 – “lead to”?

REFERENCES

Ingrisch, J., & Bahn, M. (2018). Towards a comparable quantification of resilience. Trends in Ecology & Evolution, 33(4), 251–259.

Ingrisch, J., Karlowsky, S., Anadon-Rosell, A., Hasibeder, R., König, A., Augusti, A., Gleixner, G., & Bahn, M. (2018). Land use alters the drought responses of productivity and CO2 fluxes in mountain grassland. Ecosystems, 21(4), 689–703.

Van Sundert, K., Brune, V., Bahn, M., Deutschmann, M., Hasibeder, R., Nijs, I., & Vicca, S. (2020). Post-drought rewetting triggers substantial K release and shifts in leaf stoichiometry in managed and abandoned mountain grasslands. Plant and Soil,

448, 353-368.

Vicca, S., Gilgen, A. K., Camino Serrano, M., Dreesen, F. E., Dukes, J. S., Estiarte, M., Gray, S. B., Guidolotti, G., Hoeppner, S. S., Leakey, A. D. B., Ogaya, R., Ort, D. R., Ostrogovic, M. Z., Rambal, S., Sardans, J., Schmitt, M., Siebers, M., van der Linden, L., van Straaten, O., & Granier, A. (2012). Urgent need for a common metric to make precipitation manipulation experiments comparable. New Phytologist, 195(3), 518–522.

Wang, Y., Huang, Y., Fu, W., Guo, W., Ren, N., Zhao, Y., & Ye, Y. (2020). Efficient physiological and nutrient use efficiency responses of maize leaves to drought stress under different field nitrogen conditions. Agronomy, 10(4), 523.

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