Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-100-AC1, 2020

Biogeosciences 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.

Claudia Hahn et al.

claudia.hahn@unibas.ch

Received and published: 26 September 2020

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:

1) 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.

Response: Thank you for this suggestion it confirms and helps to strengthen our interpretation of the results and, thus, the story of the paper. We followed the instructions of the referee and calculated Is according to Vicca et al. (2012) and present these data in table 2. As suspected by the referee, the resulting Is data confirm the cumulative soil water potential data that we report for the individual seasons and years. Our interpretation that "differences in spring vs summer vs fall soil moisture were likely not the main reason for the lower drought sensitivity in spring" is thus confirmed by this additional analysis.

2) 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.

Response: All data and R scripts and now provided in a separate link.

3) 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.

Response: We now added analytical statistics in additional tables in a supplement for figures 5, 6, 7 and 8 (updated figure numbers), and added test statistics (p-values) to the text. For figures 3 and 4 we did not add additional statistics because (i) these are two figures to give the reader the overview over the time course of plant growth over all the six harvests, (ii) the analyses of the individual key harvests are given in in tables 3 and 4, and (iii) compared to the huge differences in growth among the harvests, the standard errors are so small, that an additional table would not deliver additional information. Figures 5, 6, and 7 both comprise a panel for relative and absolute changes of the response variable. Here, all statistical analyses have been done with natural log transformed data, which was needed to meet the assumptions of the models. The analyses thus match panels a) in Figures 5, 6, and 7. Panels b) with the absolute changes complement this information and are given for a better understanding of the system, and the text in the Results contains descriptive means. Given this situation, it is neither needed nor common to provide further statistics, as

the relevant analyses are all done with the transformed data (which was indicated by the data itself). It would also be hard to find an appropriate parametric model for the absolute changes, given the distribution of values; and nonparametric methods are not available for multifactorial data structures. We hope that the referee kindly agrees to this strategy.

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).

Response: We followed this advice and replaced "resilience" with "recovery" throughout the manuscript according to Ingritsch et al. 2017.

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.

Response: We agree with the comment by the author that our study does not allow to conclude that highly productive grasslands are more drought resistant than low productivity grasslands. We do show, however, that the grass species and cultivars that we investigated are more drought resistant in the phenological stage of highest productivity than in the other phenological stages where productivity is much lower (Fig. 3a). To clarify this, we re-wrote this statement in the abstract and in the discussion and deleted figure 5 as suggested by the referee.

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?

Response: We agree and therefore deleted the entire first paragraph.

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

Response: Resilience was replaced by "recovery" here and throughout the manuscript.

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

Response: We corrected this and now talk about four species of which two were grown in two cultivars.

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.

Response: Temperature sums were calculated based on treatment-specific temperature measurements. Thus, Tsum was slightly higher for drought treatments than for controls.

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

Response: PPT(ctr) includes the +20 mm watering events.

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

Response: We decided to delete sensitivity from figure 9 (independent of whether

calculated per mm precipitation reduction or per Is). The reason is, that it is related to another time span (annual) as well as to another basis of comparison (mm precipitation or Is) than all the other drought responses we present (individual harvest, absolute and relative loss of biomass). Due to the annual time span, it is an unfair comparison of the treatments spring, summer and autumn drought, because autumn drought has no "chance" for a compensation during recovery. In addition, changing time span and basis of comparison might lead to confusion and this new sensitivity detracts attention from the main message of the paper.

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.

Response: As explained above, we now provide these data. Since we describe the statistical analysis in detail in the manuscript, we prefer not to upload the R codes directly (which is also not required according to the journal policy).

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".

Response: We replaced "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.

Response: The data matrix was coded so that each repeatedly measured plot was assigned an individual identifier. Under this condition, the lme() function in R correctly calculates the respective variance component given the structure of the fixed effects “grass” and “treatment”. It is neither needed nor appropriate to nest the plot variance within “grass” and/or “treatment”.

Line 206 – Did R2 refer to marginal or conditional R2? If it does not apply here, please explain. Otherwise please provide both.

Response: The marginal and conditional R2 are now provided in all summary tables of the mixed-effects analyses.

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

Response: This has been clarified (it was a two-way ANOVA), and we apologize for the typo.

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

Response: Done.

Line 290 – Did you average first and then perform one-way ANOVA? See also my comment on line 212. Please clarify.

Response: The ANOVA was performed on un-averaged data. This is now clarified in the new table A2 in the supplement.

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.

Response: As explained above, we decided to delete annual sensitivity from figure 9 altogether.

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.

Response: We agree and did add more details on statistics in tables in the supplement. More explanations are given above in the response to the general remark of R1 above.

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

Response: We guess it should read: replace “resilience” by “recovery”. We changed throughout the manuscript.

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.

Response: We follow the suggestion of the referee and replaced NPP with ANPP throughout the entire manuscript.

Line 322 – See my comment on the Abstract about the positive correlation resistance. Control plot growth.

Response: This refers to the phonological stage (see chapter 4.2) of the highest growth rate which we now clarify in the text (and deleted figure 5). See more detailed response above (response to comment in line 25).

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.

Response: We replaced “climatic” by “meteorological” as suggested.

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.

Response: In this section we report on soil water content and how it varied and not stress. We would thus like to not discuss the stress indicator here but we added Is to table 2 and discuss it in different parts of the discussion.

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?

Response: Stress is the product of duration of the stress and the intensity of the stress. Using the median was the best choice for us to combine both components of stress as good as possible in a single value. We decided against using arithmetic mean soil moisture values as it would potentially bias values towards a few extreme values and thus overemphasize soil moisture stress. Compared to the median of soil water potential, the metric of Is has the disadvantage that it is a yes / no response and does not take into consideration the increasing stress severity with soil water potential further decreasing over a the threshold of 0.4 MPa. Presenting both values (median of soil water potential and Is) has now the advantage that both can be seen by the reader. Interestingly, the values of the two variables (table 2) are highly correlated.

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.

Response: We follow this suggestion by deleting "artefact. The sentence now reads:

"An alternative explanation for different immediate drought effects on growth rates throughout the growing season are different experimentally induced drought severities throughout a growing season"

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

Response: We replaced "herbs" by "forbs".

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.

Response: Due to not measuring soil moisture in all the replicates, statistics with significant levels is not possible. However, the metrics for stress severity presented in table 2a and 2b are impressively demonstrating that drought stress in summer was not more severe than in spring. Values for soil water potential median are for 2014 -1.44 MPa and -1.44 MPa for spring and summer respectively while they were for 2015 -0.77 MPa and -0.83 MPs. For Is the values were 33 and 33 for spring and summer in 2014 and 14 and 4 in 2015.

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.

Response: The summary tables of these analyses are now provided in the supplement (Tab. A1) and we refer to these tables in the text.

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.

Response: We now discuss the relevance of nutrients in more general terms and added the suggested reference van Sundert et al. 2020.

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

Response: We replaced “resilience” by “recovery” here and throughout the manuscript.

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.

Response: We deleted the stats.

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

Response: We now refer to figure 2 and table 2 (ab) where we show this.

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

Response: We changed “rainfall” to”precipitation”.

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

Response: We added integrated values for Is.

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.

Response: We tested the colors with the tool “Color Oracle” to check if they are distinguishable for color-blind people. They are distinguishable for all 3 types of clolor.blindness.

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.

Response: Note that this is now figure 5, and the comment also applies to figures 6 and 7. Panel a) and b) have indeed not the same underlying scale. In panel a) the intervals have equal distances on the ln scale (with matches the parametric analyses, as suspected by the reviewer); correspondingly, the length of the error bars is the same in + and - direction. Next, these ln values are expressed in “percent change” (linear transformation from ln values, without changing the scale!) because this is more reader friendly, and it is then reasonable to specify a range of percent values in straight numbers (here e.g. 50, 100, 150, or -25, -50, -75). If now the intervals of these percent scales are evaluated, it turns out that the percent change of the error bars in + and – direction is not equivalent, although the plotted length is. Thus, the interpretation of errors fully matches the asymmetric errors bars, if the data (and the ln scale) would be back-transformed to linear scale. In the panel b) the scale is simply linear and means and standard errors are based on the absolute changes of the data without any transformation. Our approach is common practice, as e.g. can be seen in Figure 2 of “Schneider MK et al. (2014) Gains to species diversity in organically farmed fields are not propagated at the farm level. Nature Communications, 5.”

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

Response: We like this suggestion. In fact in a companion paper (Hahn et al. in prep), where we report the physiological responses of the investigated grass species and cultivars to drought stress in spring, summer and fall we plot the physiological stress response over stress intensity experienced. In the current manuscript we prefer not to do this because we feel that the manuscript is already quite long with 9 figures and that an additional analysis would not really contribute to the overall findings we would like to report.

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.

Response: We agree and deleted figure 5. Nevertheless, we would like to keep the message, that plants were most drought resistant during the most productive phenological stage in the growing season. However, the information that growth rate was much higher in the second regrowth than in the 4th and 6th regrowth (by a factor of 2 to 8 times higher!) can easily be depicted from figure 3. In addition, we now clarify in the text that this does not suggest that productive grasslands are more drought resistant than non-productive grasslands.

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.

Response: Yes, we first averaged the replicates per species and then took n as 4 representing the different species. The means and SEs are calculated from raw data (as was done in all figures). Doing so, no specific indication is needed. If we would have presented model predictions, we would have indicated this with e.g."predicted values from the model".

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

Response: We did delete annual sensitivity from figure 9 (old number). The main reason is that sensitivity during drought stress (figure 6, old number; table 3) should not be mixed up with annual sensitivity. In addition, annual sensitivity is not a fair comparison of the treatments because fall drought has no chance to compensate yield losses during recovery (as recovers happens only in spring next year).

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

Response: The same response as just above and as response to R1 comment to line 188.

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"?

Response: We incorporated the suggested corrections in the text.

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