

Dear authors,

I am pleased to inform you that your revised manuscript will be considered for publication in Biogeosciences after you have satisfactorily addressed the excellent and very constructive comments provided by the reviewer.

When revising your manuscript please make sure that throughout the manuscript, including the Tables and Figures (see e.g. Fig. 1-2 and Fig. 5, Table 4), all units are written exponentially (e.g. $W m^{-2}$) - see also <https://www.biogeosciences.net/submission.html#figurestable>.

Furthermore I ask you to add y-axis titles to Fig. 2 and, for better visibility, to increase line thickness of significant correlation coefficients in Fig. 4.

Finally, could you please rephrase the last sentence of your abstract to make it a clearcut and straightforward conclusion.

Best regards,
Michael Bahn

Thank you very much for the additional comments on our manuscript. We made sure that all units are written exponentially and added axis titles to Fig. 2. Furthermore, we increased line thickness in Fig. 4 and rephrased the last sentence of our abstract as suggested. It now reads:

“Our results highlight the role of water availability in controlling vegetational shifts in arctic and alpine regions, with soil moisture and winter snow conditions as the main drivers of radial stem growth, negatively affecting the species distribution in a warming climate.”

Additionally, we addressed the questions and comments of the reviewer (see below).

The authors have substantially revised the ms, addressed all suggestions and overall, I think the ms has much improved. The discussion has been completely restructured and reads now much clearer, and includes also a discussion on the limitations. The description of the methods has also been improved and methods have been adapted. However, I still have some questions regarding the methods, especially the correlation analysis and the moving window analysis, but which probably can easily be answered. Thank you very much. We addressed your questions (see below), resulting in minor changes in the manuscript.

Detailed comments:

L213: Table 1

Thank you, we meant to refer to Table 1 here.

L226: Do you mean study sites? (‘measuring sites’)

Yes, we changed “measuring sites” to “study sites”.

L231: I think also for this analysis a mixed effects model would be the appropriate approach.

Thank you very much for the suggestion. We agree that mixed effects modelling is a more appropriate approach here, since variation between sites can be included into the model as a random effect. Accordingly, we now present fixed effects derived from linear mixed effects models in a revised version of Fig. 5.

Comparing the results to the previous correlation analysis, we found that the mixed models mostly confirmed the previous results. In fact, some of the main aspects discussed in the Discussion chapter are more strongly reflected in the results from the mixed models, compared to the correlation analysis. The models revealed for example an influence of the timing of soil thawing (calculated from our environmental data as the first day, at which soil moisture > 0.15) on total annual growth, with earlier soil thawing leading to overall higher growth. We discuss this dependency on freezing and thawing processes in chapter 4.2 Environmental controls of shrub growth in the Discussion section.

In accordance with the new results we made some minor changes in the text, especially in chapter 3.3 Environmental controls of stem diameter variability of the Results section.

L231: Table 3: In Table 3 no micro-environmental drivers, nor the results of the correlation analysis are presented, therefore I think this reference is not placed well.

We meant to refer to Table 1 here. We included the reference here because the micro-environmental drivers which entered into the analysis are summarised in Table 1. We changed the text to clarify: "...from the potential micro-environmental drivers presented in Table 1"

L239: Is it correct that you use a window size of only 3 days or is this a typo? I find this quite small, you have then a sample size of N=3 per window, which is too small to do a correlation analysis? Probably I misunderstand something. I also did not find information how you did account for the different specimen and sites? Did you perform a moving window analysis for each specimen, and average the results in the end, which I think would be the right approach to do, or did you pool everything together?

It is true that the moving window size for this analysis is 3 days. We agree that this might seem very short. We chose this size after carefully testing window widths of up to six months (as mentioned in the manuscript). Overall, window width had surprisingly little effects on the results. To show that in the revised manuscript, we included results for a window width of 30 days (dashed lines) for direct comparison. We changed the text in the Material and Methods section and the figure legend accordingly. We did perform a moving window analysis for each specimen and averaged the results as described.

Table 1: Abbreviations for Soil moisture and Global radiation is missing

Thank you, we included the abbreviations in the table.

Table 2: Dependant = dependent. Why is the pearson's correlation coefficient included in the Table, this does not belong to the mixed model. Either remove or specify in the Table caption.

Does the Model R2 include both the random and the fixed effects? Can you also give the partial R2 of the random and fixed effects (e.g. with MuMin:: r.squaredGLMM(model)

Changed “Dependent”. We agree that the Pearson’s correlation coefficient is not necessary here and removed it from the table.

The Model R² was indeed calculated using the r.squaredGLMM-function and thus represents the variance explained by the entire model (conditional R²). We now added the Partial R² of the fixed effects (marginal R²) as suggested.

Figure 1: I don’t understand what the black bars should indicate. Is this spring and fall? Yes, they indicate meteorological seasons as specified in the figure legend.

L322: Negative relation to temperature in spring is really unexpected. I think it would be worse to discuss this finding.

The negative relation to temperature in spring is possibly linked to the species vulnerability to cold snaps (as described in Weijers et al., 2018). However, our findings were not very strong (see Fig. 2) and are also not confirmed by the mixed effects models, which is why we refrained from discussing this aspect and rather removed it here.

L327: which threshold?

The 5 °C-threshold. We changed the sentence to make this more clear (“...temperatures rising quickly and steadily to 5 °C...”).

L328: How did you determine the upper range of 10°C?

We included both the number of days with temperatures rising above 5 °C, as well as the number of days with temperatures rising above 10 °C into the analysis (growing degree days, GDD5 and GDD10). As we observed the best growing conditions in 2016, the year with the highest number of days with temperatures above 5 °C, but no clear link to GDD10 (for example in 2018, which had an exceptionally high number of days with temperatures above 10 °C), we came to the conclusion presented in the text:

“During 2016, the year with highest radial stem growth, we also measured the highest number of days with soil temperatures above 5 °C, with temperatures rising quickly and steadily to that threshold without reaching continuously higher values during the summer”.

L332: This is a suggestion and belongs to the discussion

This is indeed discussed in the discussion section (chapter 4.2 Environmental controls of shrub growth). We therefore removed it here.

Fig. 4: Did I interpret it correctly that the black bars in Fig.4 and Fig. 1 are opposite (black bar is spring and fall in Fig. 1 and summer in Fig. 4)? I would suggest to make this consistent.

Yes, thank you, we made this consistent.