

“The manuscript entitled “The application of dendrometers to alpine dwarf shrubs – a case study to investigate stem growth responses to environmental conditions” addressed an important issue in relation to intra-annual growth dynamics in shrubs and its environmental drivers. The manuscript has received a plethora of relevant comments from previous reviewers, which have been carefully addressed by the authors. I highly appreciate the topic as it dives into the eco-physiological mechanisms which could further clarify the Arctic greening observations, and enlighten us more on potential environmental drivers which regulate growth. After reviewing the manuscript, and enclosed referee reports, I however still felt like improvements could be made. Particularly in relation to structure, methodological explanations and the presentation of the results.”

Abstract:

“Within the manuscript the authors mention multiple times that high-precision dendrometers have not been used on shrubs before. Although I agree that it is less common, the authors have already published a manuscript in the past where dendrometer measurements have been collected on shrubs (i.e., <https://esajournals.onlinelibrary.wiley.com/doi/10.1002/ecs2.3688>; but see also González-Rodríguez et al., 2017). I would thus argue that one should be more nuanced with these statements. Also, presenting this more as a case-study within the abstract would be better for the manuscript.”

Thank you for the suggestions. The mentioned manuscript was not yet published when we first submitted this manuscript to Biogeosciences, which is why we emphasized the novelty of the method here. We rephrased the Abstract at several points, and it now reflects the state of the current research regarding dendrometer measurements of shrub growth more accurately. We also included our previous study in the Introduction.

Introduction:

“Within the first paragraph of the introduction it might be worth to spend some more time on explaining the observed greening patterns in more detail. Greening can occur because of higher photosynthetic activity of leaves, besides the general higher abundance of shrubs in an area (as noted by the authors). Although woody growth can be stimulated by having additional carbon assimilates, the link between growth and photosynthesis has received some critique which might be worth mentioning as an additional uncertainty (see Faticchi et al. 2019 *New Phytologist*: <https://doi.org/10.1111/nph.15451>). Here one could raise the question whether the growth is indeed favoured by similar environmental drivers as the ones postulated for driving greening.”

“In the second paragraph I would emphasize that most of the referred studies use inter-annual variability of (stem/shoot) growth and that to fully understand the climatic response window we have to obtain intra-annual variability in growth patterns. This could then be combined with the statements made in the third paragraph about shrub-ring series, as right now this seems separated.”

“Moreover, the authors should clarify the link between water use and growth. There is relevant literature on these issues which has not been cited or discussed. One could consider including some statements on mechanistic models showing the link between water relations and cell formation (i.e., De Schepper & Steppe 2010 *Journal of Experimental Botany*: <https://doi.org/10.1093/jxb/erq018>; Peters et al. 2021 *New Phytologist*: <https://doi.org/10.1111/nph.16872>; Cabon et al. 2020: <https://doi.org/10.1111/nph.16456>).”

“Also, the authors should explain in more detail what the physiological relevance is of understanding the swelling dynamics. Do the authors assume that it provides an indirect measure of transpiration and thus assimilation, or is this more related to identifying periods of water stress? As a large emphasis is placed on the physiological mechanisms within the manuscript, I believe that it should be clear to the reader as to why specific measurements are physiologically relevant. At the moment it reads more like a methodological argument (i.e., it is an easily extractable parameter), in addition to the fact that the swelling patterns are not clearly mentioned within the objectives.”

Thank you for your thoughts on the Introduction. We carefully revised the text, including the suggested references (as well as some additional ones, relevant in this context). We additionally emphasized the importance of gathering fine-scale, intra-annual data, as well as physiological processes governing radial stem increment, including cell formation and water relations. Here, we restructured the original text substantially. However, as this study was mainly conducted as a case study we kept the methodical focus. As indicated below, we removed the Species section from the Materials and methods section and included some more detailed information on our focal species in the Introduction instead.

Regarding the main aims of the study, we hope it becomes clear from the revised text (in the Introduction, the Material and Methods, as well as in the Discussion) that we aimed to find common growth patterns, independent from the variability in environmental conditions across the elevational gradient, yet closely linked to the specific characteristics of the positions at exposed ridges, which did not vary between sites.

Materials and methods:

“When reading the MM I find a slight disconnect with the introduction. For example it is not mentioned why having multiple elevational bands for monitoring is important. Especially, as noted by referee 1, there is only 1 individual per site which is critical. This requires clarification within the introduction, as apparently the variability in environmental conditions was more important when setting up the sites than the replication of shrubs per site. Also, within the materials and methods there is the species section where the authors explain the selection of the species with relevant literature. In my view this belongs in a shortened version into the introduction, as it explains the reader the merits of the study (i.e., why select this species).”

“In accordance with referee 3, I am still slightly confused about the different methods used for extracting growth from water relations. From the response of the authors it seems they extracted daily growth rates using the method proposed by Deslauriers and excluded solely rehydration patterns before growth initiation (based on the zero-growth concept; Zweifel et al. 2016). This approach however fully ignores the fact that stem shrinkage can also occur during the growing season, where the approach by Deslauriers would again overestimate the annual growth rates, compared to the method proposed by Zweifel. As the method by Zweifel allows for the extraction of hourly/daily growth patterns as well (see R package presented in Knüsel et al. 2021 Forests:

<https://doi.org/10.3390/f12060765>), I am really confused as to why the author combined these two approaches, without concretely testing the difference. Has this been done? Also, in the response to the comments from referee 3 they noted that they combined the zero-growth and Gompertz model, while in the methods they clearly state they use the dendrometeR package to extract peak growth rates. Please provide a more structured and clear line as to which growth and water related parameters are extracted and how. Also, again little room is provided to explain the exact “swelling” (or water related) parameter which was mentioned to be relevant within the introduction.”

“Finally, for the methods I found it distracting to already have results presented in such great detail (as noted by referee 1 and 3). One can decide to either make a dedicated paragraph within the results section to explain the environmental variability, or one can move these figure to the supporting information.”

Regarding our methods for separating growth from water relations, there still seem to be some major misunderstandings, which is why we revised the text substantially. We used the dendrometeR package solely to derive daily values from our hourly stem diameter measurements, as daily fluctuations were not our main interest and aggregating the hourly data facilitated calculation processes within the R statistical software. While there are multiple other tools available in R to do this, the package provided a quick alternative which fitted the structure of our data. For all further analysis we based our understanding of growth on the approach proposed by Zweifel (2016), assuming no growth during periods of stem shrinkage. However, this does not mean that shrinking processes were not included in our analysis, we simply chose to separate them from growth using the described approach. Here, we revised the chapter entirely to avoid further misunderstandings regarding our methods. We made sure

that the introduction of each of our growth parameters is followed by a detailed explanation, including definition, approach and physiological background.

Additionally, we moved large parts of the Material and Methods section to the Introduction, as well as to a new paragraph within the Results chapter, as suggested.

Results:

“Within Figure 3 the stem water deficit is clearly presented, yet no analyses has been performed on these patterns outside of the winter months (which has not been clearly explained in the MM). This seems strange as within the introduction the authors state: “Additionally, the time series derived from dendrometer measurements offer information not only on radial stem growth, but also on stem water relationships with higher quality and resolution than previously attainable”. If there is no intention to look at water relations or swelling/shrinkage outside of the winter months, I would specifically mention this and explain why, instead of highlighting it as generally relevant physiological information. This lower relevance of water relationships extracted from the dendrometers is again highlighted by the fact that within Table 4 the authors mention in the header “Growth parameters” while it also includes some shrinkage parameters (this is also the case for Figure 6).”

“The analyses and the representation of Figure 5 could be improved. On the x-axis a fixed shift is presented, where the selection of the intervals has not been explained in the methods. Also, it is not clear what is meant with stem variability. Does this refer to growth or another parameter extracted from the data? Moreover, there is the possibility to perform continuous moving-window correlation analyses (as presented in Castagneri et al. 2017 Annals of Botany: <https://doi.org/10.1093/aob/mcw274>) which could be more informative then fixed periods.”

“Finally, within the results it is often unclear whether correlations have been performed with daily or annually aggregated values. This should be clearly described in all figure legends and within the descriptive text, as these analyses operate with highly different sampling sizes.”

We revised the Results chapter, including some parts previously included in the Material and Methods section. While it is true that we focused more on growth than on the described shrinking processes, swelling and shrinking were included in multiple of our analysis. For example, shrinking parameters were included in all of the regression analyses. Here, the term “growth parameters” might be misleading, which is why we revised the text, differentiating between growth parameters and parameters of stem change. From the calculated stem water deficit (previously Fig. 3, new Fig. 1) it becomes clear that the main phase of stem shrinking occurs during the winter months, which is why we focused on this phenomenon later on (see Discussion).

Additionally, we replaced Fig. 5, including a moving window analysis as suggested. Here, we experimented with including varying time periods for the environmental data, similar to the original analysis. However, the results revealed no significant new insights gained from including these time spans, which is why we did not include them in the final analysis. Instead, we focused on direct influences of the on-site environmental measurements on daily rates of stem change, thus including stem shrinking and swelling processes.

We also made sure, to indicate more clearly, if daily or annually aggregated values were used in the analysis.

Discussion:

“Within the introduction a nice structure is presented, namely: “1) explain major growth patterns and their variation between years and specimens, 2) identify the most important on-site environmental drivers controlling these patterns, and 3) gain insights into potential response to environmental change”. I was expecting a similar structure to become apparent in the discussion. However, within the discussion a more parameter centred structure is utilized. I am wondering whether it then would be clearer for the reader to add an initial section where these key points are shortly addressed before diving into each parameters. Then one should also introduce the discussion structure.

The main objective is clearly defined: “The main objective of our work is thus to gain detailed understanding of the growth patterns of one common arctic-alpine dwarf shrub (*Empetrum nigrum* ssp. *hermaphroditum*) and their relation to the micro-environment”. However, within the discussion ample attention is provided to the distinct differences between macro- and micro-environmental conditions which boils down to the conclusion that topography is crucial in determining growth responses. Yet, due to the sampling design (solely 1 sample per site) and presented analyses, I wonder whether this is the strongest result one can present from this study. I find the discussion on temporal dynamics of the growth responses to environmental conditions more relevant.”

“In relation to the discussion on the lower relevance of atmospheric air temperature on growth, I do not find the response of the authors satisfying. First the authors state: “A direct comparison of annual growth derived from our dendrometer measurements and such ring width measurements at the studied sites revealed high synchrony. Here, the ring width data was linked to summer temperature as well, suggesting that the assumed temperature-growth relation holds partly true at our sampled sites”. So if this is a clear observation, then why are the author so convinced that there is no direct connection between total growth and near-surface temperature. Multiple mechanistic studies show the relevance of temperature on enzymatic kinetics within the cambium (i.e., see temperature module in the Cabon et al. 2020 model). Moreover, there is no discussion on the fact that the difference might also be caused by solely including four years of dendrometer data, or due to the fact the dendrometers incorporate both the production of xylem and phloem cells, while tree-ring studies only consider the xylem. Without such careful considerations I tend to agree with the previous concerns raised by referee 1.”

“More general I miss an overarching discussion on the limitations of the study. I think this is particularly relevant as I agree with referee 3 that this should be considered as a case study highlighting the potential of using dendrometer data on shrubs. Referee 3 for instance makes a strong point with the fact that VPD is a critical environmental factor which should be studied in the future (see Novick et al., 2016; Grossiord et al., 2018; Peters et al., 2021; Zweifel et al., 2021). Also, referee 3 makes a good point on the fact that the Gompertz fitting has its limitation, while other methods to extract the start and end of the growing season do exist (see Knüsel et al. 2021 *Forests*: <https://doi.org/10.3390/f12060765>). Some of the concerns raised by 1 referee are also valid. For example, 1 dendrometer per site/elevation is a limitation which prevented the analyses on the impact of elevation on growth parameters. Moreover, the fact that only one stem is measured per shrub does generate the question on how large the within plant variability is and how this would impact the results. All such careful considerations should be mentioned in a dedicated section, where clear recommendations should be presented to guide future research efforts.”

“Finally, I would refrain from using references within the conclusion and just highlight the most important findings and considerations.”

While revising the Discussion with the remarks on structure in mind, we came to the conclusion that the previous structure with the emphasis on parameters might not be the best choice, as several aspects were coming up multiple times in the text and some parts were therefore not as clear as they could be. We therefore revised the structure of the Discussion entirely, keeping the main aims introduced in the Introduction in mind. With this revised structure, an additional Conclusion chapter seemed unnecessary. We therefore removed it and incorporated the aspects from the previous Conclusion in the Discussion section.

Also, we put more emphasis on temporal dynamics and the complex temperature- and soil moisture-relations found in our data, showing that while we found no evidence for highly influential temperature thresholds, temperatures still played a role in determining growth patterns, but this role is more complex than previously assumed.

Regarding the methodical concerns, we included some of these considerations in the Discussion and in the Material and Methods chapter. Also, we made sure to phrase the implications and possibilities for future research more clearly, and included some additional statements, pointing out the limitations of the approach.

