

# **Drought responses of a Norway spruce forest on drained peat soil: combining sap-flow sensors, eddy-covariance, soil and UAV data**

**EGUSPHERE-2026-568**

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## **Response to Review No. 3**

We thank the Reviewer for the time and effort invested in critical assessment of our work. The review makes a number of points that we find valid and useful and will gladly respond to them.

This study has used sapflow data, eddy covariance and UAV measurements to assess the response of a Norway spruce forest to drought over two years (2020 and 2021). In the meantime, the forest was affected by thinning. Throughout the paper it is not clear what exactly the research questions are (effect of drought on transpiration or carbon fluxes or both? Effect of thinning on carbon fluxes? How thinning changes response to drought? What is the question?). - Thanks for this important feedback on the ambiguity of the study aims. We believe the complexity follows from our broad goal to conduct a holistic analysis of potential drought responses on commercially managed drained peatland forest. To achieve that based on intensive multi-scale un-controlled field measurements, we adopted a combination of approaches to analyze the impacts of atmospheric (high VPD) and soil drought (limited water availability) on tree and ecosystem-scale water and carbon flux dynamics. Further, we tested different in-situ approaches to detect tree-scale drought proxies. This admittedly makes the paper heavier read, but was necessary in order to address each facet of the problem. While we agree further refinement of text is beneficial for overall clarity, the study aims are in our opinion already clearly stated in the lines 98-110:

“The objective of this study was to compare the drought responses of two adjacent stands, a recently selection-harvested and an untreated control stand, on drained peatland soil in southern Finland. The study was conducted during the non-drought year 2020, and during a long and intense drought period in 2021 likely representative of the future Boreal climate (Balting et al. 2021), which was also the year the site was selection harvested. We hypothesized that the trees in the selection harvested stand were more stressed than the trees in the unharvest control to the drought in 2021.

Overall challenge to detect (likely) non-linear responses to progressing drought in boreal forests serves as the primary premise for this work. Therefore, a diverse set of continuous in-situ monitoring data available at the Ränskälänkorpi peatland forest harvest experiment was used to examine the possible drought responses of trees and stands. The standard ecosystem carbon, water and energy balance measurements (eddy-covariance CO<sub>2</sub> and H<sub>2</sub>O fluxes, surface energy balance, sap-flow sensors) are supplemented by campaign-wise thermal and multispectral UAV data. Based on these data, we conducted the analysis of how the drought progresses and trees respond to it, and compare and discuss the merits of different methods for observing drought stress of individual trees and tree stands.”

In fact, the Reviewer refers to the same lines in the comment on Line 107 below.

Because of the poor structure of the paper, it is not clear what new insights are gained here which we did not already have, based on considerably longer timeseries and a greater number of sites. - The reviewer does not give concrete hints as to what the issues with the structure are, so this is not very helpful. We found it most efficient to first represent each topic / use of dataset in Materials and Methods, presenting the combinations of different approaches in Results, and finally drawing a synthesis in Discussion. We will make our best to further streamline the text in the revised version of the manuscript. Our goal will be to improve the clarity overall, but specifically on the exact points identified by Reviewers. We are confident this will relax the overall concern of Reviewer 3. Please note that the two other reviewers did not find the paper particularly poorly organized or hard to follow.

The paper is written very poorly (results are provided often with a qualitative language) and basic statistical analysis is missing. - Extra care will be taken to ensure the correctness of language in the revised version.

Overall, we are against conducting statistical analysis just for the sake of it and using p-values as the god’s word. The methods and representation has been selected according to their necessity and suitability and we consider them to be sufficient for the aims of the present work. The drought responses, where present, can be readily seen in the graphical form in the figures. , Also here, the lacking the specificity of the Reviewer’s comments makes it difficult to provide a more detailed answer. We also note that other two reviewers did not find significant shortcomings in our methods, analysis or representation, except in terms of lack of clarity of some specific details (please see our responses to other reviewers).

Thus, what the authors present as “response” to drought might not hold at all. Also, I have reservations with how the authors defined drought (see details below). - The main takeaway of this work is perhaps that the responses of a conifer forest on drained peat soil are i) complex and ii) vary between trees and tree and ecosystem scale. Clearly, they also depend on the mode of recent forest management. Each type of data involved provides a unique insight into specific processes and scales involved. Thus, it is impossible to single out any single response or process

of superior importance; in fact, there are many ways to observe and understand what the “drought response” is. However, the analysis framework is straightforward enough in our opinion – it consists of identifying soil and air drought based on in-situ soil and micrometeorological sensors, and tracking the potential tree responses in the flux and sap flow data, and looking for corresponding signals in the novel UAV data. For drought onset, we made reasonable first guesses regarding the point at which the drought responses could start to be observed - and these were indeed confirmed by the results.

Overall, this was a frustrating experience reviewing this paper. I do not believe that the study is mature enough for publication in Biogeosciences. I have listed my reasoning below.

Title: Rather than listing the methods, the title should indicate what “response” of the forest is the central point of this paper. - We regret that the reviewer has felt the paper that frustrating. Regards to title, it could be useful to modify the title as “Complex drought responses of a Norway spruce forest...” As suggested above, the observed responses are disparate enough to be collectively crammed into the title.

The Abstract is written very poorly. It is not clear what exactly the research questions are (not clear response to drought is assessed based on what function?) and it is a summary of methods and some results without an appropriate link made across the methods. - We will improve the abstract as suggested also by other reviewers, taking care of the word count limit. Especially the research question will be clarified as requested.

Only the results from each of the observation methods are reported. Judging by the Abstract alone, there is no novel finding or mechanistic understanding provided by the study. It is not clear to me what this paper adds to the existing body of literature.

We acknowledge the comment and will make our best to improve the Abstract in the next version. In short, the novelties are: - 1) The use of such a diverse multi-method and -scale dataset is a novelty in itself; 2) drought responses of conifer forests on drained peat soils are known poorly in general, yet these ecosystems are central for Northern European forest carbon balance and ecosystem services, particularly in Finland and European Russia where forestry drainage contains millions of hectares of boreal peatlands, 3) A site with two harvest treatments (thinned using selection harvesting and control) is seldom featured in literature.

Unfortunately, still by the end of the Introduction the objectives are not clear (is it management effect? Why is not the relevant literature assessed for this topic?) The Introduction is a collection of points without a clear link to form the overall objective of the study and the motivation of the study is lacking. - Please see the last lines of the Introduction as quoted above. We will try our best to account for this comment and improve the introduction.

Not enough details on EC flux calculation are given: e.g., what frequency are the high-resolution data, raw data filtering approaches and checks, coordinate rotation, time-lag detection etc. - We consider this beyond the scope of the present paper focusing on ecosystem behavior, not flux data processing. Common and established methods of the EC flux community are used throughout in measurements and data processing. We assure the Reviewer that the eddy-covariance measurements were carried out by a team of highly skilled specialists and the measurements were carried out to the highest current standards. Aubinet et al. (2012), already cited, summarize the current EC measurement standards and best practices, which we followed. Further details can be found in Laurila et al. (2021), which is already cited in the manuscript, but a reference will also be added in the paragraph on EC methods.

I understand that the study employs multiple different observation methods but rather than focusing on the method would be good to focus on the ecosystem properties these methods were used to assess. E.g., in Line 22-25: combining monitoring for what factor? Dataset of what? - The dataset in question is explained the Abstract, quoting Lines 24-25: “A dataset combining eddy-covariance (EC) fluxes, sap-flow sensors, UAV mapping, soil and weather data was collected”. We understand that the Reviewer would like to see the mention of the studied quantities, such as the turbulent CO<sub>2</sub> flux density and stomatal resistance. This will be clarified in the Abstract, as per the word count limits.

Is the research question about soil (highlighted in the abstract) effect on drought response? Or on management (thinning) effect on drought response? What is the question here? Not clear. The Introduction starts with the topic of management so maybe this is the key question? We don't know. - We refer here to our earlier response on study goals.

There is also a major issue with how “drought” in the soil is extracted. The authors have used absolute soil moisture content as a threshold for drought, and not even based on the local observations but based on another study! Soil water limitation depends on water potential, NOT water content and this link is highly dependent on soil texture. The studies that are used to determine the soil water content threshold for drought are on mineral soil and not relevant for this forest.

Using previous studies was necessary in order to come up with the first approximation of what the critical soil moisture level may be. Considering the porosity and shape of water retention curve in typical drained forest peatlands, it may be that our estimate of approximate threshold soil moisture is conservative. This will be checked and use of threshold moisture value is better motivated in the revised version.

Results are presented with qualitative language: ample, remarkable. They don't provide sufficient comparison and a statistical test to show if the responses are at all significantly different is completely missing from the paper. - We do not observe a major problem with our use of the words “ample” (1 instance) or “remarkable” (1 instance). In both cases, the meaning of

these words tallies with what is shown in the figures. Otherwise, we are not ready to impoverish the language of the manuscript but will attempt to be more quantitative (e.g. providing magnitudes of relative impacts) whenever possible. We agree that this will strengthen the text.

It is not quite clear what the Reviewer means by “show if the responses are at all significantly different”. If they mean the difference between the treatments, these are very apparent in the graphical representation of the CO<sub>2</sub> fluxes (e.g. Fig. 5), sap-flow (Fig. 8), and temperature-based stomatal conductance (Figs. 10-11). If the reviewer is pointing to whether e.g. specific fluxes or resource-use efficiencies within treatment are statistically different is another question. Given that we analyze time series from field experiment under progressing drought and varying meteorological conditions, addressing statistical significance would be very contextual to selected sub-setting of data.

Bare minimum standard writing and analysis for a scientific paper is lacking. - We kindly disagree with that statement and consider it unfounded, as reviewer does not provide specific backing to this claim.

### **Specific comments:**

Abstract should already mention the timeframe of the study. – The timeframe will be specified

Line 26. “It’s temporal and spatial features are identified „, temporal features in what? What is meant by spatial features of the forest stand? – Temporal and spatial features of spruce reaction to drought are meant.

Line 32: Are you talking about an increase in temperature and decline in NDVI due to drought, which was higher in CCF plots? Not clear how this comparison reflects the effect of drought on the two blocks. – Correct, this is what is meant here. Since there is no space to delve into this any deeper in the Abstract, the reader has to proceed to the main text.

Line 37-39: the two statements are not linked. - In order to establish a link, the second sentence is rephrased as: “The Boreal managed forests might benefit from the enhancement of tree growth in the region due to higher air temperature and CO<sub>2</sub> concentration which is forecast for the coming decades (Danneyrolles et al. 2023, Wang et al. 2023).”

Line 41: What exactly is the example reference supporting? – The sentence will be rephrased as: “only if water availability is not a limiting factor in new conditions (e.g. d’Orangeville et al. 2018).”

Line 46: given that low resilience is confirmed for Norway spruce, what is your study adding? - Our study was certainly not designed to confirm the basic fact of Norway spruce susceptibility to

drought, but to explore its extent and timing in an environment that has so far been underrepresented, and management practices (stand thinning) that are suggested to reduce drought stress, using the tools that are seldom combined. As explained later on in the Introduction.

Line 51: the topic of drought identification is not linked to the previous statement. – I do not see how the lines 45-56 could be lacking this link, I think it is sufficiently clear; Norway spruce drought resilience being a major source of uncertainty for boreal forestry is the point.

Line 55: “atmospheric drought”, not air drought - While “air drought” is a valid term which is very common in other literature, it perhaps makes sense to harmonize the text and only use the term “atmospheric drought”, as suggested.

Line 61 and 62 could have been merged – Will be done

Line 77: provides a buffer rather than protection – In a sense, the microclimatic buffering can be understood as protection, as the small trees are literally sheltered from direct sunlight by the dominant trees, and grow in lower VPD conditions than the dominant trees. This will be revised.

Line 76 so the non-dominant trees and understory are growing in the dark?! – “More dark microclimate” is what is meant here. Will be rephrased as “lower PAR environment”.

Line 85: and? What does this “less competition” mean for their response to drought? What is the link? – This will be clarified. Lower inter-tree competition is beneficial at the times of lower availability of soil water because, on the one hand, the trees do not collectively drain the soil water reserves, and, on the other hand, the dominant trees do not leave the rest without access to water.

Line 87: What is even the point of this sentence here that UAV and RS is helpful or research?? – We find it logical and according to basic principles of a research article to first state the overall success and facility of UAV remote sensing tools and then highlight the current limitations and knowledge gaps. The latter might indeed be insufficiently represented in this specific portion of text, and will be given more space.

Line 91: this is not true. There are numerous studies using UAV to detect drought effect on a range of structural and physiological functional traits in forests. – Numerous is maybe an overstatement, but there are some. What the sentence in question is lacking, though, is and addition in the end – “especially in the Boreal region”.

Also, is this the place to justify a methodology? I am struggling to find the research questions. Yes, there are some background information given here and there (that the resilience of Norway spruce is tested before, effect of tree response on stand response) but what is the exact question in this study that addressed all these functions in a manner that has not been tested before? - To

our best knowledge, UAV-based remote sensing combining both thermal and RGB imaging has not been conducted in a Boreal forest drought studies, which justifies its application. The research questions follow just below and the UAV data are mentioned. We consider adding a specific mention that leaf colour (~NDVI) and temperature are the primary parameters (physiological and functional proxies) provided by the UAV platform.

Line 98: if the objective was to compare for the effect of management, this topic should be the focus and properly addressed and related literature reviewed for it, and provide the basis for the hypothesis that is put forward.

- While the harvest-control differences are a circumstance that adds an important aspect of tree density effects on drought response, this does not warrant an in-depth review of all aspects this problem. A brief overview will be added in the Introduction.

Line 104: what causes this difficulty? Reading this sentence, I would conclude that you have developed methods that specifically address the challenge of detecting drought response? (a remote sensing-focused analysis). How does using multiple methods that are each very demanding and not available easily, provide a solution to the challenges of studying boreal forests? So again: What is the research question exactly?

- On the methodological side, the research question was: What added value does the combination of several tools for ecosystem monitoring provide for drought response studies? This will be clarified in Introduction and Discussion.. Another research question, related to tree physiology, was about the potential difference in transpiration between the control and harvest blocks, as stated in the Lines 102-104.

- This should be rephrased as “difficulty to harmonize the drought indications across the multitude of existing methodologies”, as this is one important aspect. – Will be done

Line 107: OK so finally there is a mention what is studied here: carbon, water and energy? This should have been made clear in the Abstract. Sapflow is clear but UAV or EC don't indicate what entity has been measured and what is the focus of this paper. – I will try to fit this into the Abstract

Line 23: this is about density. What about the contribution to stand basal area by each species? If inventory data was collected DBH should be available for the trees. – This information is actually available in Martinez-Garcia (2026), “Short-term effects of harvesting alternatives on soil nitrous oxide fluxes in a boreal drained peatland forest”. This will be cited in the manuscript.

Figure 1: footprint area around the tower should have been shown to link the sapflow measurements with the fluxes – We acknowledge this and consider adding a cumulative EC flux contribution isoline (e.g. 60%)for clarity.

Line 150: Standard flux terminology is ignored. Calculating fluxes is processing, gapfilling and partitioning is referred to as post-processing. - We will consider the comment and ensure text follows established terminology as closely as possible.

Line 193: nearby means how many m away? Ground level means on the ground!? What variables were measured on the ground? – Immediately near the EC tower, not more than 10 m away.

Section 2.2.2 The location of the GW measurements and soil profile should also have been shown on the map. – Will be added

Line 220-226: why was soil drought defined based on thresholds of SWC from soil that is not even organic? This undermines the definition of soil drought selected for this analysis. – We do not see a reason why this cannot be done. There is an extensive body of literature discussing specifically the soil water content in peat.

Line 274: stem growth sensors – This subtitle lists the measured quantities and not sensors, therefore the replacement of “stem diameter change” with “stem growth sensors” would be inappropriate.

Line 284-282 what is the reference that validates this approach? – The task of sapflow upscaling is not novel per se, reviewed in Čermák et al. (2004) “Sap flow measurements with some thermodynamic methods, flow integration within trees and scaling up from sample trees to entire forest stands”. The previous studies have explored sap flow relationship with the tree structural parameters, including tree height (e.g. Zhang et al. (2018), “*The tree height-related spatial variances of tree sap flux in subtropical forests*”). In the current study, tree height was chosen as the explanatory variable for the following reasons. First, the number of sap flow sensors per treatment (7 and 8 in control and harvest, respectively) allows having only one explanatory variable. Second, it is known that tree-specific sap flow is strongly dependent on tree size/, in the general sense. Third, out of all tree structural parameters, tree height is most readily obtained from the UAV photogrammetric digital elevation model. Fourth, the tree height shows a strong relationship with the sap flow in the 15 trees that have sap flow sensors installed (appendix 4). Hence our use of tree height as the sap flow driver in the upscaling exercise.

Same as for equation 10. This is a rather standard expression for tree-scale stomatal conductance derived from sap flow, e.g. Oren et al. (1999) “Survey and synthesis of intra- and interspecific variation in stomatal sensitivity to vapour pressure deficit”, Köstner et al. (1992) “Transpiration and canopy conductance in coniferous forests”. The sap flow utilized therein is normalized by sapwood area of the measured tree.

Figure 2: a map without coordinates is just a shape figure. – These panels are intended to show the spatial distribution of SPEI over the territory of Finland. In that sense, it is a shape figure

indeed, and it serves its purpose. Moreover, the size of figure would not allow the addition of coordinate labels in a fashion that is readable and non-obstructive for the main colour layer.

Line 387: What supports this statement? When can the reader get this information? – if the Reviewer means the WTD drop, it is presented in the Figure 3 just below. A reference to the relevant figure and panel will be added.

Line 417: How much is “ample”? – The exact amount will be specified. This means that it was the period of strongest precipitation of the year.

Figure 4: ET (or LE) should have been plotted too – Will be added

Line 455: seasonality in what? Light? Phenology? What is this statement describing and how is it connected to the previous statement – Will be replaced with “phenology”

Figure 5 The difference between the lines (mean for each category plus the uncertainty around them) should have been tested to see if the differences are significantly large enough – This will be done

Line 468: but we don’t know if it was significantly lower? - Will be specified, as suggested above

Line 476: what is meant by mild weather? – Means “non-drought”, this will be clarified

Figure 6: why is the daytime 30-min fluxes called flux density? – In micrometeorology, the quantity which is commonly referred to as “30-min average flux” is, in fact, termed “turbulent flux density”. We will consider reformulating the sentence.

Figure 6 lower panel: this is describing the drier conditions and should have been mentioned earlier – These panels can be moved to Figure 2, where they would be a better fit indeed.

Figure 7 which of these differences are significant and which not? – The significance of differences between the red and blue bin-averages (drought/non-drought) is readily shown by the error bars plotted on the bin-average markers. As one can see, these differences are typically insignificant at VPD < 1 kPa, and significant beyond 1 kPa, although the exact threshold varies between the trees.

Line 506: if it is usual, then references should have been given to support this. – Will be added

Figure 8 seems like some panels (b, c) are cropped at top (y-axis limit should be increased) – There is only one tree that shows a much higher sap flow than the rest, and thus the present y-limits were selected to best illustrate typical behavior. The figure label will be improved to clarify this.

How was resilience defined (the abstract mentions higher resilience of thinned stands to high VPD. Also how do the authors know that this range of VPD is “too high”? – The definition of resilience will be added and related text reformulated (based on Holling 1973, “Resilience and Stability of Ecological Systems”) We are not completely sure what the Reviewer refers to in the second half of this comment, but generally VPD exceeding 1 kPa is known to initiate plant physiological responses such as stomatal closure, which also emerges from the present study.

The manuscript should have been checked for language (many grammar mistake that I won't list here) – An extra language check will be made before the revised manuscript is submitted.