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

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Response to Review No. 2

This study presents a commendable investigation of forest drought responses, integrating eddy covariance, sap flow, UAV remote sensing, and soil measurements across control and harvested blocks. The breadth of measurements spanning different scales and levels of plant functioning is a major strength of this work. The author presents results clearly for the most part, discusses limitations in considerable detail, and applies sound analytical techniques throughout. - We sincerely thank the Reviewer for all the motivating and constructive comments! Our responses will be given in blue text.

However, two issues require attention before the manuscript is suitable for publication:

1. Certain statements and conclusions lack specificity or contain internal inconsistencies that should be resolved. Key findings, particularly the discrepancy between sap flow and canopy-level indicators, should be more prominently communicated, including in the abstract. – We will address this need and sharpen these points in the manuscript.

2. Consistency in color schemes and legend readability should be improved across several figures. – Will be done according to the recommendations of the Reviewer.

I recommend this manuscript for publication, subject to the revisions detailed in my specific comments. Please note that the detailed comments below do not follow the order of major and minor revisions but instead follow the sequential structure of the paper for ease of reference.

Abstract:
The major conclusion of the paper that the sap-flow is more indicative of the drought at the stand level is somehow missing in the abstract section of the paper.
Also, the abstract could be streamlined to clearly convey the research's output.
For instance:

"Sap flow data showed that the CCF-harvested block shows greater tree resilience to high vapor pressure deficit (VPD) than the control block, likely due to the higher soil water availability during the rainless and hot period." - Thank you for this important remark. We will edit the relevant parts of the manuscript in order to highlight this idea.

Sap flow measurements revealed contrasting drought responses between the two blocks. While normalized sap flow in the control block was insensitive to soil drought conditions, the harvested block showed elevated sap flow during drought relative to non-drought periods (Figure 7). This divergence intensified during the hot, rainless June–July 2021 period, alongside rising VPD (Figure 8), though whether it reflects genuine drought resilience or simply reduced competition for water among fewer trees in the harvested block remains unresolved. – We agree that these two concepts highlighted by the Reviewer form part of the complexity of the processes under scrutiny. The evidence presented in this work could be interpreted as the fusion of both: fewer trees means more water availability, which means higher ecosystem- or stand-level resilience. Whether it involves any physiological mechanisms enhancing the drought resilience on an individual tree scale could not be verified with the available monitoring data.

Especially when UAV retrievals show a different story: stomatal conductance derived from canopy temperature was lower in the harvested block than in the control (Figure 10). While the author thoroughly discusses this and other measurement limitations in the discussion section, which I appreciate, this particular statement, in my view, remains unfounded in the abstract section. – We will try to add this point to Abstract, as much as the word limit will allow it. The Reviewer is absolutely correct that this conflict between the ecosystem monitoring products is one of the interesting observations. Unfortunately, we the potential reasons for this discrepancy remain speculative and warrant future studies.

Introduction:

There is an internal inconsistency between lines 85 and 101. At line 85, the author states that thinned stands are generally expected to have less inter-tree competition for soil water, implying greater water availability per tree and, therefore, lower drought stress in the harvested block. However, at line 101, the author hypothesizes that trees in the harvested stand experienced greater drought stress than those in the control. In my view, these two statements are contradictory. Though this is just a hypothesis, it leaves the reader confused following the introduction. Do you also think so? – Indeed, there was a contradiction between these portions of text, thank you for pointing it out. on line 83 we say "It is generally expected that thinned stands have less inter-tree competition for soil water, particularly during a drought in both Norway spruce and Scots pine (Laurent et al. 2003; Gebhardt et al. 2014, Friedrichs et al. 2009, Martín-Benito et al. 2010, del Río et al. 2017)". However, in the hypothesis on line 101 "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. ”We will revise the text, if necessary, so that no apparent unintended conflicts remain. Our hypothesis would naturally go along the lines of what had been observed previously.

Methods:

Since vegetation height, roughness length, and displacement height are required inputs for deriving turbulent fluxes from eddy covariance measurements, and given that the stands exhibit considerable heterogeneity in vegetation height, it is unclear which height value was used in these calculations. – The measurement height of 29 m and displacement height estimate of 10 m were used in the calculations (the canopy height is about 10 m, and in the Harvest block it is rather sparse). These parameters did not vary with wind direction. The displacement height could be somewhat higher in the Control block, but the difference with Harvest block is difficult to estimate. The differences in turbulent field between the two blocks were described by the roughness length, whose measured 30-min (i.e. wind direction-dependent) values were fed to the footprint algorithm.

For consistency, does the SpaFhy model use the Penman-Monteith equation to calculate ET for SPEI derivation, given that CWSI is also computed within the Penman-Monteith framework? While I do not expect this to change the interpretation, it is worth clarifying that the point-based model often uses a gradient-based resistance scheme to compute ET. – SpaFHy model uses two-layer approach to predict daily ecosystem ET. Penman-Monteith equation is used separately for canopy transpiration and evaporation of intercepted rainfall from the canopy. The respective aerodynamic and surface conductance depends on meteorological conditions, as well as LAI, plant water use traits and seasonal phenology. Evaporation from forest floor follows radiation- and temperature-driven Priestley-Taylor equation, where available energy depends on LAI of overstory canopy and ET is reduced linearly in case the organic top soil layer, resembling moss and humus layer, moisture content is below a threshold value. Forest floor ET is set to zero when soil is snow-covered. For details, see Supplementary material in Launiainen et al., 2019.

The author selects 1 kPa VPD and 0.2 m^3/m^3 soil moisture as thresholds for atmospheric and soil drought, respectively, based on earlier research. While not mandatory, these boundaries could be further corroborated from a plant hydraulics perspective. For instance, by relating them to known water potential thresholds associated with stomatal closure or hydraulic failure in the studied species. This would provide a more physiologically grounded justification, if the author chose to do so. – We will try to present further corroboration of these first-guess limits. These thresholds are indeed intended to mark the initial stages of reaction, which most importantly consists of stomatal closure.

In Equation 6, substituting Equation 7 appears to yield a zero in the numerator, which would

render the CWSI expression trivial. The correct form should likely be $(y^* - y) / (\tau + y^*)$, where the numerator captures the departure from the non-water-stressed baseline. Please check.
It is unclear whether gs was calculated at 15-minute intervals and subsequently averaged to a daily value, or whether daily mean VPD was used directly to compute a single daily gs estimate. – Apologies for the typo; the correct expression is $\gamma^*=\gamma(1+r_{sP} / r_a)$, where r_{sP} is the canopy resistance at potential evapotranspiration. However, since the r_{sP} term is typically not known precisely, it is assumed that $r_{sP} = 0$, which leads to $\gamma^*=\gamma$. Therefore, the equation becomes:

$$\mathrm{CWSI} = \frac{\gamma\left(1 + r_{s,EC}\ r_a^{-1}\right) - \gamma}{\zeta + \gamma\left(1 + r_{s,EC}\ r_a^{-1}\right)}$$

This is the actual form of the equation that was used in the present version of the manuscript.

Results

1. In Figure 3, the first row and others present variables aggregated at the ecosystem level, while the second row shows per-block comparisons. Rearranging the panels so that all ecosystem-level variables appear at the top and per-block comparisons at the bottom would improve readability. – Will be done

2. At line 451, the authors state that correlations with VPD were more pronounced in 2020 than in 2021, attributing this to the higher tree density before harvest and the stronger 2021 drought. While this interpretation is plausible, it is not directly evident from Figure 4 as currently presented. – It is better to reformulate this as “High VPD periods caused clearer reductions in transpiration-related drought proxies in 2020 than 2021”, as this is what we intended to suggest.

3. Lines 476–479 are unclear. In particular, the statement that higher sunlight during drought compensates for lower Pmax, leading to similar integrated GPP, is difficult to follow. Since Pmax is derived from the GPP–PAR relationship using a constant k over the growing season, any change in Pmax is inherently a consequence of changes in GPP and/or PAR themselves. The authors should clarify the causal logic here, as the current phrasing risks circular reasoning. – This is about the integral of the GPP vs. PAR curve. A “lower” daily curve, i.e. one with a smaller Pmax, might have the same integral as a higher curve, if the ecosystem experienced higher PAR values on that day. Adding a schematic in the Appendices might aid the understanding of this idea.

4. Lines 198–199, section 2.2.2, state that humidity, air temperature, and PAR were measured at nearby meteorological stations. It is therefore unclear how these variables are plotted separately per block, unless separate sensors were deployed in each block. The authors should clarify this in Figures 6 (e,f,i). – These measurements were located just a few meters distance from the EC tower, so, on the border between the Harvest and Control blocks.

5. Figure 6: Legend are too small to understand in the top row. – Will be edited

6. In my view, while discussing Figure 6, we could only agree that at very high VPD (> 2kPa), NEE appears to differ from low VPD conditions in both the control and harvested blocks. And NEE is the measured signal. However, the ecosystem respiration (Re) estimate used here is solely temperature-dependent, omitting potentially important drivers such as soil moisture. This may introduce unaccounted-for uncertainty into Re and, consequently, into the partitioned GPP. Furthermore, the partitioning approach presented here differs from the standard Reichstein (2003) method widely used in the flux community. Given these limitations, the authors should exercise caution in drawing strong conclusions from GPP and Pmax, as these are derived quantities sensitive to the choice of partitioning algorithm rather than directly measured fluxes. I thus encourage the author to moderate the conclusions based on GPP and Pmax throughout the paper, while still presenting them. For instance:
In line 677,
While the drought thus suppressed photosynthetic efficiency in 2021, as was reliably shown by CO2 exchange in the Control, the mounting soil dryness and prolonged VPD stress did not seem to have had a similar effect in June-July 2021. Both Pmax and GPP increased throughout the 2021 drought and during the hotter period of 2020 (Figs. 4-6). - Absolutely, NEE is the directly measured quantity, and should be given the primary attention in the discussions of Fig. 6 and elsewhere. We agree that partitioning into GPP and Re involves aforementioned uncertainties, and will modify the discussion of these quantities as advised.

7. Line 504-507: Nice paragraph. I personally really liked Figures 5, 6 (top), 7, and 8 as their messages are more straightforward and succinct. – Thank you very much for the kind words!

8. In Figure 9, panel (e) is referenced in the caption but absent from the figure itself. – Sorry, that panel had been removed from the figure, but the caption was by mistake not updated.

9. In Figure 9 (c), since meteorological forcing is measured at a single tower, it is unclear how the separate Penman-Monteith estimates for the control and harvested blocks were derived. The forcing inputs should be identical for both, as they are measured by the same tower. The authors should clarify whether wind direction criteria, analogous to those used for EC flux attribution, were also applied to partition the meteorological forcing between blocks. If so, we might be looking at different times (which block is dominant) in Figure (c) and comparing them. Its same confusion as in point 4. – The potential evapotranspiration (PET_{PM}), representing here weather-depended reference ET assuming a uniform reference vegetation, is the same for both blocks, while the sap flow timeseries are coming from each block individually. This will be clarified.

10. In Figure 10, the colorbar for gsT (panels c–d) is inverted relative to all other panels; higher values are shown in blue and lower values in red, while the reverse is true for the others. This creates confusion unless it's stated in the caption. – Thank you for pointing this out, it will be clarified in the text. Higher canopy temperature does imply lower transpiration and lower stomatal conductance. Since the reader already associates higher temperature and stress with red colours, the g_{sT} is coloured in the same manner to preserve the colour association, hence the inverted colourbar.

11. Furthermore, the figure 10 appears to show higher canopy temperature in the harvested block, alongside lower gsT, which is physically consistent with the idea that higher canopy temperature indicates greater water stress and reduced stomatal conductance. However, the accompanying text states that higher drought-period temperatures in the harvested block led to markedly higher gsT estimates. (Line 565). This contradicts the figure in my view. Please clarify. – "Markedly higher r_{sT} estimates" were meant, but mistyped as g_{sT}. Thank you for this comment, it would be best to correct this as "markedly lower gsT estimates".

12. In Figure 11, for consistency, please use the same colors for the control and harvest blocks as in the rest of the paper (blue and red, respectively), rather than distinguishing them by line style here. This makes it difficult to follow. – will be done

Discussion

The discussion section is well-written. – Thank you very much for the positive evaluation!

In section 4.2.2, the author could further elaborate on the temporal mismatch between UAV-derived stress indicators, particularly NDVI and temperature-based stomatal conductance. Water stress driven by VPD or soil moisture deficits manifests rapidly in stomatal conductance and is therefore detectable at the timescale of individual flight acquisitions. NDVI, however, reflects changes in canopy spectral properties tied to the structural and biochemical state of the photosystems, which respond to stress over days to weeks. Consequently, a single flight acquisition may capture stress responses in temperature-based conductance while missing the corresponding signal in NDVI entirely – We will develop this part of discussion as advised. This is an important note, thank you.