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.

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.

2. Consistency in color schemes and legend readability should be improved across several figures.

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.*"

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. 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.

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?

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.

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.

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.

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.

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.
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.
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.

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).
5. Figure 6: Legend are too small to understand in the top row.
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).

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.
8. In Figure 9, panel (e) is referenced in the caption but absent from the figure itself.
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.
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.
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.
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.

Discussion

The discussion section is well-written.

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

Cheers,

Prajwal Khanal