The manuscript presents an analysis into characteristics of the wind resource over a forested area in the U.S. Southeast. The topic is of interest due to limited number of previous studies in that particular area and due to addressing issues that are general for wind power in other forested areas as well. The presentation of the material is clear, and the structure is easy to follow. The figures are pleasant in appearance. I do see several major issues with the conclusions that the authors have drawn and the way that the analysis is performed. A major weakness is the definition of the drag coefficient used in the study. The way that it is calculated limits the conclusions that can be drawn due to the omission of atmospheric stratification and/or turbulence length scale from the analysis, the use of single height momentum flux and wind speed in the calculation and the lack of direct quantification of plant area density in the expression. Furthermore, the usefulness of some of the statistics presented in the manuscript, such comparison between correlation coefficients is not clearly motivated and does not seem self-evident. I suggest that the authors revisit the way that the drag coefficient is determined from the measurements, use the tower 2D anemometers together with estimations of plant area densities to calculate a reference drag coefficient. This, together with studying the effect of atmospheric stratification on the drag coefficient defined in Eq. 10 of the manuscript, either by use of the heat flux from the 3D sonic or by studying diurnal variations, would permit better founded conclusions (that may still not be as far reaching as the present conclusions, unfortunately). I furthermore suggest that the authors relate their work on heterogeneity to the body of work that already exists on the blending height and footprint of flow over forested areas. Finally, I am slightly surprised that a manuscript with 8 authors dedicated to the role of "providing feedback and supervising the work" is not more grounded in the previous literature and methodology of the field.

Detailed comments follow below. After section 4.1 I have not provided specific comments, reflecting my opinion that those sections need to be rewritten. **Authors' Response (AR)**: We sincerely appreciate the time spent by the Referee reviewing our manuscript and the constructive feedback we received. We thoroughly reviewed the manuscript improving language, clarity of figures and tables and rephrasing goals and conclusion of the present analysis to facilitate readers' comprehension. The following major changes have been implemented in the revised manuscript:

- The atmospheric stability regime is now added to the analysis based on the Obukhov length (L) calculated by the 3D sonic anemometer situated at the NEON site. After a sensitivity analysis, neutral conditions throughout the campaign are identified by: $|L| > 2{,}000\ \mathrm{m}$. The displacement height and shear exponent are averaged based on different stability regimes (stable, neutral and convective) and the results are discussed accordingly.

- The correlation analysis (cf. with Section 4.2 of the old manuscript version) is now removed from the update manuscript based on the feedback received by both Referees. The authors agree with the necessity of a more in-depth analysis of the mechanisms that cause the site variability, and the correlation analysis cannot provide this answer due to the lack of below-canopy data available at the clearing sites. Thus, this analysis will be addressed in a future publication.
- The analysis of the site heterogeneity is improved by adopting the Kolmogorov-Smirnov test under different flow conditions, namely region II versus region III operating conditions, Fall versus Winter and Spring seasons and stability regimes. The analyzed wind resources are the horizontal mean velocity (U) and the standard deviation of streamwise velocity (σ_u) which has been utilized to replace the TI based on Referee's suggestions. The results inferred from this analysis are discussed and conclusions are revised accordingly.

Please find our answers in the remainder of this document. Referee's comments are reported in blue Italic font, while our replies are in black normal font. Line numbering refers to the marked-up version of the manuscript.

line 9: Lidar -> Lidars. **AR**: Corrected.

line 19. Unclear at this Point in the read what "correlated" refers to. C_d is normally a constant, but correlation assumes variation. **AR**: This sentence is removed from the revised manuscript.

line 20. Why correlate TI and not sigma_u? U is already examined and there is a lot of covariation between sigma_u and U. In fact, omitting effects related to outer scales such as the PBLH, TI should be constant for a given atmospheric stratification. That means when you are correlating changes in TI, you are actually correlating changes in stratification. **AR**: The correlation analysis is removed from the updated manuscript. In Section 4.4 ("Spatial heterogeneity of wind resources"), the TI is replaced by σ_u to avoid any bias due to the presence of *U* in the definition of TI.

line 20. Instead of using "site heterogeneity" I suggest you use "blending height" unless there is a specific reason you have opted not to. **AR**: The blending height is related in literature to the horizontal heterogeneity specifically induced by roughness sublayers. In our study, we cannot exclude the presence of multiple sources of horizontal heterogeneity, including changes in terrain elevation, flow variability induced by local topographic features and, finally, local forest density. Thus, we prefer not to refer to the blending height as it points to the sole canopy effects and instead refer to site heterogeneity. However, we acknowledge that our analysis is similar to those focusing on the effect of roughness sub-layers onto the flow homogeneity aloft. We added literature citations showing similarities and differences between literature and our approach (lines 533 – 537): "The blending height has been quantified for flows tall forests (Raupach et al., 1996), wind tunnel studies of boundary layers over roughness sub-layers (Tieleman, 2003) and meteorology applications (Raupach and Finnigan, 1995). For the present study, the horizontal heterogeneity may be induced by multiple factors independent from canopy heterogeneity (i.e. changes in terrain elevation, local topographic elements, etc.). Thus, we will refer to h_* as the maximum height of statistical flow heterogeneity."

Table 1 - Location and height of tall towers in the U.S. South-East

City	Latitude (N)	Longitude (W)	Tower height [m above ground]
Edenton (NC)	36° 09' 54.94"	76° 37' 12.78"	345
Hiwassee (VA)	36° 57' 07.85"	80° 35' 45.90"	198
Pulaski (VA)	36° 59' 26.38"	80° 38' 53.66"	198
Lynchburg (VA)	37° 25' 15.41"	79° 02' 16.97"	218
Eagle Rock (VA)	37° 41' 53.81"	79° 43' 28.77"	198
Duo (WV)	38° 07' 08.28"	80° 35' 30.63"	296
Mount Storm (WV)	39° 16' 21.89"	79° 10' 03.91"	391
Keyser (WV)	39° 18' 50.54"	79° 08' 18.59"	353
Demopolis (AL)	32° 32' 30.19"	87° 48' 14.00"	159

line 58. Nowadays there is the standard to measure with met towers much higher than 100 m in many parts of the world. Please specify regional restrictions to clarify to the reader if this is the case here, otherwise revise the sentence. **AR**: Based on the list of ground obstacles provided by the Federal Aviation Administration (link), we identified 9 met towers in the United States South-East taller than 100 m and reported their information in Table 1. We cross referenced the latitude and longitude of each met tower against those available on the AmeriFlux database (i.e. the network managing met tower data collection and sharing in the United States) and noticed that none of the met towers has publicly available data on the AmeriFlux network. Thus, in order to obtain a thorough wind resource estimate above 100 m in the U.S. South-East, in-situ met tower measurement must be complemented by remote sensing Lidar anemometry.

line 65-66. That the lidar showed promising ability to reproduce turbulence up to 4-th order statistics seems like a rather serious misrepresentation to me. If I recall correctly the tower was only 60 m high and was placed 5 km away, limiting the ability to draw any conclusions at all regarding the ability of using the lidar for turbulence at rotor relevant heights. Also, I seem to recall a rather serious underestimation of the wind by the lidar at the tower comparison. I think the reference must be updated to more accurately represent the findings in the study.

AR: In Lee *et al*. (2025), the authors conducted an assessment between Lidar- and tower-based measurements of vertical velocity variance (σ_w^2) and TKE (cf. with Fig. 5 of Lee *et al*., 2025) and found acceptable agreement both during daytime and nighttime. The differences in the mean wind speed between Lidar and met tower were justified by the onset of early-morning conditions associated with the rapid growth of the ABL and the subsequent spatial change of wind direction; thus, the instruments showed an overall good agreement except for those periods.

Between 40 m and 300 m above ground, the authors quantified diurnal-averaged distributions of Lidar-derived skewness (*S*) and kurtosis (*K*) of the vertical velocity component (cf. with Figures 5, 7, 8, 11, 12, 13, 14, 15, 16, 17 and 18 of Lee *et al*., 2025). The results, based on *S* and *K*, indicate buoyancy-induced vertical motions during day vs. nighttime, as well as enhanced vertical motions during clear-sky conditions as opposed to cloudy conditions. These scenarios are consistent with classic ABL studies and therefore emphasize the possibility of utilizing Lidars to resolve flow's high moments over complex terrain. Finally, in their conclusion Lee *et al*. (2025) reported: "the wind speed, as well as the TKE and σ_w^2, obtained from the lowest sampling height of the wind lidar at $\approx 1.5h_c$ [the canopy height] showed reasonably good agreement with observations obtained from analogous sampling heights at the nearby micrometeorological tower.", while in the Abstract the authors reported: "Our results provide insights into turbulence processes over forested complex terrain and support the refinement of turbulence parameterizations used in weather forecast models." Thus, we believe it is correct to state that this study shows promising pathways to utilize Lidars over complex terrains.

line 67. Please clarify what distances you are referring to. **AR**: We refer to distances of the order of 1 km (now added to the text at line 98).

line 89. "Conclusions are discussed" is a bit ambiguous. Clarify if it is discussion and conclusions or only conclusion. **AR**: This sentence is modified as: "conclusions are reported in Section 5." (lines 123).

line 99. Please clarify if you mean LAI (Leaf Area Density the area of the leaves) or PAI (Plant Area Density the area of all forest biomass). The wind energy community has an unfortunate tendency to use the label LAI when actually using data and model assumptions that reflect PAI. Since this study refers to differences specifically related to the presence of leaves, it is especially important to get this right. **AR**: It is now specified that we refer to the LAI as the ratio between leaf area and ground area (line 136).

line 107. Small comment, but all these variables are not measured at 20 Hz. The wind vector and virtual temperature are. The rest is post-processed to 30-minute blocks, I assume. **AR**:

This is correct. The sentence is rephrased as (lines 146): “[…] to measure the three-dimensional wind vector and temperature with 20 Hz sampling rate and calculate, among other variables, the mean wind speed, direction, TI, and friction velocity (u_*); here we use the 30-minute averages of these quantities.”

line 124. While a good hypothesis, I think that it is premature at this point in the study to attribute the variation of wind speed just above the canopy to a general variation in momentum absorption. The displacement height is also likely to change and while the wind speed may be lower just above the forest, changes in turbulence length scales may mean that the situation is different higher up. It is also unclear how effects of atmospheric stratification, which also have a seasonal cycle, plays into this. **AR**: After introducing the monthly-averaged cycle of wind speed resolved by the 3D sonic at 30 m height (Fig. 2 in the manuscript), it is observed that months characterized by high (low) wind speed correspond to months with low (high) leaf density. Considering that the measured height is relatively close to the canopy top (1.7*h*), it is logical to assume that a significant portion of the wind speed variability is caused by transitions in the leaf coverage – at least for this height. Additionally, atmospheric stability has larger footprint at the diurnal timescale rather than the seasonal one. Thus, we believe that the velocity distribution of Fig. 2 is primarily related to the canopy senescence.

*line 125. Drag is not just PAI, it is the vertical integral of PAD*U^2. So, while the plant area density may be higher, the wind speed is lower so one cannot tell on the PAI alone if the drag is actually higher or lower unless a quantified analysis including wind speed is presented.*
AR: This sentence is rephrased as: “due to the higher foliage density present in the canopy and the lower wind speed in the above-canopy space.” (lines 165 – 166).

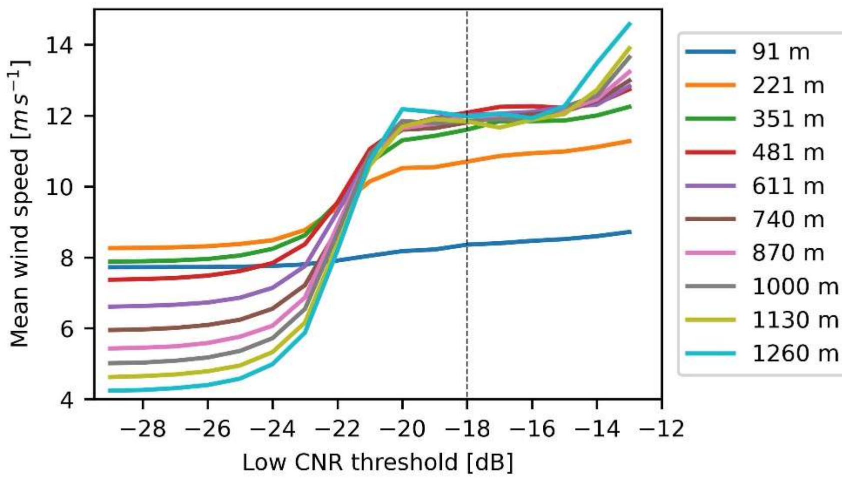

Figure 1 - Sensitivity of the horizontal mean velocity calculated from the 6-points VAD at Treetops on the low-SNR threshold. Ten heights within the ABL are reported. The vertical dashed line reports the lower CNR limit currently adopted in the manuscript (-18 dB).

line 153. Filtering on CNR means there may be an implicit filter on wind speed (see work by Gryning). Perhaps this could be worth mentioning in the discussion. **AR**: To address this point, we studied the sensitivity of the horizontal mean wind speed on the choice of low SNR threshold ($\mathrm{SNR_{low}}$) for the scanning Lidar located at Treetops. The threshold is chosen within $\mathrm{SNR_{low}} \in [-29, -12]\mathrm{dB}$ with 1 dB incremental step size. For each $\mathrm{SNR_{low}}$ value, the instantaneous radial velocity probed at a certain height by each of the six VAD beams have been set to Nan if the associated SNR value is below $\mathrm{SNR_{low}}$. For each 30-minute period sampled at each height, the mean velocity is then calculated based on the algorithm of Paschke *et al*. (2015) and, finally, the overall average is calculated at each height for the entire duration of the campaign. The results are reported in Fig. 1 of this document. It is noticed that, below -23 dB, the mean velocity shows little sensitivity to the SNR threshold; however, as the reference height goes above 221 m, the mean wind speed monotonically decreases. This result is unphysical and it is related to the residual effect of low-CNR outliers in the radial wind speed; thus, the low CNR threshold cannot be selected within this interval. By contrast, as the low CNR limit is assumed between -20 dB and -15 dB, the mean wind speed shows little sensitivity and monotonically increases as the height increases. For lower CNR limits larger than -15 dB, the mean wind speed increases, thus it becomes sensitive to the choice of CNR threshold. In conclusion, the current choice of -18 dB does not impact the present quantification of mean velocity throughout the ABL based on scanning Lidars. In the revised manuscript, at lines 200 – 206, it is reported: “Gryning and Floors (2019) emphasized that the choice of low-SNR limit may induce an artificial over-estimation of the horizontal mean velocity in the ABL. To avoid this issue, a separate analysis (not shown here) has been conducted to quantify the sensitivity of the mean velocity resolved by the scanning Lidars throughout the ABL on the choice of lowest acceptable SNR. Little sensitivity was found for lower thresholds in the interval [-20, -15] dB. Thus, the current choice of rejecting instantaneous data points featuring SNR below -18 dB does not introduce any artificial bias in the following mean velocity estimates."

line 158. I suspect there was a particular reason for the chosen width (2.5 sigma instead of 3.5), I think it would be appropriate with motivation of the reason for the stricter filter. **AR**: The choice of 2.5σ follows a visual inspection of the time series recorded by the scanning Lidars. The threshold provided in literature (3.5σ) left a significant presence of outliers in the velocity time series, thus we chose a more restrictive threshold to discard the presence of outliers.

line 163. Given the filter for spike removal ($\pm 2.5\sigma$) one would expect only a few percent of the data being rejected if the radial wind is normally distributed. The numbers in figure 4 report much larger rejection rates which point to very long tail(s) of the radial wind distribution. It would be good to know for the reader if the rejection rates include only the data rejected by the $\pm 2.5\sigma$ -filter or if data rejected by any internal filters of the lidars are also included. As far as I know the ZX does not use CNR, so I guess there must be difference between the scanning lidars and the ZX lidars in this respect? **AR**: The spike removal is implemented after the CNR filter; thus, the data rejection percentage plotted in Fig. 4a of the manuscript is due both to the decreasing CNR along the line-of-sight and to the outliers removal. This is now specified at line 214: "The percentage of data rejection due to the quality control (both SNR and outlier filters) is reported in Fig. 4".

Regarding the profiling Lidar, the filter is applied on the instantaneous values of wind speed by rejecting points outside of [0, 30] m s^{-1} (cf. with line 196 in the manuscript) due to the lack of direct quantification of the CNR.

Eq. 6 and 7. Please clarify if the wind direction relative to the rotor plane has been taken into account or not. **AR**: At lines 280 – 281, it is added: "It is noticed that, in Eqs. (6) and (7) the wind direction is assumed perpendicular to the rotor plane."

Line 260. The momentum loss of importance is that through the entire canopy and the use of "interface" is somewhat ambiguous in that sense. **AR**: We apologize for any confusion we created. This sentence is now rephrased as "the momentum absorption at the canopy interface." (lines 332).

Eq 10. The Yi 2008 paper presents a rather elaborate investigation into the C_d dependency on height within the canopy. Crucially, the definition of C_d also assumes that there is a local (to each height) balance of the shear stress divergence (see eq. 8 or 10 of Yi 2008 for instance). The rest of the paper studies the vertical variation of C_d, which is a different thing from the vertical variation of the canopy density. In your Eq. 10 you have only a bulk relation between *the shear stress and the local velocity and this is very different. I think the citation of Yi is very misleading. Bulk relations, such as your Eq. 10 are normally used over surfaces with very small displacement heights, to represent the skin (or surface) friction but I would argue that they are unsuitable when the displacement height is a factor, in other words, when the drag is distributed vertically. In your case, the density of the vegetation will vary across season, something that will be implicit in your C_d value and that makes it very different from the C_d value of Yi 2008 and most of the C_d values used in the wind energy community.* **AR**: We acknowledge that the reference to Yi (2008) may cause confusion in the present context, and it has been removed from the text. We also acknowledge the difference between local and bulk drag coefficients. Although below-canopy velocity measurements are available from the met tower at NEON site, no independent estimate of the LAD is available from the NEON database, thus we cannot estimate the vertical distribution of drag coefficient. Instead, we rely on the drag coefficient calculated at the canopy top as a proxy for the seasonal evolution of canopy roughness induced by the leaf senescence.

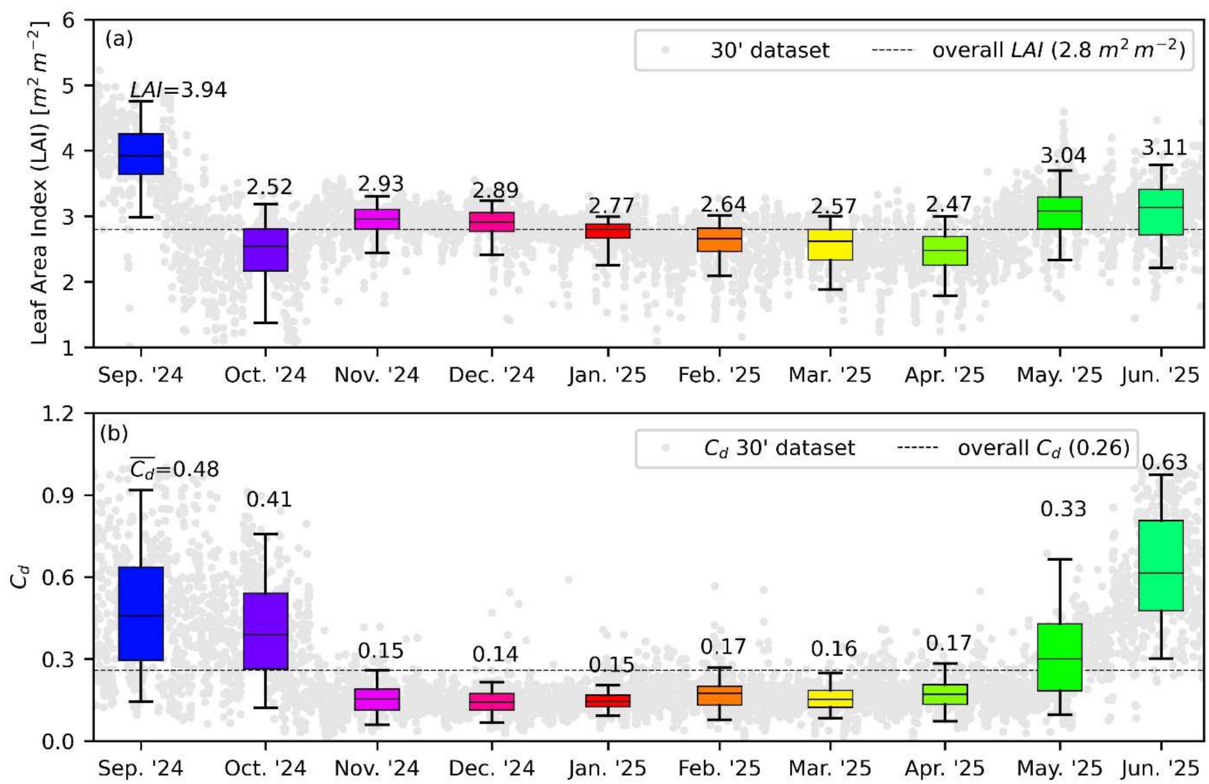

Figure 2 - Quantification of canopy roughness evolution across months at the NEON site. (a) Leaf Area Index (LAI) calibrated over 30-minute averaged below-canopy velocity profiles. (b) Drag coefficient time series and monthly statistics. The numbers report the monthly averaged LAI and C_d.

To prove that the drag coefficient is representative of the seasonal leaf cycle, we calculated the Leaf Area Index (LAI) over each 30-minute mean velocity profile below the canopy (measured by 2D sonic anemometers at 0.3 m, 4 m, 12 m and 19 m above ground). In particular, the LAI is calibrated from the exponential velocity model (Cionco, 1965):

$$\frac{U(z)}{U_h} = \exp\left[0.5 \text{ LAI}\left(\frac{z}{h} - 1\right)\right].$$

Here, *h* is the canopy height (18 m) and U_h is the mean velocity at the canopy top. The LAI values calibrated during the LEAFF campaign at NEON are reported in Fig. 2a of this document (both as 30-minute values and as monthly-averaged statistics) and compared with the C_d time series. It is noticed that months featuring relatively low C_d (< 0.2 as monthly average, Fig. 2b), i.e. November through April, are also characterized by relatively low LAI (< 3 on average), whereas months characterized by high C_d (> 0.3) are also characterized by $\text{LAI} > 3$ (with the only exception of October). The analogy between the monthly-averaged distributions of LAI and C_d corroborates the initial assumption that the drag coefficient is a robust parameter to model the canopy roughness.

Furthermore, given a fixed height in the atmosphere, the RHS of your eq.10 will be a function of the atmospheric stratification (it is the reciprocal of the square of U/u_*) which makes it unsuitable to evaluate on seasonal basis (given there is a seasonal variation in stratification). Finally, the equation should contain the total shear stress, not just the longitudinal component. **AR**: The variability of C_d introduced by the atmospheric stratification is addressed in the remainder of this document by distinguishing C_d values quantified for different stability classes. The definition of shear stress is now corrected adding the transversal component (Eq. 10).

Line 278. I wonder if the non-zero intercept is rather that you are fitting a linear curve to a nonlinear relationship. The volume filter of the lidar is fixed with time, but the length scale of turbulence is very different in stable and unstable conditions, hence the lidar measurement should agree better for larger values of the shear stress (since they tend to happen in unstable conditions when the length scale is larger) and vice versa. **AR**: based on another Referee's recommendation, the estimate of the shear stress based on Lidar data is now removed and replaced with the analogous from 3D sonic anemometry. Figure 6 is removed from the revised manuscript.

Line 285, Figure 7. I wonder how much the distribution is actually due to the atmospheric stratification. I made a quick using measured values of z/L and plotted "C_d" from the square of the reciprocal of the MOST-wind profile. While the value needs to be adjusted for the "missing" plant area to match your value, there is considerable variation even though the "plant area" is not changed. The variation only comes from the variation in the length scale of the turbulence in relation to the height z-d. **AR**: Based on another Referee's feedback, the quantification of C_d is now performed based on the turbulent shear stress calculated by the 3D sonic, as opposed to extrapolate it from the profiling Lidar shear stress profile. The pdf of drag coefficient is therefore re-evaluated and plotted in Fig. 3 of this document based on different stability regimes. Convective conditions are characterized by larger drag coefficient on average (0.30) than neutral and stable conditions ($C_d = 0.23$). This is consistent with previous literature studies that reported higher drag coefficient due to enhanced vertical buoyancy motions (e.g., Bradley, 1971; Mahrt et al., 2001). However, it is noticed that the previously observed variability in C_d (i.e. between <0.1 and 1.0) is still present when the drag coefficient values are divided into different stability classes. This supports the hypothesis that the main source of variability of C_d is the seasonal evolution of the canopy roughness.

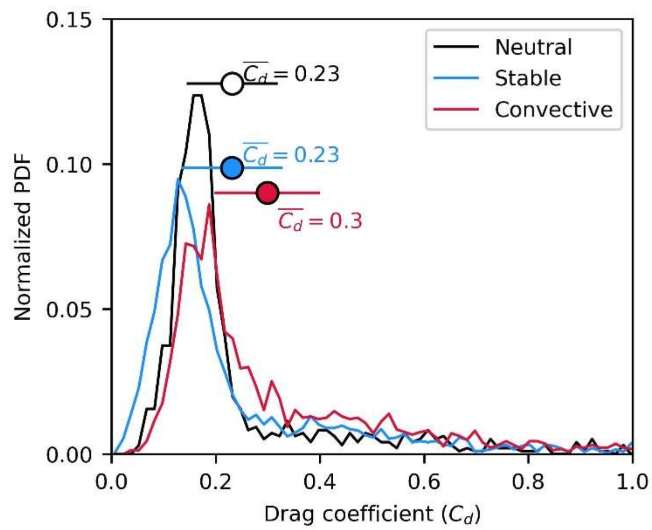

Figure 3 - Normalized pdf of drag coefficient evaluated during the LEAFF campaign under different stability regimes. The distinction between neutral and non-neutral regimes is based on an Obukhov length value of: $L = 2{,}000\ m$. Circle symbols report the average values, while uncertainty intervals report the standard deviation

Lines 300-308. It is important to differentiate between the effects of displacement height and roughness length. First you state that the roughness is larger, then that it is the displacement height. A denser natural canopy would normally mean a larger displacement, but a lower roughness. In the same way it is by no means necessary that a denser canopy has more drag (it is blocking the wind and the drag is proportional to the plant density times the wind speed squared). Figure 6 in Jackson 1981 gives a good illustration of this. **AR**: These lines have been rephrased to emphasize that the difference between forest vs. clearing site could be a possible explanation to the observed differences in displacement height, yet it cannot be verified by the current experimental setup. At lines 425 – 432, it is now written: "Recalling that MLBS and Treetops are located within small clearing areas, while the NEON site is within the forest canopy, the present scenario could be due to the lower roughness present at MLBS and Treetops. A more robust verification on canopy influence would require wind measurements over a nearby site that had zero forest canopy influence; we note that the small clearing sizes at MLBS and Treetops excluded this."

Line 316. See above comment. **AR**: We have rephrased this sentence emphasizing the role played by the site location without pointing exclusively to the differences in canopy roughness. At lines 453 – 455, it is now reported: "In conclusion, the present results demonstrate that, for tall wind turbines over complex terrain featuring hub-height 6.1 times taller than the forest layer, the site location is still a crucial factor impacting the available wind resource."

Line 339. As the authors themselves argue in the above section, there is considerable variation due to the diurnal cycle and the same should then apply to the annual cycle. Whereas the annual cycle also has vegetation variations, the diurnal does not. Perhaps a good test is to redo figure 10, but for an average diurnal cycle, to verify that what you see is not simply the cycle of PBL stratification. **AR**: To address the effect of diurnal cycle in the distributions of $\widetilde{U}$ and C_d, the monthly averaged values have been calculated separately for different stability regimes. The results (plotted in Fig. 4 of this document) show that, for each month, the averaged C_d values are quantitatively similar across different stability regimes. Conversely, significant variability is found in the monthly-averaged distributions of $\widetilde{U}$ in that convective conditions are associated to lower wind speed at the ABL top. This is caused by the upward momentum transport induced by daytime thermal buoyancy. Thus, thermal stability has marginal influence on the seasonal evolution of drag coefficient.

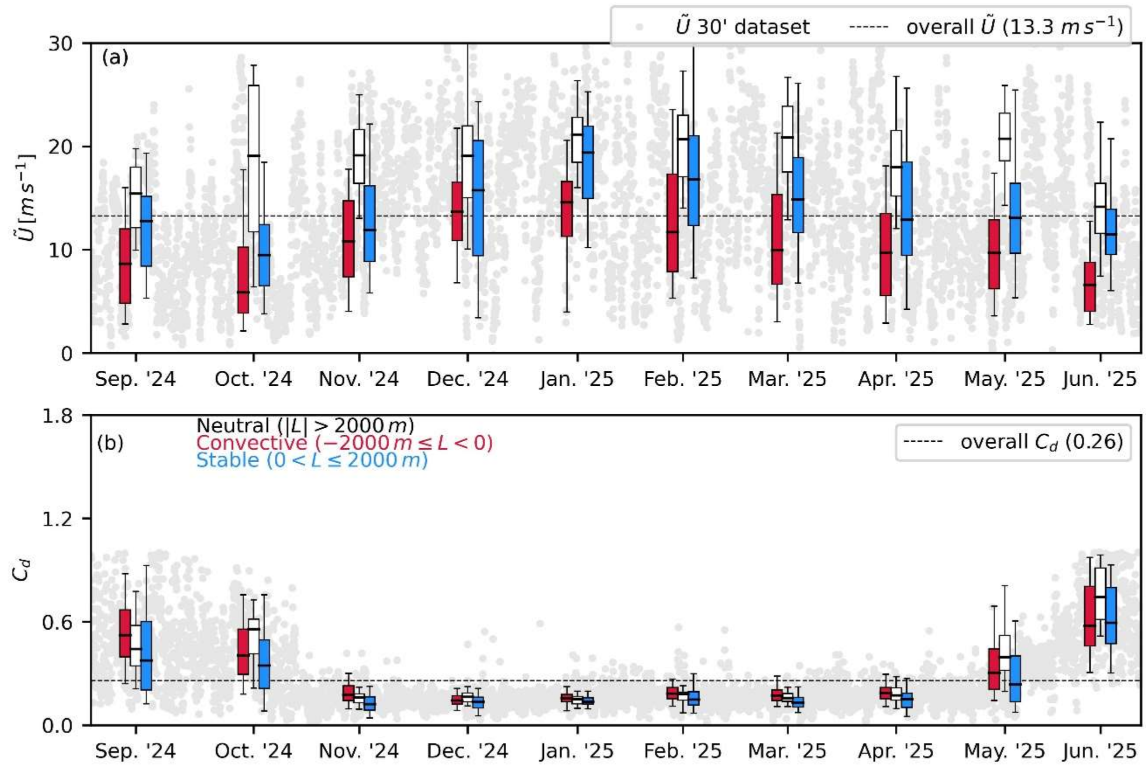

Figure 4 - Quantification of wind variability across months at the NEON site for different stability regimes. (a) Mean wind speed every 30 minutes probed at 1 km height. (b) Drag coefficient time series and monthly statistics; the numbers report the average Cd for each month. Box colors represent convective (red), neutral (white) and stable (blue) regimes

Line 348. Again, you have not actually shown that momentum absorption in the canopy is lower. **AR**: The displacement height represents the height of mean momentum absorption within the roughness sub-layer. Thus, even if the variations of d are not solely related to the changes in the canopy roughness, it is still correct to say that higher (lower) d corresponds to higher (lower) momentum absorption. In the manuscript, this sentence has been reformulated as (lines 477 – 478): "The result, plotted in Fig. 11, indicates lower displacement height (10.0 m, 10.2 m and 13.2 m at Treetops, MLBS and NEON, respectively) during November-March which indicates lower momentum absorption (Fig. 11a)."

Line 355 and above paragraph. I think it is important that you consider and demonstrate that what you see in these figures is not the atmospheric stratification. The shear exponent is a very strong function of the atmospheric stratification and the variation shown in Fig. 11 seems consistent with the changes you would expect from the seasonal cycle of stratification. **AR**: For data collected at NEON, we repeated the statistical analysis of d and α distinguishing between different stability regimes. This analysis is not repeated for MLBS and Treetops data due to the lack of temperature measurements at these sites. The results are plotted in Fig. 5 of this document and Fig. 12 of the revised manuscript. For all stability conditions, the displacement height (Fig. 5a) is larger during the period of high C_d (and higher leaf density) and low synoptic wind (September – October). The uncertainty intervals do not allow us to draw any conclusion on the effect of thermal stability on the displacement height. By contrast, the shear coefficient shows a clear dependency on the atmospheric stability (Fig. 5b). Stable conditions feature larger shear exponent than neutral conditions due to suppressed vertical mixing characterizing nighttime conditions. On the other hand, daytime convective conditions feature lower shear exponent due to enhanced buoyancy mixing as compared to neutral conditions. For all stability conditions, the months featuring lower C_d and higher synoptic speed (November – March) are characterized by larger values of α than months characterized by lower synoptic speed (September – October). In conclusion, the dependency on synoptic conditions is still present when the shear exponent is averaged based on distinct atmospheric conditions. This is now stated at lines 493 – 540.

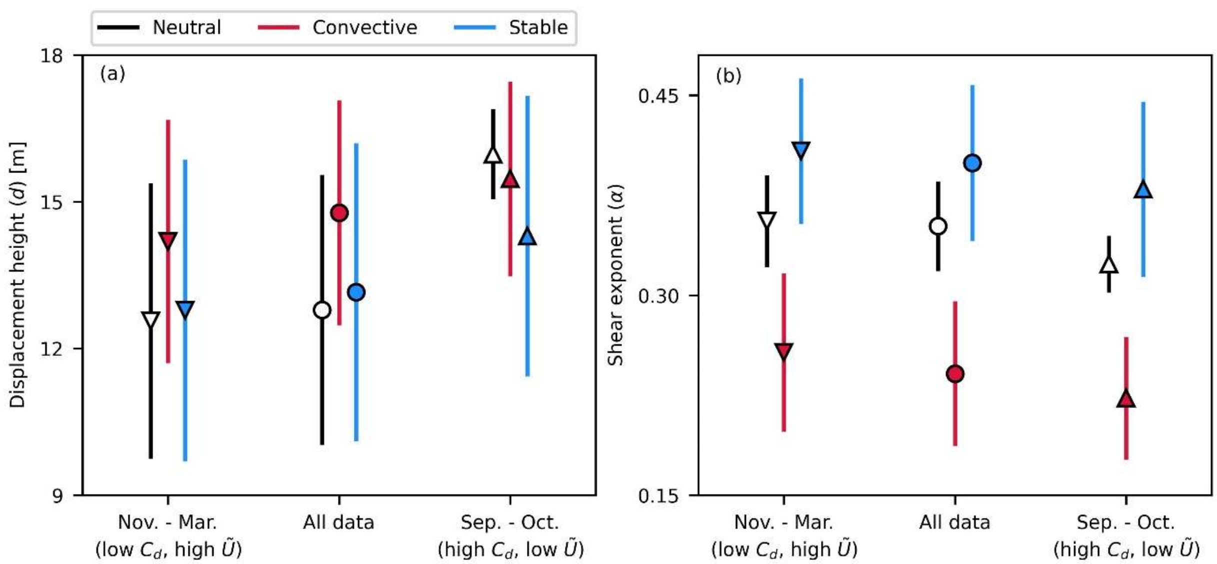

Figure 5 - Statistics of mean velocity parameters (d, α) calculated for different stability conditions distinguishing between months of high C_d/low $\tilde{U}$ (September-October) versus months of low C_d/high $\tilde{U}$ (November-March). (a) Displacement height, d. (b) Shear exponent, α. Symbols refer to mean values, while the standard deviation is reported by the uncertainty intervals.

Line 360. I don't think you have shown this. It is a strong statement that requires careful investigation. See comments above. **AR**: We agree with the Referee about the importance of carefully relating the observed wind resources to the momentum absorption from the canopy. The interpretation of the results arising from the correlation (previously present in Section 4.2) deserves a more in-depth study and should be inferred from all the sites examined in this work to obtain a meaningful site-to-site comparison. For these reasons, the correlation analysis is removed from the revised manuscript, and it will be addressed separately in a future project. For the present study, we focus on the site-to-site comparison of wind resources and, subsequently, characterize the site heterogeneity of the wind statistics.

Section 4.2 This whole section needs to be reconsidered. I don't really understand the motivation behind the analysis. C_d is a nondimensional property, so it is clear that $\widetilde{U}$ will correlate better with U than C_d will. How should the results be used? For me it is difficult to understand from the plots what is spurious correlation and what is a physical connection between the variables. In Figure 14: How come Cd and TI have so low correlation close to 30 m? It is almost the same quantity. Same goes for Figure A1, it is surprising to me that the correlation is zero between Cd (essentially TI squared) and hub-height TI. **AR**: Please see comment above.

Section 4.3, discussion around figure 15. I don't think that this analysis actually shows how heterogeneity of the surface is reflected on the wind resource. The correlation could come from non-local features such as large-scale variations and I suspect it is highly sensitive to the averaging time used to determine the statistics. Considering a situation with an offshore site, it is unclear to me what the differences would be to the plots you present. One would still expect to see less correlation on lower height simply due to the random-error effect of turbulence on the statistics. This is also consistent with the observation in lines 424-425 since stronger wind resource likely means larger variations. **AR**: We thoroughly reviewed Section 4.3 (now Section 4.4 in the revised manuscript) and changed the analysis in the following aspects: first, the site heterogeneity is quantified for U and σ_u instead of TI. Second, the analysis is repeated for different operating regions of the reference turbine (region II and region III), different seasons (Fall vs. winter-spring) and different stability regimes. Overall, it is shown that higher wind speed flows are associated with enhanced heterogeneity, both in U and σ_u. Consistently with the Referee's observation, this result could be motivated by non-local large-scale flow fluctuations, and therefore not related to the canopy roughness. Thus, any direct mention of the canopy roughness as cause of the site heterogeneity has been removed. The revised Section is reported at lines 541 – 621.

Section 5. I believe the whole section needs to be rewritten following further analysis into the role of atmospheric stratification on the results. I would have liked to see that the heterogeneity section relates to previous work on blending heights above forests and potentially also on footprint. I am skeptical of the method and value of comparing correlation coefficients of Cd (as per Eq. 10 in the manuscript) and U_1km with the wind speed at the rotor heights. Firstly, a direct comparison is of limited value since dimensions of the compared quantities are different, secondly, the 1 km wind speed is very rarely available in wind resource estimation, so I would like to see a better motivation of how the results are useful. **AR**: The Conclusion has been rewritten after a thorough review of the manuscript and reorganization of the results. As previously stated, the correlation analysis is now removed from the revised manuscript, and the analysis of site heterogeneity has been investigated more thoroughly. Additionally, we indicate the canopy roughness as a possible cause of the observed site variability of wind resources, yet we acknowledge that other factors may justify this result – for instance terrain elevation, topographic features, internal boundary layers, etc. . The conclusions are now summarized as follows:

- The site immersed in the forest exhibited the lowest capacity factor (−3.9% to −0.9% with respect to the other two sites), the highest shear exponent, and the highest displacement height with respect to sites located in clearing areas. Although these results may indicate an effect of canopy drag on the mean velocity profile within the rotor area, other concurrent effects cannot be excluded a priori, e.g. topography-induced flow features, changes of terrain etc. . What emerges from this analysis is that, even for tall wind turbines (hub height of 110 m), the wind resources are still dependent on the choice of site.
- The difference in wind resources at the tested sites is even more evident when the wind resource assessment is carried out during different seasons. Specifically, higher (lower) displacement heights are found during months characterized by high (low) C_d. Similarly, larger (smaller) shear exponent values are found for higher (lower) values of $\widetilde{U}$. When conducted based on stability regimes, the wind resource assessment indicates larger (smaller) values of shear exponent during stable (convective) periods and during periods of high (low) free atmospheric wind.
- The site heterogeneity of wind statistics, quantified by the Kolmogorov-Smirnov *p*-test, reaches from 65 m up to 151 m, thus well within the modeled rotor layer (45 m to 175 m). The site heterogeneity is enhanced during high-wind conditions, i.e. for data points collected in region III of the modeled wind turbines, as well as during the season of high $\widetilde{U}$ and during nighttime stable conditions.

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