

Review: *This is a nicely written study with good and clear figures. However, there are a few relatively serious issues with the analyses, and a major revision is suggested.* **Authors'**

Response (AR): We sincerely appreciate the feedback by the Referee and the constructive comments. We thoroughly reviewed the manuscript's text, figures and tables to address the Referee's comments. 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: $|L| > 2,000$ m. The displacement height and shear exponent are then 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 observed site-to-site inhomogeneities, 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 conducted separately in future publications.
- The analysis of the site heterogeneity is improved by adopting the Kolmogorov-Smirnov test under: 1) different flow conditions, namely region II versus region III operating conditions; 2) Fall versus Winter and Spring seasons; 3) 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 responses below. Referee's comments are reported in blue Italic font, while our responses are reported in normal black font.

Overview comments:

Site description – In the introduction (lines 65-67), the authors state that the purpose of the paper is to study site representation in context of micro-scale variability. To do this, readers need to be informed about microscope variability in terrain and forest cover, but this information is not given (see comments to section 2.1 below). **AR:** We added further details on terrain variability and Leaf Area Index to address this comment. In Figure 1b of the manuscript, we have added the altitude above sea level of each observational site. Furthermore, at lines 139 – 140, it is added: “Within an area of 5 km – by – 5 km around the

facility, the terrain surrounding MLBS is characterized by an average elevation of 1,101 m with standard deviation of 119 m.” Although the National Ecological Observatory Network does not provide direct estimates of the Leaf Area Index (LAI) for the NEON tower location, we have added a reference that calculates the LAI for the selected site (Wang et al., 2023) and estimates it as $LAI = 2 - 3$ for the forest around the MLBS site.

Characterization of displacement height and shear coefficient – The authors have chosen to characterize the displacement height and shear using each 30-minute profile. Because (1) atmospheric stratification is not taken into account, (2) there is natural variability of the flow over a limited averaging time, the resulting values of both parameters are likely huge. If the purpose is to say something general about the wind profiles at the different sites, it is better to study mean profiles taken over many samples, grouped over different stabilities in the different locations and extract the parameters from those. **AR:** We agree that part of the variability in the mean flow model parameters is due to the thermal buoyancy. We have added a Sub-section (Sect. 3.3 in the revised manuscript, lines 282-295) focused on the quantification of thermal stability via Obukhov length, the latter based on the 3D velocity and temperature probed by the sonic anemometer at NEON. The variability of the mean velocity parameters (displacement height and shear exponent) for NEON data is now distinguished between convective, neutral, and stable thermal conditions, and one figure is added to the manuscript to discuss the effect of thermal buoyancy onto the mean velocity parameters. Due to the lack of temperature measurements at MLBS and Treetops, we cannot draw any conclusion about the variability induced by thermal stratification at these sites. More details are reported in the remainder of this document.

Correlation analysis – The authors interpret the drag coefficient as momentum absorption from the surface. But this is not correct; the drag coefficient expresses a ratio between the drag force (extracted momentum/surface area) divided by wind speed square. For a fixed atmospheric stability, absent streamlining of canopy elements, C_d should be constant. Because both turbulence intensity and drag coefficient are normalized quantities and both vary with atmospheric stability over tall roughness elements (see Zilitinkevich et al 2008 and Arnqvist et al 2015, both in BLM), it is logical that they correlate, but the interpretation that the authors put forward is simply not correct. I am not sure what makes sense to do here, but maybe the authors could consider to study correlations with u^* instead of C_d , since u^* is strongly influenced by local heterogeneities. Also, it would be better to study correlation with σ_u rather than TI . **AR:** We acknowledge the point made by the Referee about the correlation analysis. Based on this feedback, and another similar from a different Reviewer, the correlation analysis based on NEON data is removed from the revised manuscript, and it will be addressed separately in a future project. For the present study, we prefer to focus on the

site-to-site comparison of wind resources and, subsequently, characterize the site heterogeneity of the wind statistics.

Focusing on the choice of C_d versus u_* , the scope is to resolve the seasonal evolution of two of the physical phenomena driving the flow (flow's inertia and canopy drag), thus we need to isolate the effect of changes in the canopy drag from changes in the free-flow inertia. In the above-canopy space, the friction velocity (u_*) is determined by the velocity gradient, hence u_* may change both due to the surface roughness and to the free flow's inertia (see, e.g., Kaimal & Finnigan, 1994). By contrast, the drag coefficient is defined as the friction velocity normalized by a reference mean velocity, thus it is sensitive only to changes in the surface roughness. For this reason, we deem C_d to be a better parameter than u_* to isolate the transitions of canopy roughness. However, we acknowledge that it is incorrect to state that C_d represents momentum absorption. In the manuscript, at line 332, it is now reported: "The latter [the momentum loss at the canopy top] is proportional to the drag coefficient (C_d) [...]"

Scope of analysis and use of scientific literature – The authors claim to study microscope variability using a well instrumented site, but, compared to previous studies that focused on micro-scale effects, their instrumentation and site description are not fit for this purpose. Rather, their strength compared to previous studies over forest is the scanning lidars that can measure way above the surface. The problem with the scanning lidars is that data availability drops with height and the analysis then somewhat loses relevance as a wind siting study. The effect of data section and data availability is not clear enough. Further, when installing turbines in forested areas, there is typically some clearcutting, because the turbines are so large, and because of this, the difference between a clearing measurement point and a site like NEON may not be relevant. Overall, the scope of the study should be expressed more clearly. The authors should also strengthen their use of scientific literature. The citations seem rather random (for example, there is a lot of research literature regarding measurement errors by lidar profilers in complex terrain that is not mentioned) and based mostly on the last five years.

AR: We apologize for any confusion we inadvertently created when we stated the objectives of this research. We thoroughly revised the manuscript and rephrased our goals in light of the feedback received by the Reviewers. Our goals are: first, to establish whether the site location is still a relevant factor able to shape the wind resources for heights 3 to 10 times the canopy height; second, to investigate the long-term evolution of the wind resources at the tested sites; third, to quantify site-to-site heterogeneity of the wind resources for several consecutive months. The Abstract is rephrased to reflect these goals (lines 17 – 21): "The present analysis aims to: 1) establish how variable the wind resource is at closely-spaced sites; 2) quantify the variability of the wind resources in relation to thermal stability, seasonality changes of C_d and free-atmosphere wind speed; and 3) quantify the vertical interval of wind resource heterogeneity at different sites."

From the experimental perspective, the novelty of this study is the long-term deployment of scanning and profiling lidars (both on the ground and on top of a meteorological tower) to accomplish the above-mentioned research goals. This enables unprecedented insights of the flow structure within the rotor area of future wind turbines (i.e., 2.5 to 10 times the canopy height for the tested sites). Although Lidar anemometry is a well-established technique to quantify wind resources over complex terrain, at the time of the experiment there was no published literature addressing the present research goals via observational data spanning the spatio-temporal range achieved in the present study (i.e., 9 months of data recorded up to 1,000 m above ground).

Finally, we agree with the Referee on the importance of having a comprehensive body of citations to properly frame our research goals. Throughout the manuscript, we have enriched our literature review on several topics, including the assessment of Lidar-derived turbulence measurements against high-resolution sensors on complex terrain (lines 73 – 93), effects of atmospheric stability on wind turbine performances (lines 283 – 284), challenges to the applicability of the logarithmic mean velocity model (lines 316 – 317) and horizontal heterogeneity of turbulent fluxes over roughness boundary layers (lines 548 – 549).

Specific comments:

Abstract

Line 4-5: Scientific results regarding how the land surface interacts with the atmosphere are not country specific. And terrain inhomogeneities likely affect the flow the same way in the US as in other countries. Hence, a lack of investigations in the US is not a strong motivation for a new study. **AR:** Although our research strategy has already been implemented in other countries, we believe it is important to emphasize its novelty in the US since the US terrain feature large extents of complex topography covered by forests, yet it lacks comprehensive observational datasets which would be instrumental to assess several numerical weather prediction models operating at regional level in the US (e.g., High-Resolution Rapid Refresh Model).

Abstract -general: there are too many abbreviations and acronyms introduced that are either not used more than once or twice (U , Tl , U^3) or are scientifically irrelevant (LEAFF). This makes the abstract hard to read. Further, Region II is not defined. **AR:** The LEAFF acronym is now removed from the abstract. Due to the limitations of available characters in the abstract dictated by the journal, we prefer to address the definition of Region II in the main text (line 126). The remaining abbreviations are utilized multiple times throughout the abstract; thus, to keep the abstract concise, we prefer to leave them as they currently are.

Introduction: Tower-mounted wind lidars were also used in Mann et al (2008), Dellwik et al (2010) to benchmark against tower data and quantify the flow tilt angles over a small forest. Although it is technically challenging to hoist a lidar to tower top in a dense forest, it is not scientifically important to be the first; the important thing is what you do with the data. Hence, to be the first in the US to place a lidar to the top of the tower is not really that relevant, unless you really wish to discuss the technical challenges. **AR:** We are grateful to the Reviewer for highlighting this aspect. The references to Mann et al. (2008) and Dellwik et al. (2010), among others, are added to the Introduction at lines 86 – 91: “For example, Dellwik et al. (2010); Mann et al. (2010) utilized one continuous-wave Lidar to calculate flow tilt angles on complex terrains and assessed its error against co-located sonic anemometry. Similarly, Traeumner et al. (2012) compared Lidar field measurements near a forest edge against the flow around a wind tunnel model resolved by a high-fidelity Laser Doppler Anemometry apparatus. Upon the positive assessment against in-situ sensors, these results indicate the suitability of the Lidar to resolve momentum-carrying turbulent structures developing from the forest edge.”

Addressing the lack of observational data in the U.S. South-East is one of the main objectives of this analysis, as well as one of the overarching research goals established by our funding agency. Thus, we believe it is important for the readers to know that the present dataset is the first of its kind to be collected in the U.S. South-East. This information may motivate further research in this area, both from the experimental standpoint and from the perspective of assessing numerical weather models operating at regional scale.

Section 2: Table 1 and general. The authors make a clear distinction between profiling and scanning lidars, but the scanning lidars are configured as the standard profilers, and it would be good if this was clarified in the paper. **AR:** In the text we report the scan strategy designed for the two scanning Lidars, i.e. 6-point Vertical Azimuth Display (VAD) mode. This technique is widely common in the atmospheric science community to reconstruct vertical profiles of turbulent statistics, so we are confident that readers are familiar enough with this technique without making any further distinction.

2.1 Similar to above, ownership and who operates a site, is not scientifically interesting. I suggest to move information on ownership, project name, and who hosted the experiment to the data availability or the acknowledgement sections. Adding extra acronyms make the text harder to read (for example, UVA and LLNL are mentioned in the title of Table 2, which is confusing unless UVA and LLNL have introduced well known standards for lidar settings). **AR:** We believe that the information mentioned by the Referee pertains to the main body of the manuscript (in addition to the ones already present in the acknowledgment section) since readers may be interested in reproducing experimental setups similar to ours,

and therefore can benefit from the clear description of which sites are investigated, which institutions are involved and which previous campaigns they can refer to. In Table 2, the column headers are edited to clarify that the reported information is related to scanning Lidars.

2.1 Are the tree heights and LAI really uniform at the three sites? To understand micro-scale variability over the area, the local topography, the local forest characteristics likely play a very strong role (see for example the work by Boudreault in 2014-2015 and Ivanell et al 2019). How does the distance to a forest edge vary in the clearings (is the lidar placed in the center or off to a side)? **AR:** The Lidars were placed at the edge of clearing areas (cf. with Fig. 2 of this document) both at MLBS and Treetops. Although historic measurements of the LAI are not available from the NEON site managers, the presence of gap areas (and heterogeneities in the forest coverage in general) has been extensively studied in the past (Shaw et al., 1983; Belcher et al., 2008; Cassiani et al., 2008) to show that clearing areas induce flow acceleration in the surrounding above-canopy space lasting for several times the canopy height in the streamwise direction. However, since the sites are located over rather complex topography, throughout the manuscript we specify that the presence of clearing areas is not the only cause for the observed variability of the wind resources. At lines 429 – 432, it is now reported: “[...], 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.”



Figure 1 - Aerial view (credit: Google Maps) of Treetops (a) and MLBS (b) sites and instrumentation. The wind rose is reported in panel (c) to assess the Lidars’ locations with respect to the canopy edge for different incoming wind directions.

2.1 Figure 1 b, based on the established fact that hills increase the wind speed, the wind speeds would generally be lower at the NEON and MLBS sites and the highest at the Treetops clearing, but the topographical effects are not discussed. This should be improved as it could

be an important factor for why small differences in the scanning lidars prevail at high altitudes. **AR:** The observed mean velocity is indeed larger at Treetops than the analogous observed at MLBS and NEON (cf. with Figure 9a and 9c in the manuscript), and one of the reasons behind this result could definitely be the local topography around each site. However, the present dataset does not have any spatially resolved observations in the streamwise nor spanwise directions that could link the presence of topographic features to local flow distortions. This point is now addressed in Section 4.1.1 (lines 446 – 453): “Another factor that could explain the difference in the capacity factors observed at the examined sites is the local topography. For the main wind direction (West-Northwest), the observational sites are located on the up-slope of a mountain, with Treetops located at higher altitude above sea level than MLBS and NEON, thus more subject to speed-up effects caused by the increasing terrain slope (see, e.g., Britter *et al.*, 1981). This could be a compelling motivation to explain the larger CF at Treetops and MLBS. Due to the lack of spatially resolved velocity measurements in the streamwise direction, however, this hypothesis cannot be verified by the present experimental setup.”

2.1 Figure 1e: The ZX lidar at the MLBS appears to be placed in close proximity of a large building. Since the authors aim to quantify and generalize the effects of clearings, and the surface drag, it seems important. I suggest that the authors check how the wind profile changes with wind direction and distance to nearest obstacle (either the forest edge or significant buildings) and avoid sectors with influence from very large local obstacles. **AR:** The building is situated on South with respect to Lidar (cf. with the arial view in Fig. 2b of this document) while the main wind direction at MLBS is West-Northwest (Fig. 2c), thereby placing the Lidar upwind the building. Furthermore, the building is nearly 10 m tall, and the lowest height scanned by the profiling Lidar at MLBS is 30 m above ground; given the short distance among the instrument and the building, the flow disturbance introduced by the building is deemed to not affect the measurements collected at 30 m and above.

2.1 Figures 2 and 3: The seasonal variability in Figure 2 is here linked to the seasonal variability of the trees. Hence, the authors seem to argue that low wind speeds are caused by the presence of leaves on the trees and high wind speeds are caused by the absence of leaves. But the later analysis clearly shows that the variability is predominantly linked to synoptic conditions, so the text is somewhat inconsistent. **AR:** The seasonal variability in the wind speed measured by the 3D sonic is indeed caused, among several factors, by the change in the canopy roughness induced by leaf senescence. However, as the Referee correctly points out, the synoptic forcing plays also a major role in determining the wind speed at the measured height. To clarify this point, at Lines 179 – 180 it is now added: “Although the seasonal wind speed cycle appears to be induced by the leaf senescence cycle, another strong seasonal cycle is followed by the synoptic wind at the ABL top.”

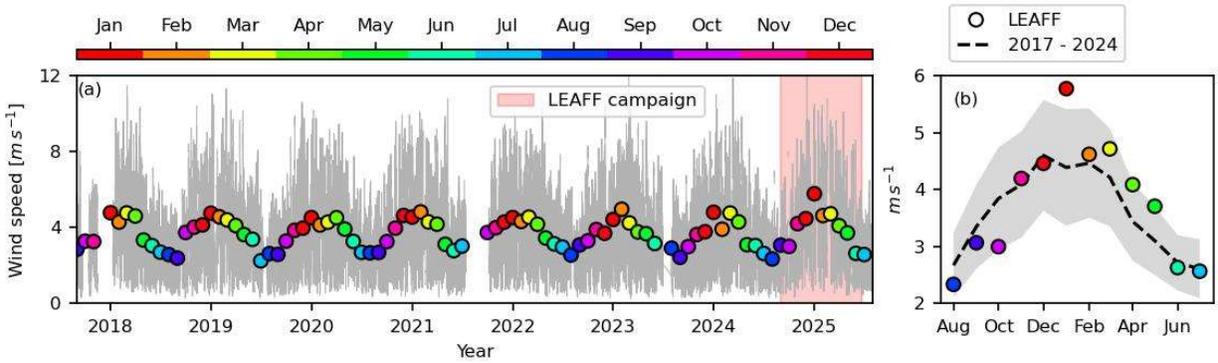


Figure 2 - Wind speed measured by 3D sonic anemometer. (a) Time series of 30-minute averaged values (gray line) and monthly averaged values (circles). (b) Comparison between monthly averaged velocity distribution in 2017 - 2024 and during the LEAFF campaign. The gray shaded area corresponds to the monthly standard deviation.

The authors do not present the average seasonal cycle and based on the presentation, it is not possible for a reader to see whether the wind year of 2024-2025 is average or how it differs from other years, which would be interesting to learn. **AR:** We compared the monthly-averaged wind speed during the experimental campaign (August 2024 through May 2025) with the analogous quantity averaged during 2017 – 2024, i.e. prior the LEAFF campaign. The result is reported in Fig. 3 of this document. The monthly-averaged wind speed during the LEAFF campaign (Fig. 3b) is within the statistical uncertainty around the velocity cycle averaged across previous years (except January 2025), thus confirming that the wind speed measured during the LEAFF campaign is made by statistically representative samples of the annual velocity cycle.

Section 2.2

Using Foken (2004) algorithm, please state whether the statistics were recalculated after the spikes are identified or whether the data was discarded after X many spikes were removed. **AR:** The statistics are calculated only after removing the spikes. We did not set a maximum threshold on the number of spikes detected for a selected time series.

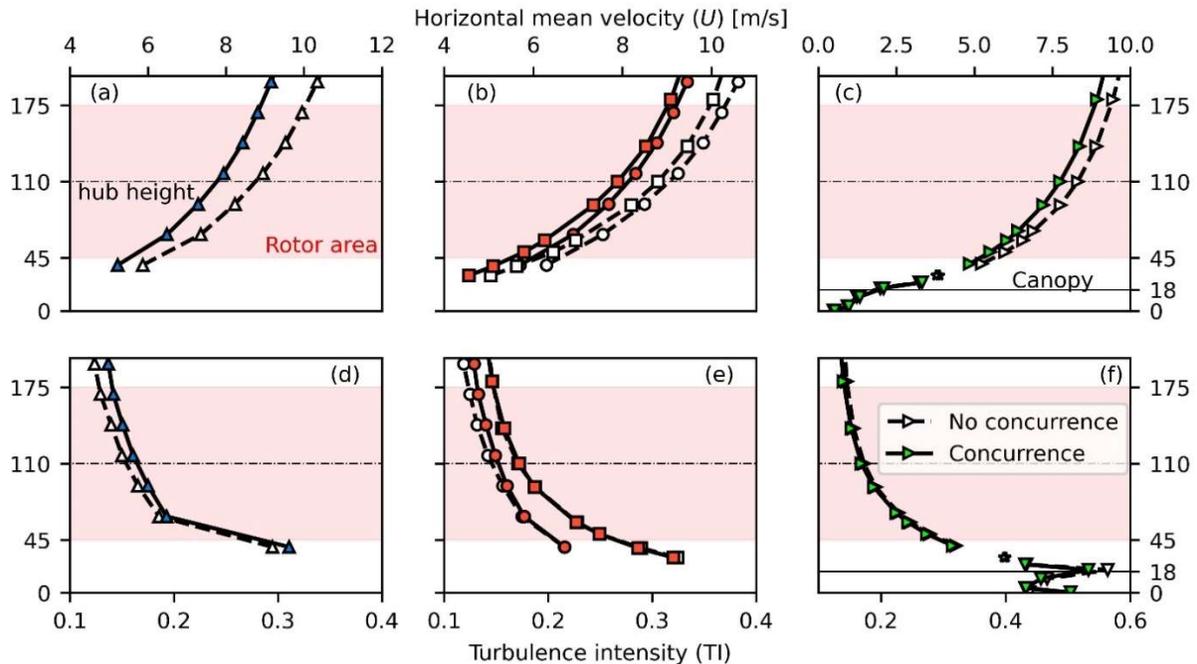


Figure 3 - Comparison of mean velocity (a, b, c) and TI (d, e, f) obtained with (empty symbols) and without (colored symbols) imposing data concurrence among tested sites. (a, d): Treetops; (b, e): MLBS; (c, f): NEON. The rotor layer is highlighted with red color.

In the last paragraph, the authors state that they only use time periods where data are concurrent and available at all heights below 600 m. Judging from Figure 4a, this would mean that 50% of all data are discarded. If this is the case, the authors should discuss what bias they introduce in their results, by omitting so much data. Or maybe the authors allow a height dependent availability for some of the analyses and based on the results, I wonder whether the requirement for concurrent data is also sometimes relaxed? **AR:** We acknowledge the importance of obtaining a high rate of data availability throughout the investigated volume to minimize statistical uncertainty. The results presented in Section 4 (focusing on the site intercomparison wind resources and site-to-site heterogeneity) are inferred when statistics are simultaneously available from all considered instruments (profiling Lidars, scanning Lidars, 2D and 3D anemometers) since these results are based on site-to-site comparisons, thus it is necessary to have concurrent data availability from all sites. This is now clarified at lines 224-225: “In the remainder of this study, unless otherwise indicated, a further down-selection is made to consider only time periods when statistics are available from all the instruments for all the probed heights below 600 m above ground.”

As pointed out by the Referee, the statistics calculated based on the entire available dataset may differ from those calculated only in presence of concurrent data from the remaining instruments. The vertical profiles of horizontal mean velocity and TI calculated with and without concurrence are reported in Fig. 4 of this document. The mean velocity profiles

quantified within the rotor layer are larger when no data concurrence is imposed, with the largest quantitative differences between concurrent and non-concurrent profiles found for the Treetops site. This is motivated by the fact that the data rejection rate is highest at Treetops, thus the difference between concurrent and non-concurrent mean profiles is larger. By contrast, the difference is lower for NEON (Fig. 4c and 4f) where the data rejection rate is smaller. Finally, minor quantitative differences are found in the TI profiles (Fig. 4d, 4e and 4f), thus the constraint of concurrence has only a minor impact on this statistic.

Section 3.1. Two rotations are used to calculate the mean wind speed, i.e., the wind speed is calculated along a local 30-minute streamline. But wind turbines cannot typically harvest the vertical component of the flow, and it is therefore more relevant to use wind speeds based on one rotation only. **AR:** The 30-minute averaged wind speed reported throughout the manuscript refers to the horizontal component only, thus the vertical velocity is not taken into account to calculate the first-order statistics. Conversely, to calculate σ_u and $\overline{u'w'}$, it is necessary to rotate the Lidar velocity vector into streamwise, spanwise and vertical components to be consistent with their analogous evaluated from 3D sonic anemometry.

Section 3.

Overall, I really like that the authors here group the data according to operating regions, although this approach is not followed through in the paper. **AR:** The distinction between data points in region II and region III is now maintained throughout the Results section.

Table 4. Please define the wind speed intervals in the title, corresponding to Regions I, II and III. **AR:** This information is added to Table 4.

Table 4 and Figure 5. Are the data presented really based on concurrent availability? To me, it looks like the jumps in data (Figure 5) would indicate different availability, and that the jumps rather correspond to different gaps in data availability. How was data screened for this figure? **AR:** When the results involve intercomparisons among different sites (as in Table 4 and Fig. 5), they are inferred from concurrently available data availability from all instruments (Lidars and in-situ anemometers), as detailed in Section 2.2. The jumps observed in Fig. 5 are due to the data availability resulting from the quality control process and the data concurrence constraint. To reduce the statistical uncertainty of the pdf in Figure 5, we have removed the concurrent availability of C_d (previously present) from the intercomparison of wind resources among sites, since the drag coefficient is not involved in this part of the analysis. The updated pdf is reported in Fig. 5 of this document and utilized to replace Fig. 5 in the manuscript. The values reported in Table 4 and Table 5 of the manuscript are updated accordingly.

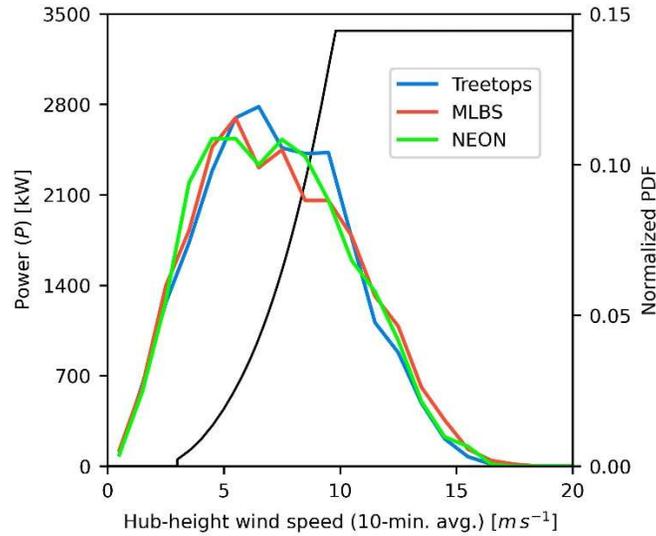


Figure 4 - Normalized probability density function of hub-height wind speed at the tested sites (right axis) and power curve of 3.4 MW IEA wind turbine power curve (left axis).

Section 3.3. The authors are quite defensive about using the power law rather than the logarithmic profile and list a number of arguments that are all problematic. The authors claim to introduce roughness in the analysis by including a displacement height, but a displacement height just represents a displaced surface caused by dense canopies. The roughness effect is also present in the shear, in the lowermost part of the profile. Since the roughness is usually quantified by a roughness length, as defined in the log-profile, the section turns confusing. In a clearing, the forest height is zero, and hence the displacement height (if it related to the physical properties of trees) should also be zero. The two parameters are just fitting parameters that are affected by atmospheric stability, distance to forest edges, topography, local large buildings etc. **AR:** The displacement height represents the height of mean momentum absorption, and it has been proven to correlate with the aerodynamic roughness (Thom, 1971; Jackson 1981, Raupach *et al.*, 1991, Nakai *et al.*, 2008) if the logarithmic law is chosen to model the mean velocity profile. Although its quantification on complex topographies is more challenging than on flat terrain, the displacement height still maintains its physical meaning; thus, it can be utilized to quantify the momentum absorption from the mean velocity. To limit the effect of roughness on the shear exponent, the mean velocity profiles are fitted only within heights greater or equal to 30 m above ground (corresponding to 1.7 times the canopy height) up to 175 m (corresponding to the turbine's top tip). Focusing on the displacement height quantified within clearing areas, the Lidars were installed by the clearing's edges (cf. with Fig. 2 in this document), both at Treetops (Fig. 2a) and at MLBS (Fig. 2b). Assuming West-Northwest as main wind direction (Fig. 2c), the mean velocity profiles still reflect the presence of a wide

forested area in the vicinity of the observational site. Thus, the displacement height can still be greater than zero.

Further, it is incorrect that the wind profile can be assumed to be logarithmic in a height interval of a few canopy heights from the surface. This was an important finding made in the 1990ies, and the term “roughness sublayer” was introduced. **AR:** We corrected our previous statement in the manuscript specifying that the mean velocity in the above-canopy space shows a logarithmic distribution from $z \approx 2h$, i.e. from twice the canopy top, up to $z \approx 5h - 6h$ according to several results from numerical studies (Finnigan et al., 2009), wind tunnel (Raupach et al., 1996, Segalini et al., 2013) and field experiments (Garratt, 1978; Shaw et al., 1983; Amiro, 1990). The text is revised as (lines 313 – 318): “However, the best-fit of the logarithmic law parameters (friction velocity, displacement height and aerodynamic roughness) strongly depends on the vertical interval of validity of the logarithmic law, which are typically assumed between $z = 2h$ to $z \approx 5h - 6h$ (i.e. 36 m to 90 m – 108 m) based on numerical studies (Finnigan et al., 2009), wind tunnel (Raupach et al., 1996; Segalini et al., 2013) and field experiments (Garratt, 1978; Shaw et al., 1983; Amiro, 1990; Arnqvist et al., 2015).”

Despite this criticism, I find it unproblematic that the authors choose to use this way of fitting the data, but their analysis should acknowledge the very wide variety of factors that influence the resulting parameters. **AR:** We acknowledge the presence of many complexities listed by the Referee that increase the uncertainty of the current model parameters. The results obtained from the mean velocity fitting are now discussed distinguishing between different atmospheric stability classes (convective, neutral and stable). More details are reported in the remainder of this document.

Section 3.4

Please refer to the literature on estimation of uw from lidar profilers and discuss what the random scatter in the data will mean for the 30-minute C_d estimation! Given that fluxes can be considered constant in the roughness sublayer, I question why they at all extrapolate down to canopy height, where a local tree crown may influence the results strongly. Also, low wind speed data should be removed from the analysis to avoid a strong influence from very low wind speed situations, in which the drag coefficient would be calculated from a ratio of near-zero numbers. **AR:** The quantification of $\overline{u'w'}$ based on profiling Lidar data is now removed and replaced with the estimates based on the friction velocity calculated by 3D sonic anemometer data at 30 m above ground ($u_*^2 = \left(\overline{u'w'^2} + \overline{v'w'^2} \right)^{0.5}$) in order to remove any uncertainty introduced by the linear extrapolation; region I, low speed data points are removed as well. As pointed out by the Referee, the shear stress is assumed to be constant

in the roughness sublayer, thus the value calculated at 30 m (corresponding to 1.7 times the canopy height) is approximately equal to the value at the canopy height. Figure 6 and the related discussion are removed from the manuscript.

Section 4.

State what value N has and what the lidar availability for the hub height availability was. For the CF values in Table 5, which data were used, and how does a lack of 100% availability influence the results? Again, I suspect that the results reflect a lack of concurrent lidar data availability rather than micro-scale effects. **AR:** At lines 389–390, it is now added: “The latter [the number of samples used to obtain the statistics] is 3212 for 10-minute datasets and 1138 for 30-minute periods.” When the results involve site intercomparisons, the data are always concurrent among all instrumented sites. In Table 1 of this document, we have reported the displacement height, shear exponent, reference velocity and capacity factors evaluated with and without data concurrence. The analysis of displacement height and shear exponent does not differ significantly from the one currently in the manuscript, i.e., NEON experiences the largest displacement height and shear due to the higher canopy density, while Treetops and MLBS feature similar (lower) displacement heights. By contrast, higher CF values are found when all the data are considered. Although this represents a bias in the provided CF values, it is important to base the analysis on concurrently available data from all the sites for a meaningful comparison of the spatial distribution of wind resources.

Table 1 – Sensitivity of the wind resources at the tested sites on the constraint of data concurrence. The statistics obtained without data concurrence are reported in parenthesis.

Site	Displacement height [m]	Shear exponent	Reference velocity [m/s]	Capacity factor (CF)
Treetops	11.3 (10.9)	0.28 (0.27)	$U_{hub} = 7.44$ (8.76) $U_{eq} = 7.45$ (8.36)	49.67 (61.75) 49.78 (58.15)
MLBS	10.9 (10.8)	0.26 (0.23)	$U_{hub} = 7.79$ (9.04) $U_{eq} = 7.78$ (8.50)	52.65 (62.82) 52.49 (58.26)
NEON	13.6 (13.6)	0.32 (0.29)	$U_{hub} = 7.42$ (8.31) $U_{eq} = 7.30$ (7.94)	48.76 (54.64) 47.55 (51.22)

Figure 9 is really nice and makes full use of the range of the lidars. It would be interesting to see how this result differs for the different regions of wind turbine operation. **AR:** The distinction between diurnal cycles of wind speed, wind direction and TI are reported in Fig. 6 of this document for the MLBS site; analogous diurnal cycles are obtained for the remaining two sites. As expected, the mean wind speed is larger throughout the entire ABL when the hub-height value is selected in region III (cf. Fig. 6a and Fig. 6d). It is noteworthy that, during

early daytime conditions (06:00 – 12:00 local time) data in region II shows an average wind direction of 300 deg (Fig. 6b) as opposed to ~270 deg for the rest of the day and in region III (Fig. 6e). Finally, the daytime TI values are larger for region II conditions due to the normalization with the mean velocity (lower in region II). Although there is a clear distinction in the diurnal average patterns, we deem this information not to be essential for the manuscript, and we prefer to omit this figure to avoid breaking the main discussion.

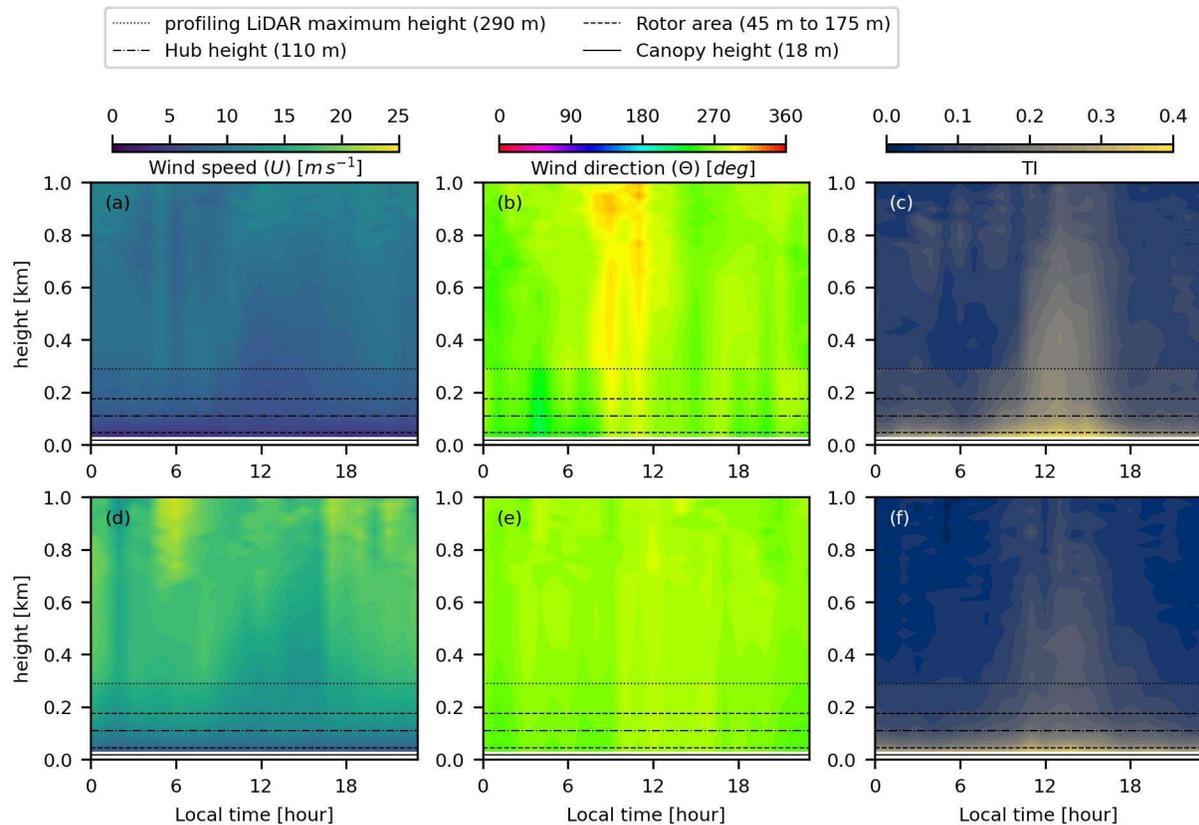


Figure 5 - Diurnal cycles of horizontal wind speed (a, d), wind direction (b, e) and TI (c, f) obtained when the reference wind turbine operates in region II ($3 \text{ m/s} < U < 9.8 \text{ m/s}$) (panels a, b, c) or in region III ($U > 9.8 \text{ m/s}$) (panels d, e, f).

For figure 11, I suggest that the authors consider group profiles (exclude problematic directions and include effects of atmospheric stratification) to get more precision in the estimates. Figures 12-14 are problematic for reasons stated above. **AR:** To justify the large variability of the mean velocity parameters, we repeated the statistical analysis distinguishing between different atmospheric stability classes for mean velocity profiles measured at NEON and calibrating the mean velocity model within 30 – 175 m (i.e. 1.7 to 9.7 times the canopy height). 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. 7 of this document and Fig. 12 of the revised manuscript. For all stability conditions, the

displacement height (Fig. 7a) 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. 7b). 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. Former figures 12 – 14 are now removed from the analysis.

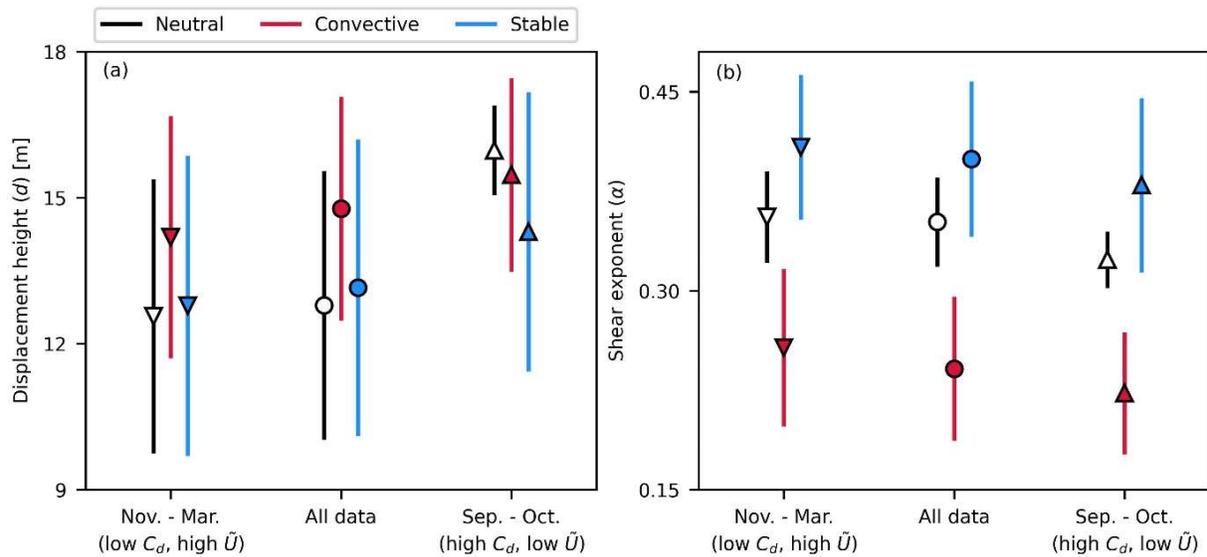


Figure 6 - Statistics of mean velocity parameters (d , α) calculated for different stability conditions distinguishing between months of high C_d /low \tilde{U} (September-October) versus months of low C_d /high \tilde{U} (November-March). (a) Displacement height, d . (b) Shear exponent, α . Symbols refer to mean values, while the standard deviation is reported by the uncertainty intervals.

For figure 10, please avoid using low wind speed data (maybe focus on region 2 data only), to make sure that the estimated C_d is robust. **AR:** Figure 10 is updated in the manuscript utilizing the C_d values obtained from 3D sonic anemometry. Following the Referee's suggestion, data in region I are discarded.

The results in the last figure and the summary in Lines 420-425, which are also reflected in the abstract, are counterintuitive. Counterintuitive results can still be correct, but in order to make sure, I suggest that the authors (1) consider to only use region 2 data, to avoid that the

low mean wind speeds during the early autumn dominate the results and (2) only use concurrently available data (unclear whether this is done) and (3) make sure to avoid wind directions, where, for example, a large building in one of the clearings may influence the result. **AR:** The last Sub-section of the Results is rewritten based on another Referee's feedback. The analysis of site heterogeneity is now conducted via Kolmogorov-Smirnov p -test distinguishing between p -values in region II and region III both for mean velocity and σ_u , as well as data collected during different seasonal periods and different atmospheric stability regimes. The revised Sub-section is reported at lines 541 – 621. The requirement of concurrent data availability was already present in the previous version of the manuscript. Finally, for the main wind direction in this study (West-Northwest), no major obstacle is found around any site for the heights scanned by the Lidars.

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