# ACP-2021-598 - Authors reply to the comments of the reviewers

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## General answer by the authors

We would like to thank both reviewers for their comments and suggestions to correct and improve this manuscript. We have tried to answer thoroughly to all their comments. Please find below the comments of both reviewers in black fonts and our corresponding answers in blue fonts.

## Reviewer 1

### Major comments

1. L113. Was this a period of relatively statistically-stationary flow at the tree location? I ask because non-stationarity (e.g. strong accelerations and decelerations of the wind field) at a time scale comparable to the ones induced by the tree wake could lead to departures from the usual atmospheric turbulence. It would be useful to analyze the flow variability at scales comparable to the height of the tree over the local friction velocity too ensure that the considered periods are indeed stationary flow periods.
   The experiment lasted between 14:30 and 16:20 (Figure A1 in the original manuscript). The local mean friction velocity during that period, estimated using the sonic measurements at the height of 4 m at the upwind meteorological mast was approximately equal to 0.4 m/s during this measurement campaign, see Angelou et al. (2021). The tree height over the friction velocity corresponds to a time scale $\tau_H$, which is equal to 6.5 m / (0.4 m/s) = 16.25 seconds. In Figure 1, we show the time series of the amplitude of the wind vector at 4 m, using mean values over $\tau_H$. There was no significant wind speed trend over the 1 hour and 50 minutes of the experiment. Using a linear fit, we find a slope of less than 0.001 m/s per $\tau_H$, and, therefore, we assume that the investigated time series can be treated as stationary.

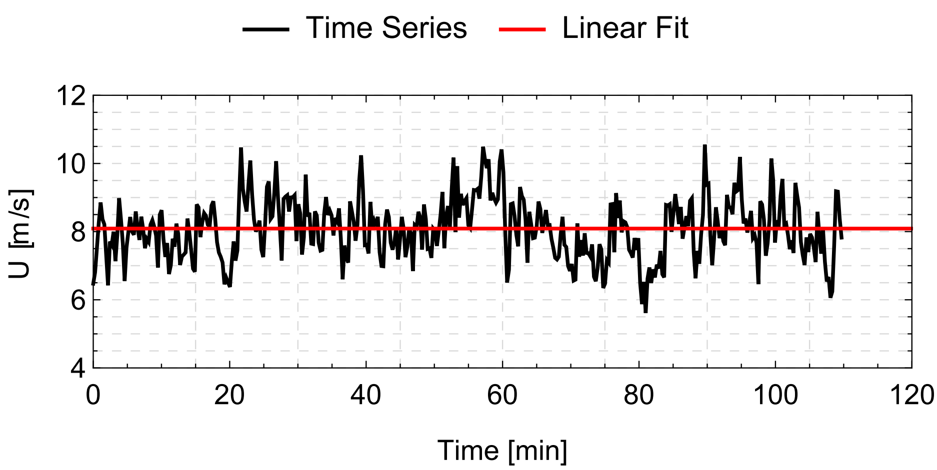


Figure 1: Time series (black) of the mean wind speed at 4 m, at a time scale $\tau_H$ equal to the tree height over the friction velocity. The red line depicts the result of the linear fit.

The following sentence will be added after the line 114 of the original version: *In order to assess the stationarity of the free flow, we define the time scale* $\tau_H = u_\star/H$, *where* $u_\star = (\langle u'w'\rangle^2 + \langle v'w'\rangle^2)^{1/4}$ *was calculated from the 4 m sonic anemometer at the* $M_1$ *mast and* $H$ *is the tree height. Using the mean of the friction velocity (0.4* $ms^{-1}$*),* $\tau_H$ *= 16.25 s. Over this time scale, a linear fit of the wind speed showed a slope of less than 0.001* $ms^{-1}$ *per* $\tau_H$*, and the time series of the free flow is, therefore, considered to be stationary.*

2. L131. Can you provide more details as to what the "corresponding distribution" refers to? Is this from 26' sampling at different locations? The processing procedure should be better described to enable an assessment of its impact on results as well as to enable others to reproduce these results in the future.

   The "corresponding distribution" refers to the distribution of the vertical component $w$ of the wind vector in each grid cell. We initially tried to apply a filter on all the velocity components, but the only significant effect was found when applying it to $w$, which is due to geometry of the wind scanner setup. Each wind lidar measures the projection of the wind vector to the direction of the laser beam ("line-of-sight"). For the lower part of the measurement plane, the elevation angle of the laser beam direction gets close to 0 degree. This situation would correspond to trying to measure the vertical velocity component with a sonic anemometer, which only had near-horizontal transducer pairs. Any slight misalignment of the transducer pairs or noise in the line-of-sight wind speed measurements would result in a large error in the estimation of the vertical component.

   The sentence of the original manuscript:
   *First, a filtering was applied in each 130 grid cell by treating as outliers those wind vector measurements with a vertical component* $w$ *outside the inner and outer lower fence of the corresponding distribution.*

   Is now re-written to:

   *First, a filter was applied on the wind lidar data. The filtering was based on the calculation of the inner and outer fence of the distribution of* $w$ *in each grid cell. Those wind vector estimations with a vertical component outside the two fences in each grid cell were treated as outliers and were not included in the analysis.*

3. L145. This comparison is really interesting. I encourage the authors to provide a quantitative comparison (percentage values) for the second order moments as well. Since I can imagine that variations can be as high as 200% at certain locations, perhaps one can show the values and mention that these are within the observed uncertainty? This, again, would be very valuable information in my opinion, especially when considering the scope of the study. What's the impact of these "errors" (assuming the sonics are correct) on the eddy viscosity and mixing length quantities?

   We agree that it is important to provide an assessment on the accuracy of the estimated 2$^{\text{nd}}$ order moments and also agree that this was not presented thoroughly in the original version of the article. The estimated values of the 2$^{\text{nd}}$ order moments, using the wind lidars and the sonic anemometers is presented in the Figure 3. The relative error in the longitudinal variance varies between -6% and -58%. The maximum relative error is found in the sonic anemometer at the center of the wake (sonic anemometer at 4 m, south boom at $M_2$), where the turbulence is very low. The systematic underestimation of the longitudinal variance by the lidars can be explained by their larger probe volumes compared to the path lengths of the sonic anemometers. Hence, a part of the high-frequency variance is filtered out in the lidar data. The $\langle u'w'\rangle^2$ and $\langle v'w'\rangle$ fluxes are less affected by the lidar probe volumes, since the high-frequency co-spectrum drops faster than the power spectrum in the inertial subrange. For the momentum fluxes, we still find relative error values that are up to 128%, excluding the $\langle u'w'\rangle$ observations at the lowest heights (1.5 m), which due to the geometry of the experimental setup (despite the filtering described above) are found to be very sensitive to random noise. Furthermore, we have not included the vertical momentum flux measurement at 4 m at the north boom of the $M_2$ mast, where $\langle u'w'\rangle$ was found to be equal to 0 $m^2s^{-2}$, leading to an unspecified relative error. These errors are a lot smaller in the case when the momentum fluxes have high values as it is the case in the wake of the tree. In those locations, which are the most important for our study, we find that all errors are smaller than 20%. For the fluxes, the disagreement between the sonic anemometers and the lidars, can be explained by the random error caused by the disjunct sampling of the velocity field (see answer 2 to reviewer 2, below). A longer time series would reduce the random error of all the second order moments. Concerning how the disagreement between the two measurements affect the further results, we refer to Figure 6, where we see a good agreement between the estimated length scales by the sonic anemometers and the lidars. For this calculation, it is the error on the length of the stress vector that matters $\{\langle u'v'\rangle, (\langle u'w'\rangle\}$. For the high-gradient region, this error is typically significantly less than 20 % which explains the good agreement between the estimated length scale values using the wind lidar and sonic anemometer measurements. The following sentences have been added in:

Section 4.1: *The relative error in the longitudinal variance varies between -6% and -58%. The maximum relative error is found in the sonic anemometer at the center of the wake (sonic anemometer at 4 m, south boom at $M_2$), where the turbulence is very low. As far as it concerns the momentum fluxes, we find overall relative error values that are up to 128% above the tree, but the error in the wake is limited to 20%. This does not include the vertical momentum fluxes at the lowest heights (1.5 m), which due to the geometry of the experimental setup, are found to be very sensitive to random noise. Furthermore, we have not included the vertical momentum flux measurement at 4 m at the north boom of the $M_2$ mast, since the correlation between the longitudinal and vertical component was found to be equal to 0 $m^2s^{-2}$, leading to an unspecified relative error.*

Discussion: *The relative difference between momentum fluxes estimated using the wind lidar and sonic anemometer measurements in the wake area, where high variance is observed, was smaller than 20%, and this can explain the good agreement between the corresponding length scale estimations which are presented in Figure 6. A larger data sample would help to reduce the random error variance on both the estimations of the second order moments and of the corresponding momentum fluxes.*

4. L174. Assuming that the along wind gradients are much smaller than the transverse wind gradients is a rather strong assumption in this "complex geometry" flow. Can the authors support this assumption anyhow?

   Dellwik et al. (2019) studied the wake generated by the same tree in our study using numerical simulations, that were extensively and successfully validated by sonic anemometer observations. According to that study, (please see Figure 10 in (Dellwik et al., 2019), the along-wind gradients are much smaller than the gradients across the wake at the downwind distance of $1.3\times H$. In addition, we have estimated the along wind gradients ( $\frac{\partial U}{\partial x}, \frac{\partial V}{\partial x}$ and $\frac{\partial W}{\partial x}$ ) in the vicinity of the 10 sonic anemometers on the $M_2$ mast, since the scanning plane by the wind lidars was located approximately 1.3 m upwind of the from the sonic anemometers. The values that we find are one order of magnitude lower than the values of transverse gradient. Therefore, we think that the results of our study are not biased by disregarding the along wind gradient. We elaborate more on this topic in our answer to the 7$^{\text{th}}$ comment of Reviewer 2.

5. Eq2. I recommend using standard index or vector notation as this expression is a bit confusing. Plus it seems to me that the eddy viscosity cannot be a scalar in this case but should rather be a first order tensor, otherwise this expression implies that u0v0/(du/dy) = u0w0/(du/dz) (if I understood the expression correctly). I invite the authors to clarify this point.

   After the recommendation of both reviewers we have changed the formulation of Equation 2. Please see our reply to the Comment 7 of the second reviewer for a detailed answer, along with the corresponding changes that we did in the revised version of the document.

6. L188. I am not sure what point the authors are trying to make here. The eddy viscosity is mathemati-

cally defined as a ratio of fluxes and mean wind gradients, and as such, it can indeed be used to describe the overall momentum flux within a plant canopy. Whether it makes physical sense though, that is another point. For example, in the presence of counter gradient fluxes, its value would be negative, which is unphysical and would lead to e.g. a blow up of simulations. Similarly, if the main flux is from large scale coherent structures, then the concept of eddy viscosity is not the right one, even if its value is positive. With their analysis, the authors have just shown that K can be mapped to fluxes, but this is just a result of their mathematical definition. Further, it is not clear to me what percentage of the total momentum flux is really caused by the considered Reynolds stresses - can the authors quantify it? I bet that dispersive flux contributions might be significantly larger, i.e. $(uw \cdot u_0 w_0)$, cause this flow is not statistically homogeneous and there is strong subsidence and flow three-dimensionality in the wake region. This also justifies why the authors have found a rather small mixing length in their studies. By the way, the authors can probably compute a good estimate of the total drag that the tree is exerting on the flow directly from the velocity map in Fig. 4(a). This would help determine the overall contributions of u0w0 to the total drag.

The point of the presented analysis is to show that it makes sense to use the eddy-viscosity formulation to predict fluxes from the gradients in the wake of the tree. The results show that in the region around the periphery of the wake, in which momentum transfer processes between the free and the wake flow take place, the vector of the fluxes is anti-parallel to the mean transverse gradient. We study the flux-gradient relation in many individual sub-areas that each have a size of 0.5 m × 0.5 m. These distributed observations are treated as point observations and the spatial variability within each area is assumed to be negligible. This assumption is justified by the good agreement with the sonic anemometer observations in the wake of the tree. Dispersive fluxes occur where a spatial average is taken over an area with strong spatial variability. Indeed, if the wake of the tree could only be represented by a single grid point in a numerical model, the dispersive component of the flux would be highly significant. Concerning the last comment about the total drag of the tree, we refer another article of ours (Angelou et al., 2021), which was published recently. In that study, we estimated the drag force on the tree from the momentum deficit in the wake. We also quantified that the contribution of the momentum fluxes is less than 10% of the overall drag induced by the tree to the flow.

## Minor comments

1. L12. Perhaps better to say “extracting”?
   Corrected
2. L13. Cause → Can cause
   Corrected
3. L16. The increase in turbulence is not only because of increased wind gradients, but also via wake generation and via the adverse pressure gradient that they generate.
   We have changed the word ”leads” with the one ”contributes”, therefore the revised sentence is: This, in turn, contributes to generation of turbulence ..
4. L38. Critical extension → Since the authors are not modifying the measurement instrument/methodology, perhaps it is better to say “a new application”?
   Corrected

# 1 Reviewer 2

## General Comment

In this manuscript the authors perform turbulence measurements in the field, in the wake of an isolated large tree. This is a quite complete work, done with relatively heavy measurement devices: a multi-lidar and several sonic anemometers placed on two meteorological masts. This results in an important database which is statistically analyzed in this manuscript. This is an important work, the observation efforts needed to record this data base is very appreciable. It will be a useful database for the community. I have not seen a data availability statement: it would be useful to provide this information. Below I have several suggestions and comments.

Answer: Thank you for the comments and the suggestions. The data are going to be accessible upon request. A statement will be added in the revised manuscript.

## Specific comments

1. I did not understand figures 2a-b, concerning the orange dots. I understand that there are two meteorological masts, one of them (M2) being on the red dot. Why is the orange dot in a different location? The $M_2$ mast was instrumented with in total 10 sonic anemometers, two at each height. The orange dots correspond to the sonic anemometer locations on the Northerly boom on the $M_2$ mast, whereas the red dots corresponds to the locations on the Southern boom. We re-write the lines 103–105 of the first version of the paper as:

   *The sonic anemometers were installed on booms pointing towards the direction of 200° relative to North on both masts. To get a higher coverage of the complex wind field in the tree wake, the $M_2$ mast was instrumented with five additional sonic anemometers, on opposing booms pointing towards 20° relative to North.* In order to clarify this point, we have also re-formulated the text in the caption of Figure 2:

   *Drawings of the top (left) and the front (right) views of the experimental setup used in this study, where the locations of the tree, the short-range WindScanners ($WS_1$, $WS_2$ and $WS_3$), the scanning pattern (blue) and the locations of the sonic anemometers on the up- ($M_1$) and down-wind ($M_2$) meteorological masts, are depicted. The locations of the 10 sonic anemometers on the opposing booms on the $M_2$ mast are indicated with red and orange color, respectively.*

2. For each grid, it is indicated that there is approximately 25 iterations per 10 minutes period. Since the whole data set is recorded during a 3 hours periods, I understand that there are 18x25 = 450 values at each grid point, to perform the statistics. Is this correct? If correct it does not seem to be a large sample size to perform statistics. This point should be clarified in the manuscript and discussed in the discussion section.

   We agree with the reviewer that ideally more measurements would be required to estimate the wind statistics with a higher accuracy. From the 3 hours period examined in this study only 101 scanning pattern iterations are selected finally in the analysis due to the wind direction requirement that was chosen. However, despite the disjunct sampling performed here, we argue that the resulting statistics are still sufficiently accurate.

   In order to assess theoretically the accuracy of the calculated $2^{\text{nd}}$ order moment we refer to the work of Lenschow et al. (1994), where it is shown that for the estimation of $2^{\text{nd}}$ order moments and fluxes, it is important to use a sensor that can acquire observations instantly and not averaging over time. Then the systematic and random errors in the estimated fluxes is going to be dependent on the number of measurements ($N$) and the time between consecutive observations ($\Delta$). When $\Delta$ is a lot larger than the time integral scale of the measured parameter then the systematic relative to the ideal flux (denoted here as $F_i$) can be estimated using equation 56 in (Lenschow et al., 1994):

$$\frac{F_i - \langle u_1' u_i' \rangle_s}{F_i} = \frac{\Delta}{T} \quad \text{with} \quad i = 2, 3 \tag{1}$$

and equation 59 in (Lenschow et al., 1994) for the random error:

$$\sigma_F^2 = \mu_f \frac{\Delta}{T}. \tag{2}$$

In the two equations above, $T$ is the length of the period examined, $\mu_f$ is the variance of the flux time series (see Lenschow et al. (1994) equation (45) and equations thereafter) when $T$ is approximating infinity and $\langle u_1 u_i' \rangle_s$ denotes the mean of the measured momentum flux over the period $T$ and with a sample separation $\Delta$.

Using the time series of the sonic anemometer in the M2 meteorological mast, we estimated the time integral scale of the momentum fluxes, by integrating the autocorrelation function until the first time lag for which the autocorrelation function dropped to zero. The results are presented in Figure 2. In this figure we can see that the time integral scales of the momentum fluxes in the wake of the tree (heights 4 and 6 m) are between 2.22 – 4.53 seconds for the $\langle u'v' \rangle$ and between 0.17 – 0.32 seconds for the $\langle u'w' \rangle$.

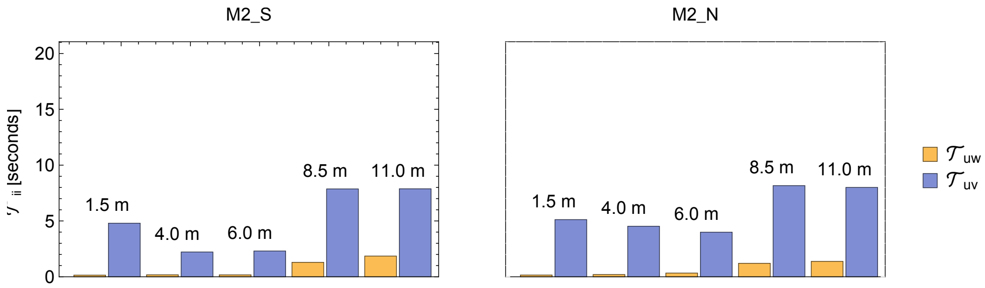


Figure 2: Time integral scale of $\langle u'v' \rangle$, $\langle u'w' \rangle$ estimated using the time series of the measurements acquired in each of the 10 sonic anemometers of the M2 meteorological mast.

Since, the wind lidars are measuring very fast (approximately 200 Hz) and the time of the disjunct sampling is lot larger than the time integral scales, then we can use the Equations 1 and 2 to derive an estimation on the theoretical systematic and random error in the measured second order moments. We find that despite the limited amount of data the systematic error of the measured momentum fluxes is about 1% (26 seconds over 2626 (26x101) seconds). The random error is between 6% and 15% for the case of the vertical and transverse momentum flux, respectively.

3. I don't understand the normalisation indicated in line 131: "the wind speed measurements of each iteration were normalized by the corresponding 26-second mean wind speed". The normalisation should be the same for all data. If normalization depends on the samples, one needs to understand what are the statistics of these normalization values. This should be clarified.
The data acquired during one iteration of the scanning pattern (26 seconds) were normalized by the mean wind speed (over 26 seconds), using the measurements of the sonic anemometer at the $M_1$ mast. This way a time-dependent scaling was applied to the time series in order to have a constant mean value of the upwind conditions at 4 m, over a time scale equivalent to the scanning duration of the wind lidars.

4. The stresses $< u'w' >$ and $< v'w' >$ are estimated, as well as normal stresses. Since instantaneous values are taken every 25 seconds, it is not the real stress which is estimated (the real stress would need

to resolve turbulent scales, of the order of seconds or lower). The estimates $< u'w' >$ and $< v'w' >$ are done using coarse-gained estimates of u', v', w', as their fluctuations are likely to be much smaller than real values of stresses. This should be mentioned in the text and also discussed in the discussion section.

As we discuss in our answer to the $2^{nd}$ comment of the reviewer the accurate estimation of the second order moments is not solely depending on the time of the disjunct time sampling. We agree that ideally we should have more measurements in order to reduce the random error in our estimated values. We will add the following text to *Discussion* section.

*From the 3 hours period examined in this study only 101 scanning pattern iterations are selected finally in the analysis due to the wind direction requirement that was chosen. The period of sampling in each grid cell (26 seconds) is a lot larger than the time integral scale of the momentum fluxes in the wake of the tree. This enables the theoretical estimation of the systematic and random error in the calculation of the momentum fluxes based on the the mathematical formulation presented in Lenschow et al. (1994). We find that the largest contribution to be expected in due to random errors which are estimated to be approximately equal to 15%. The relative difference between momentum fluxes estimated using the wind lidar and sonic anemometer measurements in the wake area, where high variance is observed, was smaller than 20%, and this can explain the good agreement between the corresponding length scale estimations which are presented in Figure 6. A larger data sample would help to reduce the random error variance on both the estimations of the second order moments and of the corresponding momentum fluxes.*

5. The gradients along $y$ and $z$ of the mean velocity are estimated (fig 4b,c). How this gradient is estimated numerically should be indicated in the text (central difference, some smoothing is done with a kernel?). The gradients are estimated numerically by:

   (a) first estimating numerically the forward difference between neighbouring grid cells along the $y$ and $z$ axis using:

$$\frac{\partial \langle \hat{u} \rangle}{\partial \hat{y}} = \frac{\langle \hat{u} \rangle [i+i]\rangle - \langle \hat{u} \rangle [i]}{\hat{y}[i+1] - \hat{y}[i]} \text{ and } \frac{\partial \hat{u}}{\partial \hat{z}} = \frac{\langle \hat{u} \rangle [i+i] - \langle \hat{u} \rangle [i]}{\hat{z}[i+1] - \hat{z}[i]} \tag{3}$$

   (b) and subsequently by applying a linear interpolation and finding the corresponding value of the gradient in the $y$, $z$ coordinates of the grid cell

   The following sentence has been added in the manuscript in the Section 4.2: *The gradients were estimated by first calculating the forward difference of the mean longitudinal wind speed between neighbouring grid cells in the $y$ and $z$ direction, and subsequently by finding the corresponding value in the coordinates of each cell using linear interpolation.*

6. Figure 4c plots one normal stress. It could be interesting to plot the 2D kinetic energy and also to estimate the turbulence intensity, by dividing fluctuations by the mean velocity, to obtain a percentage.

   During the data analysis performed in this study we estimated both the turbulence intensity (TI) and the turbulence kinetic energy (TKE), but we decided not to include them in the manuscript. Figure 3 presents both plots, where it can be seen the following: a. there is a clear increase in the TI along the wake and this increase is spatially inhomogeneous, which is in accordance with the inhomogeneous porosity of the crown of the tree. b. The TKE, which here is equal to $1/2(< u'u' > + < v'v' > + < w'w' >)$, is higher along the periphery of the crown of the tree. At the lower measuring heights ($\hat{z} < 0.5$) the estimated values of TKE are affected with noise, which originates from the difficulties of estimating the vertical component of the wind close to the ground (see Figure 4).

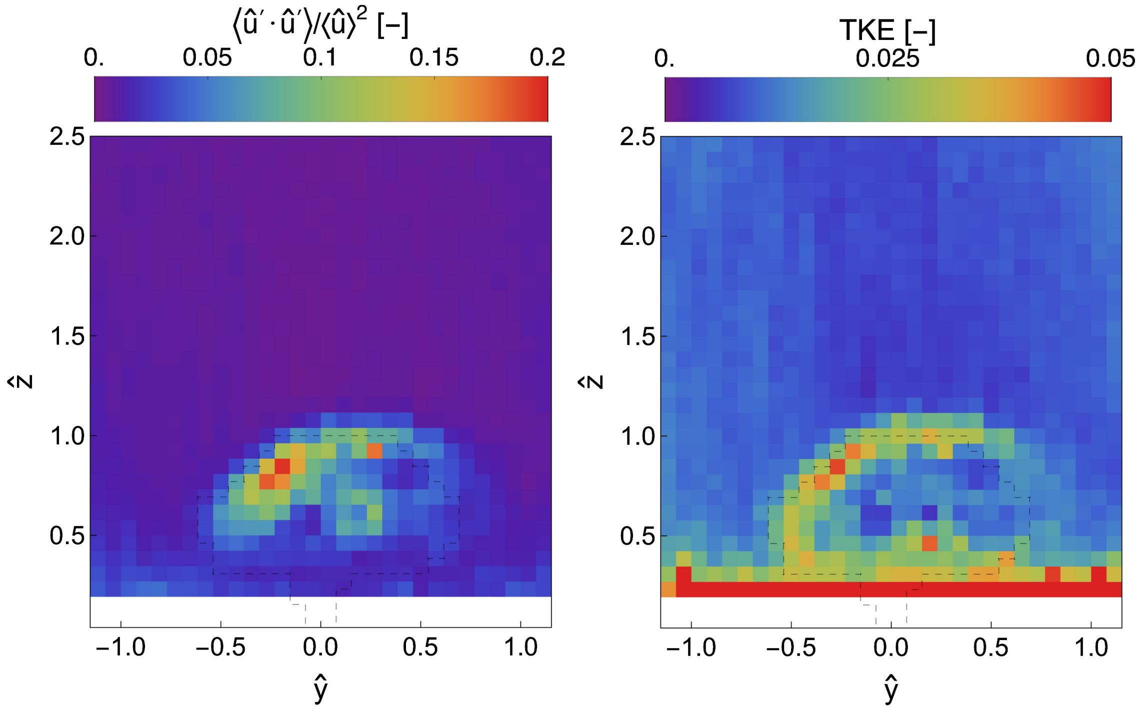


Figure 3: Contour plots of the estimated turbulence intensity (TI) and turbulence kinetic energy (TKE) along the scanning plane.

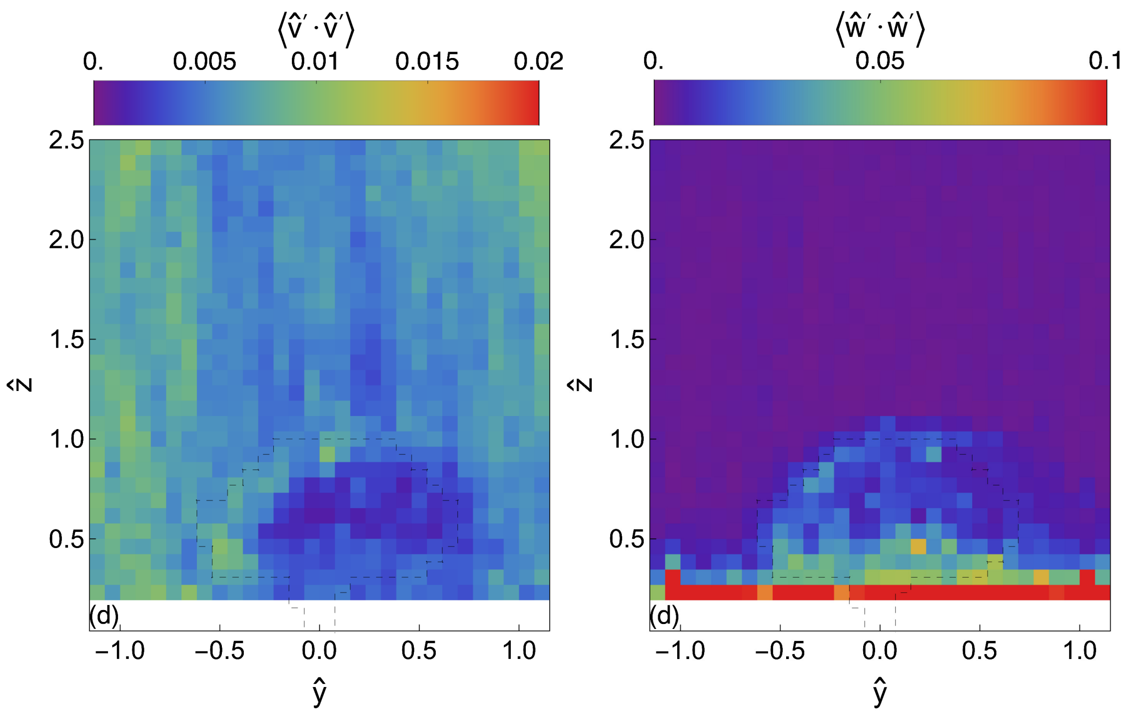


Figure 4: Contour plots of the estimated $< v'v' >$ and $< w'w' >$ variance along the scanning plane.

7. Equation 2 is not the correct expression for the eddy-viscosity closure. This should be done using the mean strain tensor. On each side of the equation there should be a tensor (in LHS the anisotropic Reynolds stress tensor). Here a vectorial expression is proposed. The correct hypothesis involves the normal stresses, which are not included in the vectorial representation. The main problems in closures are coming from the normal stresses. In the same equation K should be replaced by the standard notation ($\nu_T$). The authors claim in the abstract, in the discussion and in section 4.3 that the Boussinesq hypothesis has been tested and validated. These claims should be modified because the tensorial expression has not been considered. Furthermore, the authors have access here to a 2D data base; they cannot claim that the full eddy-viscosity assumption, involving 3D closure of a tensorial expression, was tested. The flow behind a tree, with all forcing scales involved (through the complex structure of the tree and the tree branches) is a complex flow for which very likely the eddy-viscosity assumption cannot be valid.

We agree with the reviewer that the Equation 2 in the original version of our manuscript did not correctly state the Boussinesq hypothesis. We suggest replacing the lines 172 – 179 of the original manuscript with:

*According the eddy-viscosity hypothesis the deviatoric part of the Reynolds stress tensor is related to the mean strain as:*

$$\langle u_i' u_j' \rangle - \frac{1}{3}\langle u_k' u_k' \rangle \delta_{ij} = -\nu_T \left( \frac{\partial U_i}{\partial x_j} + \frac{\partial U_j}{\partial x_i} \right), \tag{4}$$

*where $\nu_T$ is the eddy diffusivity which is a property of the flow and the term $\left( \frac{\partial U_i}{\partial x_j} + \frac{\partial U_j}{\partial x_i} \right)$ represents the mean strain.*

Later in the text, in the old lines 188 – 189 will be replaced by:

*In this study we focus on the transport mechanism of the longitudinal momentum. For this purpose we construct a momentum vector from the following two components:*

$$\langle u_1' u_i' \rangle = \nu_T \frac{\partial u_1}{\partial x_i}, \ \textit{where}\, i = 2, 3 \tag{5}$$

*With the above equation we want to express the relation between the momentum flux and mean gradient. In this equation the along wind gradients of the vertical $\frac{\partial u_2}{\partial x}$ and transverse components $\frac{\partial u_3}{\partial x}$ are assumed to be zero. In Fig. 4 b–c, we can visually observe that the areas with high gradients are characterized by also strong momentum fluxes with an opposite sign, as Eq. 3 requires.*

Our finding that the vector of the momentum fluxes of the streamwise component is anti-parallel to the vector of the transverse gradients, suggests that the momentum fluxes are proportional to the gradients and therefore they can be predicted using the eddy-viscosity hypothesis. Overall, if we include all the components of the stress and strain tensor, except the normal gradients, then the eddy viscosity hypothesis can be expressed as:

$$\begin{bmatrix} \langle u'u' \rangle - \frac{2}{3}k & \langle u'v' \rangle & \langle u'w' \rangle \\ \langle v'u' \rangle & \langle v'v' \rangle - \frac{2}{3}k & \langle v'w' \rangle \\ \langle w'u' \rangle & \langle w'v' \rangle & \langle w'w' \rangle - \frac{2}{3}k \end{bmatrix} = -\nu_T \begin{bmatrix} 0 & \frac{\partial U}{\partial y} & \frac{\partial U}{\partial z} \\ \frac{\partial U}{\partial y} & 2\frac{\partial V}{\partial y} & \frac{\partial V}{\partial z} + \frac{\partial W}{\partial y} \\ \frac{\partial U}{\partial z} & \frac{\partial V}{\partial z} + \frac{\partial W}{\partial y} & 2\frac{\partial W}{\partial z} \end{bmatrix}, \tag{6}$$

where $k = 1/2\langle u_k' u_k' \rangle$.

Using Equation 6 and the indicator introduced in the work of Schmitt (2007), which is defined by the inner product of the stress and mean strain tensor, we find that in those grid cells with high variance the indicator is larger than 0.86 (corresponding to an angle of 30°) in 52% of the examined locations (see Figure 5).

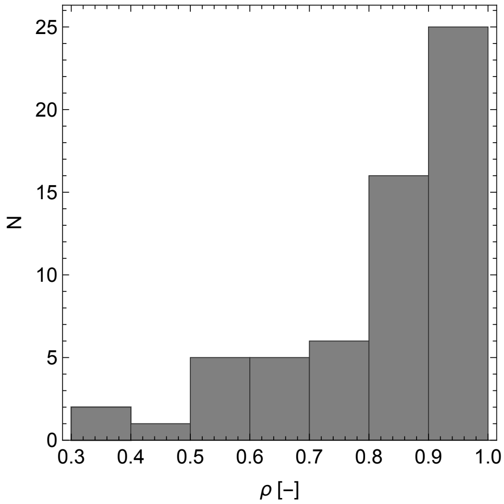


Figure 5: Histogram of the values of the indicator $\rho$ of the validity of Boussinesq hypothesis.

In the revised version of our article we will clarify in the abstract, conclusions and discussion that we focus only in the validity of the eddy-viscosity hypothesis in relation to the momentum fluxes and the corresponding transverse gradients.

8. Sonic anemometer data provide time series of the velocity done at a larger frequency (20 Hz). This could be used to perform Fourier power spectra, helping to characterize the flow (Reynolds number, injection scale, dissipation scale...). Such scales may be useful for comparison with the mixing length data which are estimated in section 4.4

   We agree with the reviewer that the power spectral density estimated using a Fourier analysis on the 20 Hz time series of the sonic anemometers can provide an insight on the turbulent characteristics of the flow in different parts of the wake. We are currently in the process of preparing another manuscript where we focus on that analysis. One of the reasons for not including these results in this manuscript is that the length scales derived by the spectra of turbulence cannot be related in a straightforward way to the mixing length scale estimated based on the flux-gradient relationship. This has been shown in the case of atmospheric boundary layer flows in the work of Peña et al. (2010).

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