Reviewer comments for manuscript ACP-2021-598

Wind lidars reveal turbulence transport mechanism in the wake of a tree

Summary comment

In this manuscript, a laser-based remote sensing instruments is used to analyze flow statistics in the wake of an open-grown oak tree. The subject is indeed interesting and relevant, given that the current knowledge on turbulence in the wake of trees is limited and that it profoundly influences exchange processes between the land surface and the atmosphere. The *Introduction* is appropriate, reads well, and is well referenced. The *Methodology* section is OK but could be improved (MC1 and MC2). The *Results* section starts off with the validation of Lidar measurements against the sonics, which is convincing and interesting but lacks important information (MC3-MC5). The authors then propose an interesting overview on spatially-distributed flow statistics, and conclude with an analysis of the eddy viscosity and mixing length concepts, which unfortunately comes across as flawed (MC6). The written english is good overall. I invite the authors to address my comments below before the manuscript can be considered for publication.

Mayor 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.
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.
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?
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?
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 $\overline{u'v'}/(du/dy) = \overline{u'w'}/(du/dz)$ (if I understood the expression correctly). I invite the authors to clarify this point.

6. L188. I am not sure what point the authors are trying to make here. The eddy viscosity is mathematically 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. ($\overline{uw} \gg \overline{u'w'}$), 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 $\overline{u'w'}$ to the total drag.

Minor comments

1. L12. Perhaps better to say “extracting”?
2. L13. Cause → Can cause
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.
4. L38. Critical extension → Since the authors are not modifying the measurement instrument/methodology, perhaps it is better to say “a new application”?