

Comments to EGUSPHERE-2025-3148

General comments

Thanks to the authors for revising the paper. The paper has improved, but it still lacks focus and rigor in some respects.

The most problematic aspect remains the momentum flux from the lidar. There have been numerous studies that quantified the accuracy of Reynolds stresses measured by the lidar. Some obtained some success with $\overline{u'w'}$ using a line-of-sight variance de-projection (e.g., <https://doi.org/10.5194/wes-10-361-2025>, <https://doi.org/10.1175/2010JTECHA1389.1>, <https://doi.org/10.1063/5.0209729>). All those authors also show extensive validation vs. reference sensors and highlight the inherent limitations due to the lidar acquisition principle, noise, and sampling error.

Here the authors propose an approach which seems totally novel for lidar applications but without any metrics for its accuracy compared to, for instance, a sonic anemometer. Moreover, the approach seems to stitch together different classic results from literature sources that are not consistent. E.g., Blackadar's paper is about neutral flow, MacDonald's is stable flow, Mellor Yamada in Eq. 26 uses a more complex formulation for the eddy diffusivity. The assumption of isotropic turbulence throughout the whole boundary layer is also very crude.

In substance, if the authors want to propose a novel method to estimate $\overline{u'w'}$ from lidar, they should consider a dedicated campaign and a manuscript. The suggestion is to remove any mention of entrainment or momentum flux and just rewrite this as a data description paper with preliminary results on gravity wave observations and wake validation.

The motion correction results are now more compelling thanks to the bug fix and the new figures, but the tilt correction still has some issues (see below).

The comments about the lack of wind speed/direction and the use of RHI for wake scanning were partially addressed, but the limitations have been discussed more carefully so can be acceptable for preliminary work like this one.

Specific comments

- Section 3.3: The tilt correction in Section 3.2 is only done for the vertical component, but the horizontal component also suffers from tilt effects and requires a correction. Please revise.

- L372, “This correction does not affect the turbulence statistics,”: This sounds incorrect, because a static tilt angle if uncorrected or not corrected adequately will change the estimated Reynolds stresses. For instance:

$$w'_{tilt} = u' \sin \theta + w' \cos \theta \rightarrow \overline{w'^2_{tilt}} = \overline{u'^2} \sin^2 \theta + \overline{w'^2} \cos^2 \theta + 2\overline{u'w'} \cos \theta \sin \theta$$

Said differently, Reynolds stresses change by changing the axis on which they are evaluated.

- Section 5.2: Please add a sentence to justify why the observations are unlikely to be a KHB case based on the comment provided in the response.