Reviewer 2

Dear authors,

Thanks for the manuscript. I think in general the manuscript is well written, coherent and the contents relate to a very interesting topic, which is that of the floating lidar turbulence measurements. However, although the manuscript presents a quite interesting dataset that can be used to analyze the impact of motion-induced effects on lidar turbulence, I think that the manuscript reads more as a technical report than a journal paper. Below I will provide some general and specific comments with respect to this and different aspects of the study.

General comments

1. As I mentioned, right now we are reading an interesting technical report but not a research paper. The reader is not gaining anything new from the paper as the data and some analyses are presented without further investigation. The authors mentioned that they are going to propose a motion-compensation algorithm based on this dataset. I think that that is what this report needs to have potential for a paper, so I encourage the authors to start the paper by the planned algorithm to compensate for motion and investigate its goodness using this dataset.

We appreciate your review and your constructive feedback regarding our manuscript. In response to your insights, we have undertaken substantial revisions to bring the manuscript more in line with the conventions of a scientific paper, moving away from its earlier semblance to a technical report.

It is crucial to clarify our current paper's primary focus. We no longer address the topic of motion-compensation algorithms in the updated version of the manuscript. The initial version of the paper may have given a misleading impression in this regard.

Presently, we are not prepared to introduce an algorithm that outperforms the established methods found in the existing literature. Our central objective with this paper is to introduce and comprehensively present the dataset we have collected. Our focus is twofold: first, to identify the main sources of error in turbulence measurements using a FLS; and second, to address a conspicuous gap in the current literature. This gap largely stems from the scarcity of experimental studies in this domain, which have predominantly focused on numerical analyses, particularly in relation to mean wind parameters. Our next step involves validating our results through numerical analyses.

We envision this dataset as a valuable resource, one that can be readily shared within a consortium of upcoming (European) research projects, particularly those that are dedicated to the development of motion compensation algorithms. It is our hope that this dataset will play a pivotal role in advancing research in this field.

2. Line 31: it is nice you are aware that there are two main errors for lidar turbulence measures (most people do not know this) but it is also strange that you think that the cross-contamination effect always results in an overestimation. This is not the case always. If by some reason your compensation algorithm always assumes an overestimation due to cross-contamination, you need to review it deeply.

You are right. We have modified the text: “The inter-beam filtering can lead to either underestimation or overestimation of turbulence metrics. This discrepancy arises from the modulation of energy associated with eddies characterized by specific wavenumbers.”. Page 2, lines 38-40.

3. Table 3: I am not sure of the value of this table. The “degree” of deviation of the mobile lidar turbulence compared to the fixed lidar turbulence should be both turbulence and scanning-configuration dependent. Here you seem to average across all cycles which I guess have different turbulence characteristics, so you are kind of averaging apples with oranges.

Indeed, we averaged across all cycles, complicating the interpretation of the results. Consequently, we opted to exclude this table as it did not contribute meaningful value to the paper.

4. One important question: did you by chance make a cycle without motion at all? That would be interesting to have as part of the analysis to know whether there is an inherent bias between the units.

Certainly, we have recorded such a cycle, capturing a 2-hour dataset during which both lidars remained stationary. We have incorporated a corresponding figure (Fig. 4a) illustrating the scatterplot of the standard deviation of LOS velocity from both lidars. This plot serves as a comparison to the scatterplot obtained by the fixed lidar and the ostensibly 'mobile' lidar in motion (Fig. 4b). Page 10, lines 202-210.

5. Section 3.3/Figure 8: there should not be that much difference between the spectra of the mobile and the fixed lidar (particularly at the large scales) apart from the area around the peak at the specific period. Why is it different (see my previous comment)? Maybe some error bands could show that these differences are not significant as they seem to be?

We have addressed this crucial point also highlighted by the second reviewer. We have conducted an in-depth investigation into the spectra and the impact of amplitude on the level of spectral energy. In the previous version of the manuscript, we exclusively presented spectra associated with a 15-deg. amplitude. Our findings revealed that, at lower frequencies, the spectral energy for LOS velocity derived from the fixed lidar surpassed that from the mobile lidar.

The tilting of the lidar system introduces data acquisition from diverse air masses and turbulence conditions, leading to fluctuations in turbulence measurements. This phenomenon may account for the observed disparity in spectral energy between measurements from both lidars. In the updated manuscript, we have enhanced Fig. 7 with additional panels (a, b, c), illustrating spectra associated with a 5-degree amplitude. Notably, these spectra demonstrate an overlap in spectral energy derived from measurements of both lidars at this amplitude, within the low-frequency domain (large scales).

We added this text:

“The spectra obtained from the mobile lidar clearly exhibit a spike in energy corresponding to the rotation frequency. The height of this spike remains consistent for each motion period and is lower for the lowest amplitude. For both amplitudes, the spectral energy measured by the mobile lidar surpasses that of the fixed lidar for the higher frequencies. Moreover, this difference in spectral energy becomes more pronounced for the lowest motion period. Conversely, at lower frequencies, the spectral energy associated with a 15-deg. amplitude, derived from measurements of the fixed lidar, consistently surpasses that of the mobile lidar. In the case of a 5-deg amplitude, the spectral energy derived from measurements of both fixed and mobile lidars shows overlap." Page 12, lines 234-240.

And a possible explanation in the discussion:

"The study highlights that turbulence measurements obtained from FLS are more sensitive to changes in orientation (amplitude of motion) than to motion periods. This finding underscores the significance of changes in measurement geometry due to platform orientation. The analysis revealed a strong correlation between high RMSE and high amplitude. Additionally, it was observed that amplitude significantly influences the measurement of spectral energy, particularly in the low-frequency domain, associated with high turbulence length scales. When the lidar system tilts, it effectively acquires data from diverse air masses and turbulence conditions, resulting in fluctuations in turbulence measurements". Pages 19-20, lines 334-339.

6. Lines 309—314: these lines cannot be part of the conclusions. You have not described the motion-compensation algorithm and you are here giving us hints of what it can do. As mentioned in my first comment, I suggest you start this manuscript by proposing/explaining the algorithm and then you evaluate it by comparison with this nice dataset.

We have revised the conclusion and systematically eliminated any references to the motion-compensation algorithm throughout the entire manuscript.

Specific comments

1. Line 25: Delete an extra ")".

Thank you. Removed.

2. Line 33: Delete "in most cases, … However,".

We removed this part when we rewrote the introduction.

3. Line 33: Replace "do align" by "compensate each other".

Similar to our comment just above.

4. Introduction: Peña et al. (2022) presented another way to assess the impact of motion on floating lidars that you would like to check.

Thank you for this valuable reference. We now mentioned this study in the introduction: "Pena et al. (2022) utilized simulated lidar profiler measurements within synthetic atmospheric turbulence fields to evaluate how buoy motions affect turbulence estimation. Their simulations revealed that translational motions of the buoy notably influenced the accuracy of turbulence estimates.". Page 2, lines 53-56.

5. Lines 163—166 are also "strange" ways to measure these impacts (see general comment 3).

We removed the part linked to Table 3. See our response to your point 3 in General Comment.

6. Line 175: "latter metric" … it is difficult to understand what do you mean here.

We've enhanced this section to provide a more lucid understanding for the reader. Section 3.2, page 12.

References:

Peña A., Mann J., Angelou N., and Jacobsen A. (2022) A motion-correction method for turbulence estimates from floating lidars. Remote Sensing, 14, 6065