

# Analysis of the measurement uncertainty for a 3D wind-LiDAR - Reply to RC2 from Referee #3

Wolf Knöllner<sup>1</sup>, Gholamhossein Bagheri<sup>2</sup>, Philipp von Olshausen<sup>1</sup>, and Michael Wilczek<sup>2,3</sup>

<sup>1</sup>Fraunhofer Institute for Physical Measurement Techniques IPM, Freiburg, Germany

<sup>2</sup>Max Planck Institute for Dynamics and Self-Organization (MPI-DS), Göttingen, Germany

<sup>3</sup>Theoretical Physics I, University of Bayreuth, Bayreuth, Germany

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As far as we can see, this review is identical with the one of round 1 of the discussions. We here provide our replies again for convenience and completeness.

**Review:** *The article presents an error propagation analysis for the Cloud Turbulence Lidar (CTL). The analysis uses both synthetic and measured time series to assess the variability that could be expected in the measured time series due to the uncertainty in the 3 detectors reported longitudinal velocity. This error is treated as Gaussian and added to the longitudinal velocity which should be measured by each detector for the synthetic/experimental velocity. The 3 longitudinal velocities with added noise are then reconstructed to reproduce an approximation of the wind vector time series which would be measured, which is then compared to the original synthetic or experimental velocity.*

*The authors then use these approximated time series to test a filtering approach that can be used to post-process the added noise and reduce the random noise that has been artificially added in.*

*In general, the article is well written, and to the extent that the authors analysis is conducted, appears to be appropriate. However, there are several concerns that I regarding the applicability of this analysis to the general uncertainty output from the system. The biggest concern is simply that the uncertainty analysis only propagates a single error (specifically the detector error). As such, the study is very limited in its approach. Their own error propagation analysis provided in Section 3.6 indicates that the error is going to be amplified the precision of the support structure and resulting angle of the detectors to the plane of the lidar. The geometric dimensions of the array are assumed to be precisely known and constant. What is the sensitivity of the wind to uncertainty in the dimensions? Can this structure be assumed to be perfectly rigid during a measurement? Does the measuring distance,  $h$ , impact uncertainty? Surely temperature changes will result in some expansion/contraction of the support structure? Furthermore, uncertainty in the Euler angles used to transform the wind velocity from the lidar frame of reference to the inertial frame of reference can produce significant uncertainty in the resulting wind components. This effect is also not considered.*

**Reply:** Thank you for raising this point. Several different error sources are mentioned by the reviewer that we have grouped into three sections that we address one by one in the following. We also adapted the manuscript to include these considerations (line 40f and appendix A).

## **Geometric tolerances**

We expect this to be a negligible source of error since the precise geometric dimensions of the measurement frame can be measured before mounting of the device to the CloudKite balloon. This includes the distances between the telescopes (side length), and also the distance and lateral position of the foci, which are straightforward to measure in a laboratory setting with millimeter accuracy. The analysis presented here also assumes that all three beams hit the focal volume under the same angles, which is more intricate to ensure. A geodetic instrument like 3D laser tracker can be used to precisely measure all coordinates (instrument and foci) in 3D space with an accuracy even far better than 1 mm. From this calibration procedure the angles can be extracted and compensated for.

## **Influence of wind and temperature on the geometry**

The spatial resolution, i. e. the measurement volume, is assumed to be  $1 \text{ m}^3$ . During alignment of the setup,

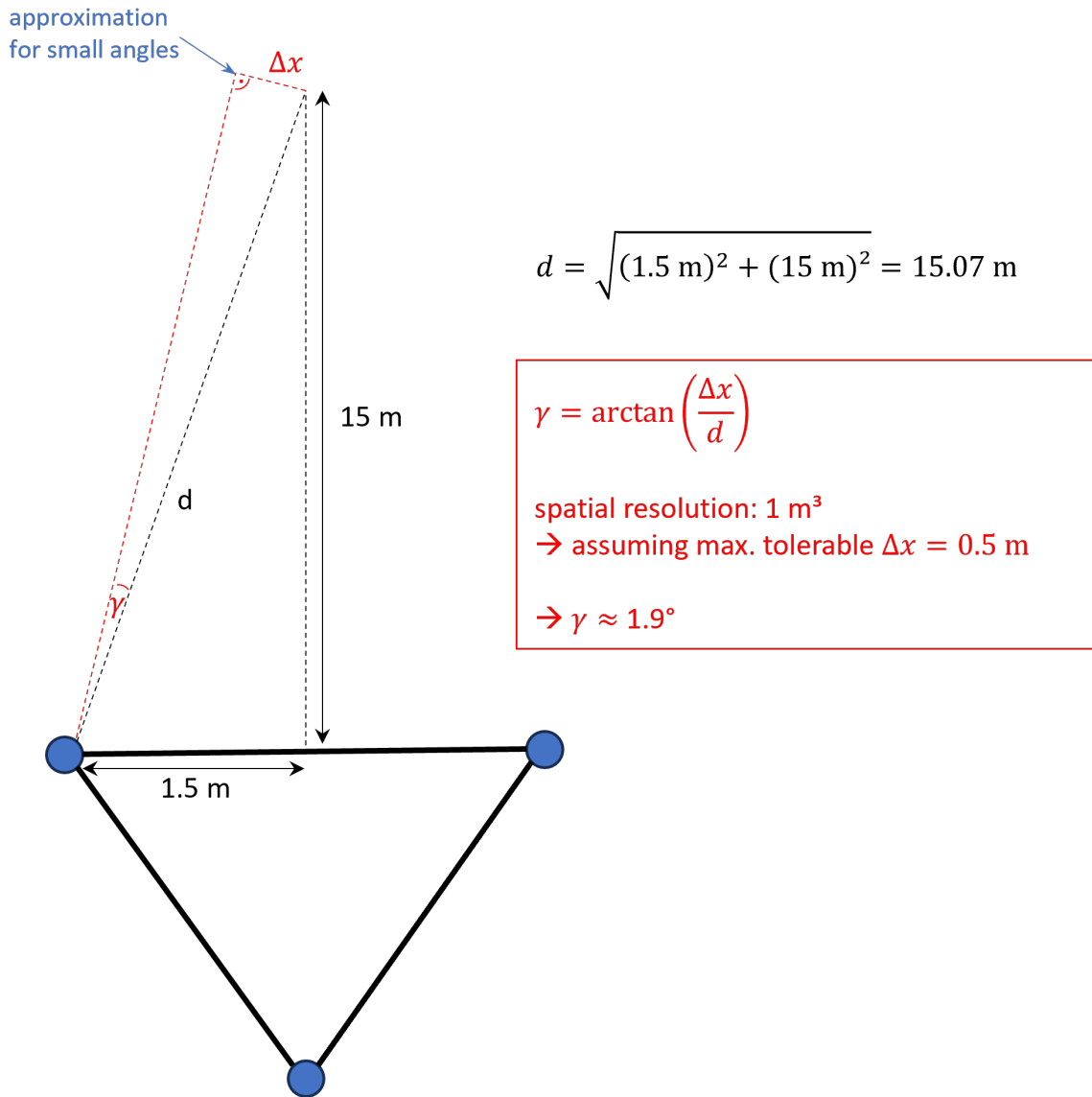


Figure 1: 2D estimation of the change in angular orientation of one telescope head required for a lateral change of its focus by 0.5 m.

before mounting, all three foci are superimposed onto one point by the use of deflection mirrors in the telescope heads.

The change in angular orientation of one single telescope required for its focus to move by 0.5 m, i. e. half the spatial resolution, can be estimated. As illustrated in figure 1, the estimated (2D approximation) change in angular orientation is  $1.9^\circ$ . Considering the stiffness of two connected carbon tubes (see Figure 1 in the main manuscript) and the very small attack surface for the wind,  $1.9^\circ$  seems like an unrealistically high value for bending due to wind which is why we think this error is also of minor importance.

Concerning the effect of temperature, we assume operating temperatures between  $0^\circ\text{C}$  -  $40^\circ\text{C}$ , and alignment of the setup under lab conditions at  $20^\circ\text{C}$ . Thus, a maximal change in temperature of  $20^\circ\text{C}$  must be considered. The temperature extension coefficient of carbon is  $2 \cdot 10^{-6}\text{K}^{-1}$ . Considering the longest dimension, i. e. the 3 m bars between the telescopes, this results in a maximal change in length of merely 0.12 mm, which is negligible, even considering the the lever of 15 m to the focus for changes in angular orientation.

### Dynamic tolerance

With dynamic tolerance we refer to the fact that the CloudKite and the attached measurement device might be moving during the actual measurement. There are several points to consider here: First, we should mention that the absolute location (in world coordinates) of the point of measurement does not have to be known precisely for these types of measurement.

Second, the influence of the motion during the acquisition of a single data point, i. e. during 100 ms, must be considered. It is known from previous measurement campaigns that the CloudKite platform motion has its

main frequencies around 1 Hz [Schröder, 2023]. This is one order of magnitude slower than the acquisition of a single data point. However, there might still be some movement within 100 ms. This can be regarded as an increase of the actual measurement volume.

Third, there is the platform motion during the whole measurement run, which might last up to many hours. This leads to a motion of the focus, i. e. the point of measurement. This motion can be tracked using inertial measurement units (IMUs). For this reason, two IMUs in each telescope head are integrated in the measurement device. Whether this also allows for the correction of the tracked movement depends on the parameter of interest in the post-processing. For example, the mean wind velocity could be corrected for the platform motion. For other parameters it can be more intricate or even impossible. However, this is an error source that influences the analysis of the measured data but hardly the individual measurement data points. Therefore, a detailed analysis of the consequences of this platform motion is beyond the scope of this paper.

**Review:** *Therefore, as noted, this manuscript is somewhat limited to simply the propagation of the detector uncertainty to the measured wind in the lidar plane of reference, with an analysis of the filter/smoothing functions best suited to reduce this added noise. In this context, careful characterisation of the detector uncertainty would be important for the analysis. However, this characterisation is limited to just two sentences, which does not sufficiently justify the stated detector uncertainty of  $\sigma^{det} = 0.04\text{ms}^{-1}$ . From what I can tell, the authors take the resolution of the sensor output of  $0.01\text{ms}^{-1}$  and assume a 99 % confidence bound(?) of  $3\sigma$  to get a standard deviation of the uncertainty of  $0.04\text{ms}^{-1}$  (assuming that they are rounding up?). However, this is just guesswork on my part. If this is the case, then the error propagation analysis is not even assessing the uncertainty of the individual lidar measurements, but is simply assessing the propagation of the resolution limitations of the individual lidar measurement.*

*I would therefore recommend that the authors, at the very least, provide more detail and care into the assessment and description of  $\sigma^{det}$ . The paper would also be much more strengthened by including additional error sources into their analysis, however this may require significant revisions of the manuscript.*

**Reply:** The main topic of the paper is, indeed, how each individual measurement error from three line-of-sight LiDAR sensors contributes to the measurement uncertainty of the three spatial components of the measured wind vector. Therefore, the individual measurement error  $\sigma^{det}$  is the relevant quantity.

While our methodical approach is general and thus applicable for different values of  $\sigma^{det}$ , it should still be clear how we derive  $\sigma^{det} = 0.04 \text{ ms}^{-1}$ . We have added a new section 2.2 on the estimation of  $\sigma^{det}$ .

Also, it seems the reviewer accidentally used a sensor resolution of  $0.01 \text{ ms}^{-1}$  instead of  $0.1 \text{ ms}^{-1}$ . May be this also contributed to the confusion?

**Review:** *Additional comments:*

1. **Review:** *Figure 2 was a little confusing for me due to the perspective. Specifically it took me some time to understand that the plane of the lidar was parallel to the oncoming wind field. I think the confusion comes from the kite being angled to the mean wind, but the lidar appearing to be drawn on the kite. Once I had figured out the arrangement of the CTL on the MPCK, the text of section 2.2 made more sense, but perhaps the authors may wish to add more details/different views to Figure 2 so that others may not be equally confused.*

**Reply:** We thank the reviewer for this hint. We adapted Figure 2 accordingly, so that it is more intuitively clear now. Specifically, we have simplified the drawing so that only the lower part of the keel is shown, which now is perfectly aligned with the  $x$ - $z$ -plane, as in reality.

2. **Review:** *Tables 1 and 2 have redundant information and could be combined. Note that whereas Table 2 refers to 10 Hz as the sampling rate, Table 1 refers to the same quantity as the time resolution. Technically, the time resolution is 0.1 s, not 10 Hz.*

**Reply:** Thank you for the suggestion. We have considered combining the two tables. However Table 1 summarizes some basic specifications of the LiDAR setup whereas Table 2 specifies all modeling parameters. We therefore think that it is appropriate to keep the two separate. The only redundant information is indeed the sampling rate (we changed the wording to measurement rate as this is more appropriate). We have changed ‘time resolution’ to ‘measurement rate’ in Table 1 to make the presentation consistent.

3. **Review:** line 182: should be 'lose', not 'loose'.

**Reply:** Thank you, we have corrected the typo.

4. **Review:** 4. *The synthetic time series input the noise as a white noise process, what justification is there that the detector uncertainty appears in the form of white noise. Note that the nature of the noise could impact the efficacy of the smoothing for noise removal/uncertainty reduction.*

**Reply:** Thank you for raising this question. We have added section 2.2 to the manuscript to describe the assumed measurement uncertainty in more detail.

On the detector we have the coherent interference of the local oscillator with the signal. The local oscillator is adjusted so that it is the dominant noise term, hence we are in the shot noise limited detection scheme. Consequently, there are many photons arriving on the detector (more precisely: on each of the two balanced photo detectors). Also, having a peak coming out of the noise floor (otherwise the wind velocity can not be extracted, which can happen in reality, of course) means that there is also a significant number of signal photons involved. For large numbers of photons the Poisson distribution approximates the normal distribution very well.

Now the subsequent question is: Does the dominance of white noise lead to an uncertainty of the peak position that is normally distributed? We have performed a simple simulation of a beat signal with added white noise, averaged 4000 spectra as in a typical measurement, and analysed the peak positions which were found by fitting a Gaussian function (see Appendix C). The result shows two things:

- (a) The peak positions and thus the velocities follow a normal distribution. It makes intuitively sense that the uncertainty of the peak position follows a symmetric function. The Poisson distribution is not symmetric for small mean values, but it approximates the normal distribution well for large mean values.
- (b) Increasing the level of added white noise, i. e. decreasing the signal-to-noise (SNR) ratio, leads to an increase in the fluctuation of the peak positions (the measured wind velocity). This shows that a decreasing SNR leads to increased uncertainty. However, it remains normally distributed for low SNR.

Also, this is the main criticism mentioned by referee #1. As referee #1 had some additional comments on this matter, we would also like to refer to our reply to referee #1.

## References

Marcel Schröder. *Cloud Microphysics Investigations with the Cloudkite Laboratory*. Phd thesis, Georg-August University Göttingen, Göttingen, March 2023. Available at <https://hdl.handle.net/21.11116/0000-000D-06A7-0>.