We thank Anonymous Referee #1 a lot for reviewing our manuscript thoroughly. Their feedback helped us to improve the text, so that it is easier to read and understand. Below you find a copy of the referee's comments and our response marked in red.

Anonymous Referee #1

The article by Kelberlau and Mann addresses an interesting problem that profiling lidar measurements are facing. Now-a-days, the site suitability of wind plants depends on the profiling lidar data, and the cross-contamination issue is a great concern for it. Thank you for working on this topic.

The article provides the required background theories and explanations to follow the document. The article is a follow-up of their previous article (Kelberlau and Mann (2019)) that they have published on a technique called squeezed wind vector reconstruction to reduce the cross-contaminations of lidar measurements. This article is an application of the published technique to the WindCube V2 profiling lidar data. The published technique is based on the Taylor 'frozen' hypothesis (turbulent eddies are advected by the mean wind speed), and the hypothesis can only be applied along the mean wind direction. However, the authors have worked on the non-aligned (line-of-sight(LOS)is not aligned to the mean wind direction) flow using the concept of Taylor 'frozen' hypothesis. In the end, the authors show that their method works mainly for he aligned flow. Even for the aligned flow the technique does not work for the spanwise component. Therefore, the application and effectiveness of the work is limited.

In the article, the authors have introduced separation distance as a part of the squeezed reconstruction technique. Separation distance should a parameter based on the mean wind speed and time lag. The authors have mentioned that separation distance represents statistical average, not the actual separation, and I do not understand the exact application of this parameter in this work. The temporal frequency of the data collection by the lidar is low unlike continuous wave lidar (what the authors have worked on their previous article) and LOS measurements (full scan) are updated in every 3.85 s. The authors have not provided any clear data on the amount of reduced distance (and corresponding time delay) between the LOS measurements due to their technique.
In section 2.5 we describe that the longitudinal separation distances are reduced to the values given by Eq. (25) when the method of squeezed processing is applied. A visualization of the conventional and reduced longitudinal separation distances is included in the revised version of the manuscript (Figure 2).
Considering the low temporal frequency of the lidar (WindCube V2) data, the authors will not get measurement data at the target time after the considered advection. The method would be beneficial if there is high frequency data so that the user can get more data in space to take the benefit of the reduced spatial distance. It is not clear what the authors do here. It would be nice if the authors provide a block diagram of the work process of the squeezed reconstruction method applied in this work (with a sample data). In addition, showing a figure like Figure-4 of Kelberlau and Mann (2019) article would be nice.
We added Figure 2 to the manuscript that we think adds clarity to what we do. It shows the position of measurement locations in parallel to Fig. 4 of Kelberlau and Mann (2019) but will also help to better understand the concept of longitudinal and lateral separation distances.
Coherence model based on measurements is showing that the longitudinal coherence drops to approximately zero with 90 m separation distance and the corresponding wave number is close to 0.06 (Davoust et al. 2016). Then why is the Taylor 'frozen' hypothesis effective here?
Davoust and Terzi (2016) measure low longitudinal coherences upstream of an operating wind turbine. In a similar experimental setup, e.g. Schlipf et al. (2015) find longitudinal coherences which are stronger than the values of Davoust and Terzi (2016) but still far from unity. Both studies measure in the induction zone of operating wind turbines which might have an influence on the

longitudinal coherence. Also yaw-misalignment might reduce the measured coherence. We measure in undisturbed flow where Taylor's frozen turbulence appears to be a reasonable assumption for the wave numbers of interest.

D. Schlipf, F. Haizmann, N. Cosack, T. Siebers and P. Wen Cheng, Detection of Wind Evolution and Lidar Trajectory Optimization for Lidar-Assisted Wind Turbine Control, Meteorologische Zeitschrift, Vol. 24, No. 6, 565–579, 2015

Specific comments:

1.Page-4, L-8: These line-of-sight velocities are the product of...Is this the way lidar measures? We numerically model the lidar measurements in this way. Make it clear.

In order to make clear what the lidar does and how we model this behaviour we changed this passage to: *"These line-of-sight velocities are the weighted average of the radial wind velocities along the stretch of the lidar beam illuminated by the range gate. A reasonable weighting function to model the line-of-sight averaging is the convolution…"*

2.WindCube V2 has pulse repetition frequency (PRF) 30k.The default PRF used by the lidar is 20k; then required time for LOS measurements should be 30/20=0.6667s. Why it is 0.72 s? And also, why the required total time to finish the scan pattern is 3.85 s?

The timing data given in Table 1 is extracted from original Windcube output files. The time in excess of 0.6667s is probably required to switch from one beam direction to the other. We do not know why the switching from one cardinal direction to the next occurs faster (0.72s) than switching to the vertical beam (0.97s). We added: "*The reason for the different times required to change the beam direction is not known to the authors.*"

3.How do the authors get Equation 17?

We added a step in Eqs. 16 and 17 and described in a different way where these equations come from.

4.Are all the conditions provided in Page-9, L-10 and L-11 correct?

We checked these conditions carefully and believe that they are correct.

5.Is k_{scan} (Equation-23) related to the scan pattern time? Or is it particularly related to the vertical beam (5th beam)? Due to the vertical beam, there are no measurements for horizontal wind speed. Could the authors make a comment on it?

k_{scan} is not only relevant for the vertical beam but for all five beams because each line-of-sight velocity is updated after 3.85s. The lidar is therefore blind for all wind velocity components at k_{scan}. We added this aspect to the text and wrote "*That means turbulent fluctuations which occur with the same frequency cannot be detected by any of the Windcube's lidar beams.*"

6.Page -11,L-19: what do the authors mean by, "for the combination of LOS2 and LOS4 it is similar"?

We mean that all what was described for the LOS1-LOS3 beam combination is true also for the LOS2-LOS4 beam combination but since this was unclear to the referee we deleted the quoted sentence.

7.Do the authors use Equation 25 (separation distance, r_{rep}) to calculate the time delay? How Equation 25 is different from Equation 24? It is not clear here.

The time delay is based on r_{real} We fixed this mistake in the manuscript. We will use the new Figure 2 also to visualize the difference between r_{real} (Eq. 24) and $r_{real,SQZ}$ (Eq. 25).

8.Second paragraph of Page 12, L-8 to L-12: "The methods of squeezing.." is irrelevant here. Remove the whole paragraph.

We followed the referee's recommendation to remove the whole paragraph.

9.Page-14, L-6: In case of three?

We change the formulation to "…and in the third case…"

10.Page-14, L-20 to L-25: Rewrite the sentences so that reader can understand the process: "In the process of reconstructing..."

We rewrote these sentences and also here we use the new Figure 2 to visualize the process of rearranging the measurements.

11.Page-17,L-1:The authors mention to explain the results: "the reason is that two different longitudinal separation distances are involved in the wind vector reconstruction process.... ".

However, in Table-1, authors showed that the second longitudinal separation distance is zero for the aligned flow. Could the authors explain what are they trying to explain here?

Table 2 is gives the **representative** longitudinal separation distances (r_{rep}) which are not the real longitudinal separation distances (r_{real}) as we explain at the end of section **2.4**

To improve the comprehensibility, we extended the caption of table 2, fixed the reference to section 2.4 and included a note that r_{rep} is not equal r_{real}.

12.This article is particularly trying to solve an active industry problem and the article needs to be coherent in terms of explanation which I don't see in Section4. Explanation of the results is so vague that I get always lost.

Section 4 gives a description and interpretation of many different spectra. We intend to explain them as precise as possible. However, the high number of effects considered and the use of specific terms can create confusion.