Dear Dr. Westergaard,

We appreciate your recommendation for this article to be published. Also thank you for the high level of detail in your review, which we will take into account very seriously. Below we will address each of your comments separately.

1. (major correction) Abstract, p1, l11 and l124 “the larger measurement probe volume” (L11) is directly in conflict with the actual results and the statement in L14 “lidars to accurately measure small scale flow structures”. This language should be more precise and accurate to the results. My suggestion would be to accurately describe two things: a) The measurement volume of the LIDARs is 13 to 50 cm, allowing average flow features to be resolved at 0.11D to 0.57D in the far downstream of the three turbine. b) The same volume limitation results in an effective spectral cut-off frequency between 22 and 11 Hz, respectively. This is compared to high resolution HW data.

   It is true that there is a conflict between the lidar’s ability to measure small scale flow stuctures on the one hand, and the probe length averaging on the other. Therefore this will be rewritten to provide a better picture of the capabilities. Your concerns regarding the measurement volumes will be addressed in your **comment #13**.

2. (minor), p1 L14 Why is “January 2016” important ? Maybe reformulate.

   We thought it is relevant side information to know when the measurement campaign took place. However, we will move this date to the end of the sentence, where it fits better with regard to its importance.

3. (please elaborate) w-influence, p4, L24-28 w is neglected and deemed insignificant. Never the less, it is measured to be 0.08 m/s and must surely introduce a bias, especially as the vertical projection of the measurement volume is between 0.7 and 2.6 cm (at 3 deg downwards beam orientation, and the two ranges given in fig 5).

   It is a good observation that the vertical wind speed component indeed has a contribution and one could argue that it is not insignificant. Therefore we will extend the uncertainty analysis by incorporating the potential bias in the other two wind speed components that could be caused by neglecting the vertical component.

   (major correction) coordinates of fig 4 Please correct figure 4, so the coordinate system can be understood. Preferably x/D and Y/D, so it is comparable to figure 3

   The only purpose of Figure 4 is actually to sketch the shape of the probe volumes and therefore it is not plotted to any scale. If we use the scale that corresponds to Figure 3, one can barely see the probe volumes, since their dimensions are so much smaller than the focus distances. Therefore we prefer to keep it like this.

4. (major correction) “rated conditions”, P6, L15 and L19 (possible other places) “turbines operating under rated conditions” is nonsense. In rated conditions, the pitch of the blades is going positive and all research shows the wake disappears. So, I am pretty sure the author means that the turbine is operating close to maximum wake deficit or maximum Cp. Please be specific: TSR = xx, Pitch = xx, corresponding to an estimate Ct of xx, resulting an estimated wake deficit of xx.

   According to our point of view ‚*rated conditions*‘ occur exactly at the one point where the wind turbine first reaches maximum power (also maximum C_p and C_T are theoretically achieved here) and the transition between torque control and pitch control takes place. At this point you indeed have the highest wake deficit. The pitching will occur for wind speeds between the rated wind speed and the cut-out wind speed, in which case the wake is indeed expected to disappear. We will make sure that the reader knows which operational conditions we mean. Therefore we will also provide values for the tip speed ratio, pitch angle and the thrust coefficient of the wind turbine during each part of the experiment.

5. (minor) TI p7, L2 Why not be specific, TI=5.4% (as measured, table 1)

   Good point to state the more precise measured turbulence intensity here. This will be adjusted.

6. (major) Table 1 and text on p7 There is an average v and w component. Albeit fairly small it is not really clear if this is a flow feature or probe misalignment ? Please be precise in the language about this.

   At the time of writing this response, it is not known where this bias stems from, but it will definitely be investigated and we agree that this has to be addressed in the revised manuscript.

7. (major) Table 1 and text on p7 There is no difference between u’ and v’ in the table, but yet a very velar difference in figure 8 and 9 scatter ? Either observe or comment on this.

   This is indeed an interesting observation which we did not pay much attention to so far. It could just be caused by the scaling of the plot and the range of the wind speeds, but this will be checked.

8. w component Same comment as C3

   See the answer on **comment #3**.

9. Error in figure 9 The offset in fig 9 red curve is indicated to be 0.00 – Looks like a mistake. Please check.

   In Figure 9 it can be clearly seen that the red regression line crosses the point (0,0), so we could not identify what error you mean. However, it is strange that the slope is quite far off unity. This will be reassessed.

10. (major) effects comparing fig 8/9 to 12/13 P9, L2 “the mentioned effects caused the scatter”. I don’t think this is accurate. The 3 mentioned effects (p7 L23 to p8 L2) is bias effects. Why would bias effects cause scatter ? The more likely effect is that the small scales are not resolved by the LIDAR.

    True, after looking through the reasons given for the scatter, it is concluded that only the first point could indeed be valid. However the other two points could be an explanation for a bias but not for scatter. Also the lidar not being able to resolve small scales is a possible reason. Therefore this part will be slightly rewritten according to this concern.

11. (minor) p9 L3 Delete “most of the very” And “are omitted here” maybe you mean is “averaged away by the large measurement volume”

    You are right that this sentence is not very accurate. Therefore we changed it to ‚…*since small scale fluctuations are averaged out*.‘.

12. (minor) p9 L5 and L6 L5 “in the wind tunnel” should be “in the wind tunnel at 1 Hz”. L6 “are estimated that well” should be “at 1 Hz follows the same trend”.

    It is indeed good to state that this validation is only valid for the 1 Hz measurements. That is why we adapted your suggestion here.

13. (major) p10, L10 The spatial resolution is given to be in the range 0.13 to 0.5 me. This gives 1/ 2*5.6/0.13 to 1/2 *5.6/0.5, which is 22 Hz to 5.5 Hz temporal resolution.

    It is good that you mention this, because there is a slight inconsistency in the manuscript. Namely the probe lengths of 0.13 m and 0.50 m indicated in Figure 5 are valid for focus distances of 10 and 20 m, respectively. However the point measurements were actually executed at a focus distance of 9 m, yielding a probe length of 0.10 m instead of 0.13 m and this translates to the calculated 28 Hz resolution. We will make sure make this consistent throughout the manuscript (in the text and the figures).

    It is generally misleading that it is suggested the LIDAR can measure up to 390 Hz.
    And the results shows the true range of resolution is 5.5 to 22 Hz. Please correct this throughout the paper.

    It is also true that, although the lidar has a sampling rate of 390 Hz, the true resolution of small scales is actually lower. This difference will be described more clearly, also taking into consideration your **comment #1**.

14. (major) figure 16 Introduce 5.5 to 22 Hz. 28 Hz is inaccurate. Also, 5/3 rule should be 5/3 Kolmogorov (since “rule” is not discussed)

    Please find the reason for using 28 Hz in **comment #13**. The manuscript will be adjusted to be consistent. Also the Komolgorov rule will be introduced in Figure 16 as you suggest.

15. P11, figure 17/18 It would be prudent to present 22 Hz (or maybe 5.5Hz) filtered data.

    In the case of Figure 16, we do not see the need to post-process the data by filtering it to 22 Hz.

16. P11, comments pertaining to figure 18 and w-component It would be prudent to comment that at 3D the w-component is not insignificant and this could have resulted in the "v-component signature", otherwise the reader is left with the impression that this is the turbulence, which is probably not the case.

    You raised the valid issue here that the vertical wind speed component (w) might have a significant effect here and even causes the signature in the lateral (v-)component of the flow. This will be discussed in the paper. Also see the answer on your **comment #3**.

17. P11, L16 (minor) "determine local 2D effects", I think you mean "measure a cross section of a wind turbine wake" or something like that

    Since this statement was indeed a bit vague, we changed this sentence to ‚*It (the figure) illustrates that the lidars are capable of determining the two-dimensional flow across a wind turbine wake cross-section.*'.

18. P11, L19 "as well as tip vortex". I don't think that is very clear. It could be turbulence in the shear layer, or the w-component or the shear layer itself.

    It will be discussed in the paper that tip vortices cannot be identified with certainty, and the possible other reasons you give will be mentioned and discussed.

19. P11, L21 (major) I tin you either need to reference where the "turbulence in the lower region" has been observed in other studies, or reference shear layer or make it clear that your speculating. Because it could be due to velocity bias in the measurement.

    Let it be clear that the ‚lower region' refers to the lower part of the plot. The wording might be a bit off here, but we are quite certain that some local effects in the v-component are ‚washed away' by the presence of the other overlapping wakes. We will try to state this in a different way and make sure that this is a speculation and not a hard fact.

20. Figure 19 and 20 Introduce 19 a , 19 b and 20 a and 20 b for clarity.

    All subfigures will be alphabetically numbered. See also **comment #23**.

21. Figure 20 There is a clear periodicity in figure 20, v – component. Is this due to reconstruction or something else. The periodicity is about 1.8 m ? Also there is some strange signatures of u and v velocity at y/D = 1 and -3 which occurs to be artificial ?

    This periodicity is most likely caused by the wind field reconstruction method or interpolation scheme. It will be investigated whether these effects can be filtered out. Also the ‚strange signature' is probably caused by invalid measurements. They will be either filtered out or their presence will be discussed.

22. Figure 20 There is distinct different direction on the v-component coming of the rotor comparing turbine 1, 2 and 3. Why ? is there a missed observation ? measurement error due to w-component?

    We think that directional differences are mainly caused by the interaction of the multiple wakes. However, this lies beyond the scope of the paper, so it will not be discussed.

23. Figure 21 and 22 Introduce 21 a , 21 b and 22 a and 22 b for clarity.

    All subfigures will be alphabetically numbered. See also **comment #20**.

24. p12, section 3.4 I am not wildly convinced the uncertainty analysis is fully representative. The two conditions given on pg. 12 does not include a number of factors. Also there is not consideration of bias errors, which may very well be higher than the uncertainty presented. I appreciate the authors can not give this now, but at least elaborate a bit more on other potential sources and their nature.

    It is true that the uncertainty analysis is not representative of all physical effects that possibly have an influence. It is mainly aimed at modeling how a given uncertainty or bias is propagated through the dual-Doppler lidar reconstruction. However, in this way of expressing the error, there is no clear separation between bias and uncertainty. A bias in either the line-of-sight measurement or one of the scanning angles will be propagated in the exact same way. Potential sources for uncertainty and biases will be elaborated on further.

25. Conclusion This: “Because of the lidar measurement principle, between 10-15% of the data is lost due to the moving wind turbine blades in the measurement region” is hardly discussed in the paper, and not what the paper is about. Suggest to strike. It would be prudent to summarize resolution results, frequency and measurement volume over D. Also, comment on future work ?

    You are right that the conclusion should not give this new information, without it being mentioned anywhere else. Also the sentence is not clear. Therefore this sentence will be omitted and the data availability will be mentioned somewhere earlier in the paper. Also you stress the importance of concluding on the lidar resolution study, which is valid and will be emphasised in the conclusion in a better way.