Comments on the Review of Changing the rotational direction of a wind turbine under veering inflow: A parameter study - Reviewer 1

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Dear Dr. M. Paul van der Laan,

Thank you for taking the time to carefully review our completely modified version of the original manuscript. We read your review in detail and appreciate you sharing your own simulation results. Here is our response to your comments:

Main comments

It is nice that you have added Section 3 and I now better understand your arguments. You mention that different magnitudes of wind veer or directional wind shear (ds) can result in different trends of wake deflection direction. In Figure 7, you show that for small ds (0.04 and 0.08° m^{-1}), a CCW rotating rotor has less wake deficit compared to CW rotating rotor (for the Northern Hemisphere). For large values of ds (0.16 and 0.20° m^{-1}), your simulations show the opposite. Both CW and CCW show a similar wake deficit for ds = 0.12° m^{-1}. I have summarized your results below (for an aligned case on the Northern Hemisphere):

I have performed two additional RANS cases with different values of ds: 0.028, 0.045, corresponding to turbulence intensity values of 4.5 and 4% at hub height, respectively. The results are plotted in Figures A1 and A2. I had previously simulated ds = 0.095. (Note that there is a misunderstanding regarding my RANS simulations from the first review round. I had used the NREL-5MW wind turbine, which has a rotor diameter of 126 m. The total wind veer of 12° then represents a wind veer of about 0.095° m^{-1}.) I have also summarized the results below for the RANS simulations (for an aligned case on the Northern Hemisphere):

Here is a misunderstanding. In the first manuscript version we apply CW for Clockwise Wake and CCW for CounterClockwise Wake. In the revised version we change to CR for a Clockwise Rotor rotation (corresponding to counterclockwise rotating wake CCW) and CCR for a CounterClockwise Rotor rotation (corresponding to clockwise rotating wake CW), as we thought it is more intuitive to think about the turbines rotation. Furthermore, we avoid the misunderstanding of interpreting CCW as CounterClockWise and CW as ClockWise. We are sorry if we caused any confusion here. So in your tables CCW should be CCR and CW should be CR.

ds	0.04	0.08	0.12	0.16	0.20
EULAG favoured rotor rotation	CCR	CCR	similar	CR	CR

Comparing this to your results:

ds	0.028	0.04	0.095
Reviewers favoured rotor rotation	similar	CR	CR

I still get the opposite results compared your LES model. If I would try to further increase the amount of directional shear in the RANS model, then the resulting boundary layer height would become smaller than the wind turbine, and then the RANS simulation would not converge because the wind turbine operates partly in the free atmosphere where the eddy-viscosity is very small. If I decrease the amount of directional shear then the effect of rotor rotation direction on the wake is negligible at 7D. To be fair, it could be that my RANS model predicts the wrong results and your LES simulations are correct.

Probably there are other influence factors between EULAG and your RANS simulation, leading to a deviation of your results from the prediction of the theoretical analysis and the EULAG results in our manuscript. E.g. looking at the equations in your papers, you apply external forces on the right hand side. Dependent on your boundary conditions etc. they could result in a large scale pressure gradient, which affects the meridional velocity component. This could explain the deviation from the theoretical analysis and the EULAG results. Here, we think further personal discussion between the Reviewer and the authors is necessary.

Further, the difference between clockwise and counterclockwise rotating actuators should not depend on the applied parametrization in this work. In a previous paper Englberger et al. (2019) (which will be published soon), we investigated the rotational direction impact for an SBL case, applying 2D slices of u, v, w, and Θ at each time step, which result from a precursor SBL LES. In Englberger et al. (2019), the wind veer is limited to the lower rotor half with 0.28° m^{-1} between 10 m and 115 m. In this case, if the actuator rotates counterclockwise the wake width and the wake deflection angle are larger and the maximum velocity deficit is smaller (more rapid wake recovery downstream). Therefore, the preferred rotational direction of the rotor in this study is counterclockwise, as it results in a higher downstream u-value up to at least 10 D. Comparing the directional shear value to this study, also the vertical extend of veering inflow matters. Different conditions of the mean wind field could also lead to differences in the directional shear value, at which both wakes behave similar regarding their velocity deficit.

The favored rotor rotation is also dependent on the relative wind direction, as shown in Figures A1 and A2. If your mean wind direction at the AD is slightly off because the inflow is developing downstream, then you could have post processed a two wind turbine case with a misaligned wind direction. I would therefore recommend strongly to also post process different wind direction cases, similarly to Figures A1 and A2. For example, you could add another figure for the cases listed in Fig. 7, taken at 7D, but as function of wind direction (or y), similar to my RANS figures, but then for wind speed not wind turbine power. If you have saved the time averaged 3D flow fields, then you should be able to just process your LES results to obtain such a plot.

In our simulations, we apply a homogeneous surface and prescribe the background wind field, which is only modified by turbulence (and the WT itself). Further, we do not apply any additional external forcing acting as large scale pressure gradient and therefore the wind direction in front of the wind turbine is constant in each simulation with $\overline{v(z_h)}_{time} = 0$ (see Eq. 8). This allows us to only reproduce your plot for 270° (see Fig. 7).

2. I now understand that the inflow is based on a parametrization of LES precursor simulations, performed in previous work, and it is good that you have added more clarifications in the revised the article. I have looked at the articles where this method was introduced and I wonder how well the parametrized inflow is in balance with the LES model when the parameters are changed significantly. For example, there are quite large deviations of the fitted LES profiles (as shown in Fig 1. of your previous work Englberger and Dörnbrack (2018)). I am aware that this review should be focused on the current work, but it is important that your inflow profiles are in balance with the LES model, especially when the goal is look at relatively small differences caused by rotor rotation directions. If the inflow profiles are not in balance, then the wake deflections can also be caused by a downstream development of the inflow. On other hand, an LES inflow is never fully in balance with the 3D domain because of the transient nature of the model. So it could be that your parametrized inflow model is just as good (or bad) as using a traditional LES precursor inflow. It is very good that you have added a few lines in the conclusion, where you discuss the short comings and possible issues with using a parametrized LES inflow model compared to using a non-parametrized LES inflow model.

The solution of the LES inside the area is disturbed by the 2 D inflow profiles of u, v, and w that change over time due to the applied turbulence parametrization. The pressure solver ensures that the solution is in equilibrium. To verify this, I repeated the reference simulation CR, however, applying the inflow perturbations only every twentieth time step. Comparing the residual divergence (domain min, max, average), the difference after 10 min is rather small.

Simulation	div$_{av}$
reference CR	-0.4e-8
20th dt CR	-0.8e-8

3. Section 2: You mention extremely small rotational frequencies of the rotor: 0.058 - 0.23 ° /s, which would correspond to a time of 103 and 26 minutes for a full rotation, respectively. This does make sense to me. Are you sure these numbers are reported correctly? You could also report the corresponding tip speed ratios, which are more common to use in wind energy. A common tip speed ratio of MW-sized wind turbines below rated wind speeds is 7.5, so then you rotor rotational frequency would be 86 ∘ / s for a wind speed of 10 m/s.

Thank you very much for detecting this typo. It should be only 0.058 - 0.23 s^{-1}, which is the number of revolutions per second. We apply 7 revolutions per minute for the reference case, resulting in 0.117 revolutions per second.

4. I think you should clarify the point of view a bit better in the caption of Figures 3 and 4. Instead of This picture is looking downwind on the wake you could use: This picture is looking from upwind towards downwind.

We added it. Further, the way of looking from upwind, downwind on the rotor is explained in Fig. 1 and section 2.3.

5. Figure 7:

You could also normalize the wind speed by the rotor averaged wind speed taken from either the inflow, or a distance upstream of the AD or at the AD location without AD.

As $\overline{u_{inflow}}$ is the same in case of CCR and CR, the plot itself only modifies for part (b) (Fig. 1). We agree, for part (b) the decrease of the difference between clockwise and counterclockwise is better represented by plotting the normalized values instead of absolute ones. However, we decide not to use this plot, as we discuss absolute velocity values in the wake in the paper in the other figures and Fig. 7 is the summary of all other ones. Therefore, the explanation of the differences is more straight forward applying Fig. 7 with absolute values instead of normalized ones and the text is also based on absolute figures.

Should case _ds18 be _ds16?

Thank you! It was a typo, we changed it.

Section 4.3: You could mention that the main influence of wind speed is the wind turbine thrust coefficient C_T through your AD controller. In addition, the shape of the mean inflow profile is dependent on the geostrophic wind speed, which most likely follows a Rossby similarity, see for example van der Laan et al. (2020b). (Without Coriolis and buoyancy related sources terms, your simulations should be Reynolds-number or wind speed independent for a fixed C_T , turbulence intensity and turbulence length scale, see for example van der Laan et al. (2020a)).

We see your argument. Our attempt with increasing the wind speed, however, was to change the atmospheric inflow. Therefor, we decide not to include this comment.

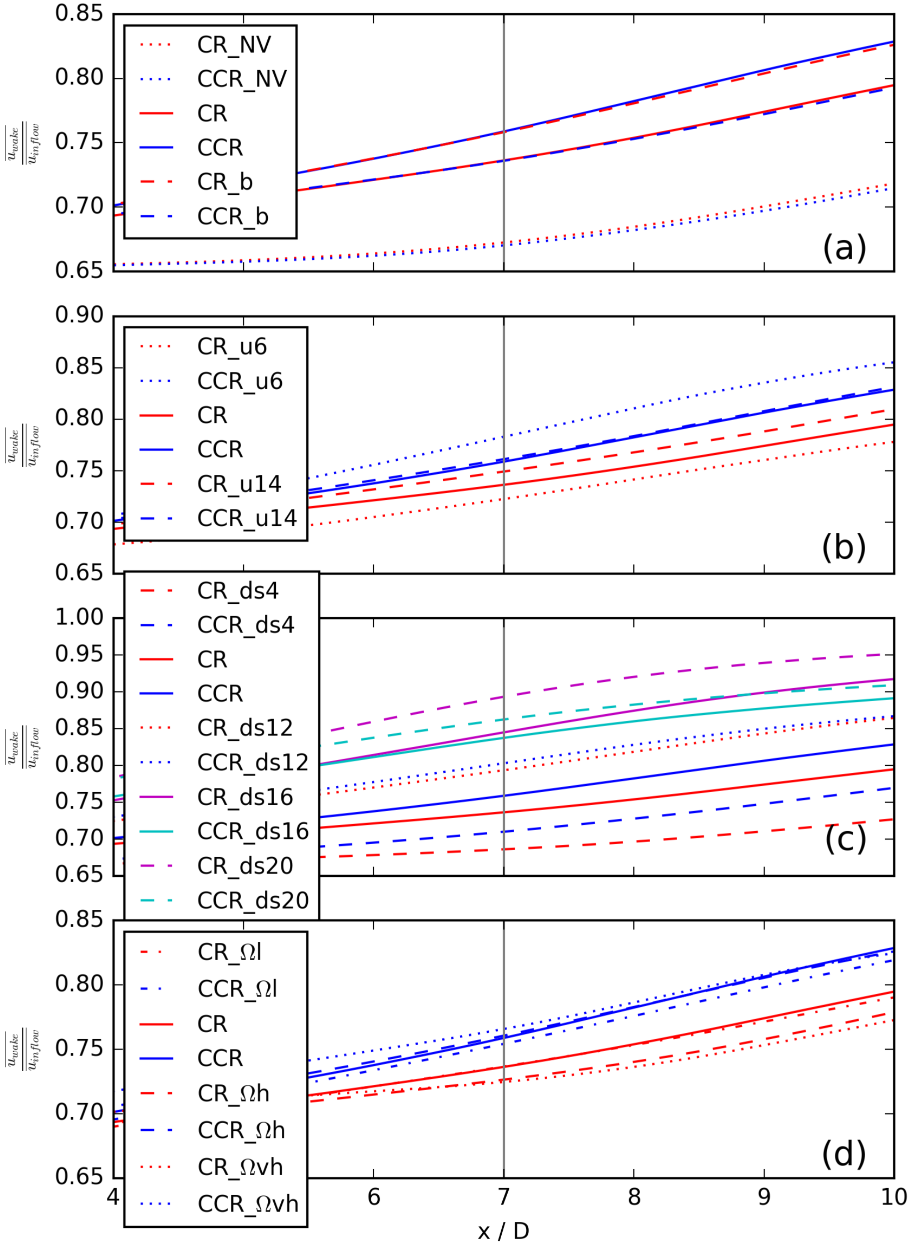

Figure 1. The rotor and time averaged streamwise velocity $\overline{u_{A_{wake}}}$ normalized by the rotor and time averaged streamwise velocity at the inflow $\overline{u_{A_{inflow}}}$ presented for a downwind region of $[4\,D;10\,D]$ with special emphasis at $x = 7$ D for the simulations CR_NV, CCR_NV, CR, CCR, CR_b and CCR_b in (a), for different geostrophic wind values in (b), for different directional shears in (c), and for different rotational frequencies in (d).

References

Englberger, A., Dörnbrack, A., and Lundquist, J. K.: Does the rotational direction of a wind turbine impact the wake in a stably stratified atmospheric boundary layer?, Wind Energy Science Discussions, 2019, 1–24, https://doi.org/10.5194/wes-2019-45, https://www.wind-energ-sci-discuss.net/wes-2019-45/, 2019.

Comments on the Review of Changing the rotational direction of a wind turbine under veering inflow: A parameter study - Reviewer 2

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Dear Reviewer 2,

Thank you for taking the time to carefully review our completely modified version of the paper. We are happy to hear that you think it is a very compelling research question. We read your review in detail. Here is our response to your comments:

Minor comments

In Eq. (15) and (16) the , and in Eq. (17) x WT and and x are not introduced.

Thank you for noticing. We included the explanations.

Page 8, lines 2-3: An addition to this sentence, that the simple model demonstrates the principle interactions between an idealized wake represented by a vortex and several inflow configurations, and that it will be compared with previously introduced LES at a later point would inform the reader on the section's intention.

We added your first suggestion in: 'In this section, we construct a simple analytical model of the interaction of the rotating wake of a wind turbine with a sheared ambient flow. The rotating wake is prescribed by a Rankine vortex. The ambient flow is described by three different inflow conditions (no veer, veering wind, backing wind). In the case of a veering inflow, the relations are also evaluated for three different parameters (wind speed, directional shear, rotational velocity).'

Your second suggestion is listed in the end of the introduction giving the main overview of the idea of the manuscript. Therefore, we did not list it in section 3.

Page 12, lines 7-10 (also relevant for abstract and conclusions): The term wake deflection is also often used to describe a horizontal displacement of the whole wake in case of a wind turbine operating with a yaw offset. Here, wake deflection is used to describe the displacement of a part of the wake relative to the wind direction at hub height (other literature coined this a skewed wake – e.g. Abkar and Porté-Agel, 2016). A sentence that clarifies the usage of wake deflection in this paper or changing the term could avoid possible misunderstandings.

We use skewed wake structure for describing the wake structure difference in the y-z plane between veering and no-veering inflow in Fig. 5. Applying 'wake deflection to the left/right', we think it is more telling in comparison to skewed wake. However, we understand your point to get confused with yaw offset. Therefore, we added an explanation the first time we use wake deflection in the text. '... wind veer causes wake deflection in relation to a vertical plan through the nacelle at $y = 0$ ...'. Now the meaning of wake deflection in this manuscript should be clear.

Page 19, lines 4-10: It might be beyond the scope of this parameter study, but for possible future studies, it would be interesting to investigate if the increased $\overline{u_A}$ for a veering wind and a CCR rotor holds for all possible locations of a hypothetical downstream turbine. The one-sided minima of the streamwise velocity in Fig. 6(b) and Fig. 6(d) that is just outside of the rotor area might (or might not) be canceling the positive effect on $\overline{u_A}$ and that could provide insights into the robustness of possible improvement for a hypothetical downstream turbine.

We get a similar comment from Reviewer 1 on the first manuscript version. In the response to him we added a plot (Fig. 1) investigating this. However, as the manuscript is already very long, we decide to not include this discussion in the manuscript. But we will refer to it in subsequent work.

Considering the horizontal profiles in the lower and the upper rotor half at $z = 75$ m and at $z = 125$ m in Figs. 5, 6, and 7, the wake is deflected in the lower rotor part towards the left (right) and in the upper rotor part towards the right (left) in case of veering (backing) inflow. As the lateral wake position depends on the inflow wind angle, the spanwise wake position approaches away from $y = 0$ D for increasing directional shear. This is presented in Fig. 1 (here). In case of no wind veer (Fig. 1(b)), there is no difference. In case of veering inflow (Fig. 1(e)), at $y = \text{-}1/2$ D, there is a small rotational direction difference in the bottom rotor part, and at $y = 1/2$ D, there is a difference in the top rotor part. The top left ($y < 0$) and the bottom right rotor parts are unaffected by the rotational direction, as there is no wake in these sectors. In case of a backing wind (Fig. 1(h)) the situation is the opposite. In case of a veering inflow, increasing the geostropic wind (Fig. 1(c)) or the directional shear (Fig. 1(f)) increases the difference in $\overline{u}$, especially in the top right rotor part. The same is valid for an increase of the rotational frequency (Fig. 1(i)). Decreasing the atmospheric or vortex strength, the difference decreases. Therefore, there is an impact at $y = 0$ D and likewise in the wake affected sectors to the right or the left. In the considered idealized simulations of this work, the impact on $\overline{u}$ has therefore the same tendency in case of staggered or unstaggered arrangements of the hypothetical downwind turbines.

Page 32, lines 32: A similar comparison to Fig. 19 for the left/right sectors from Fig. 1 would be expected at this point. If nothing interesting was learned from it, it could be mentioned in a short sentence.

The right/left comparison plot (Fig. 2 (here) does not provide any new insight. We mention the much less distinct difference between clockwise and counterclockwise rotating actuators for the left and right sectors in comparison to the top and bottom ones.

Fig. 20: The schematic illustration seem to be representative of a height away from hub height due to the wake deflection. Is it correct to assume that a similar schematic illustration for the hub height ¸would have ϵ and $\Delta\epsilon$ equal to zero and the two ΔL_y would be symmetric?

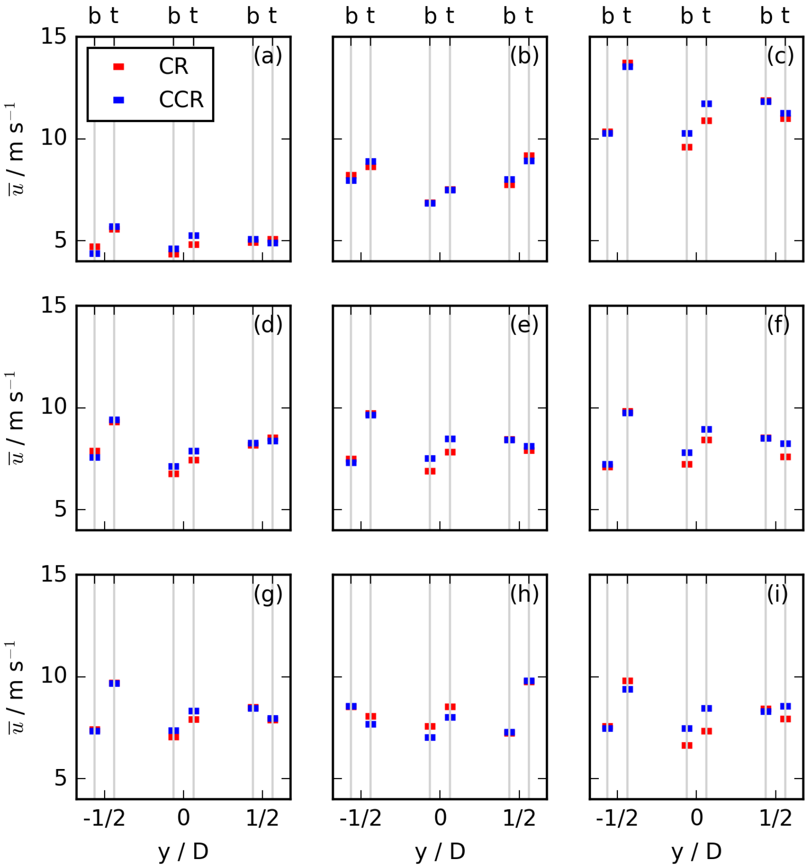

Figure 1. Sector averages of $\overline{u}$ representing the top and bottom 90°-sectors for 0 m $<$ r $\leq$ 50 m for clockwise and counterclockwise rotating actuators for the same simulations as in Fig. 19 of the manuscript at $y = 0\,$D and in addition shifted by D/2 in both lateral directions. The indices 'b' and 't' at the top x-axis represent the corresponding bottom or top sectors.

At hub height, $\epsilon = \Delta\epsilon = 0$, however, $\Delta L_y \neq 0$ all over the rotor height. L_y of the counterclockwise rotating rotor will be larger in comparison to L_y of a clockwise rotating rotor also at hub height (see Fig. 9c vs. f). We added this in the caption of Fig. 20.

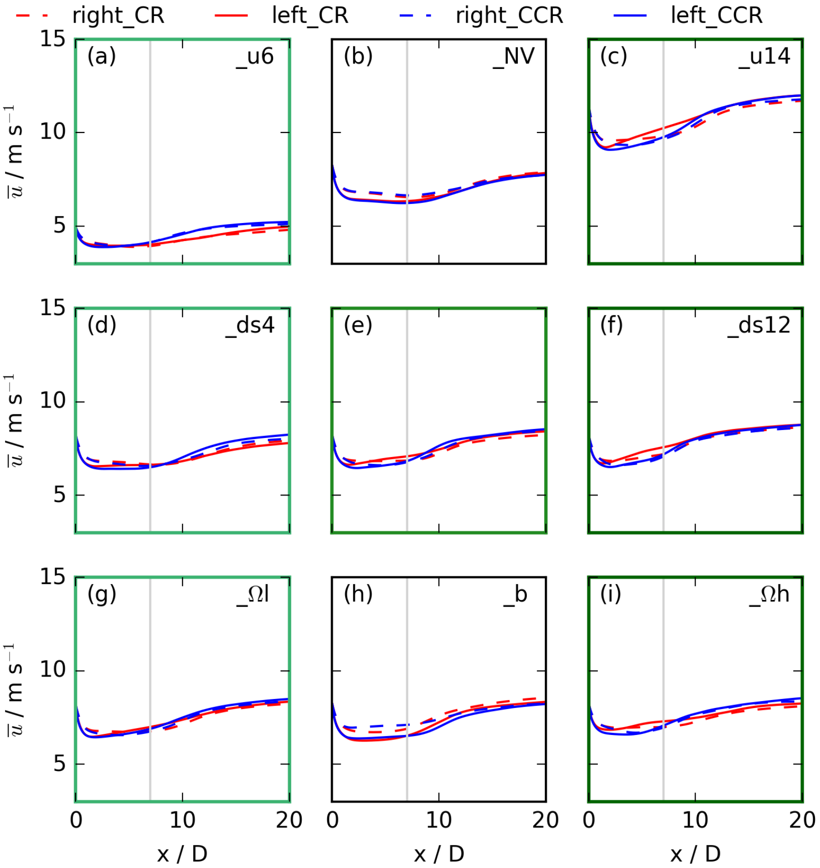

Figure 2. Sector averages of $\overline{u}$ representing the left and right 90°-sectors for 0 m $<$ r $\leq$ 50 m for clockwise and counterclockwise rotating actuators for the same simulations as in Fig. 18 (manuscript).