Comments on the Review of Should wind turbines rotate in the opposite direction? - Reviewer 1

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Dear Dr. M. Paul van der Laan,

Thank you for taking the time to carefully review our paper. We read your review in detail and appreciate you sharing your own simulation results. Regarding your comments, we think there are several misunderstandings with the first version of the paper. Therefore, with the help of your comments, we performed some far-reaching changes to the manuscript. Here is a list of the major changes.

- We changed the title.
- We explained in detail the turbulence generation method we applied in the simulations.
- We included a section, introducing a simple analytical model predicting the expected changes in the spanwise velocity field in the wake by a superposition of a veering inflow with a Rankine vortex. (New section 3)
- We added additional simulations with different directional shears (including the $0.12^\circ\ \mathrm{m}^{-1}$ value you applied in your simulation).
- We investigated the impact of the rotational frequency on the wake differences.
- We added additional plots, explaining the wake differences and its occurrence for different rotational direction of the actuator.
- We added a section comparing the numerical results predictions of the analytical model. This section explains in detail the source of the difference in the wakes between a clockwise and a counterclockwise rotating rotor in case of a veering inflow.
- We added an Appendix, verifying the application of the turbulence preserving method for this theoretical and idealized parameter study.

In the following we respond in detail to each of your comment/question.

The authors employ large-eddy simulations (LES) of a single actuator disk subjected to different stable atmospheric boundary layers to investigate the impact of the rotational direction on the potential downstream wind turbine power. The article has an interesting topic and I think it is worth publishing an in depth study about it.

We are pleased you think it is an interesting topic and it is worth publishing a detailed study about it. When we received the replies from the reviewers, we realized that we had insufficiently introduced the fairly new topic. The differences were only described and not thoroughly explained. We have corrected this oversight in the revised version.

However, I have four main concerns with this work.

First of all, the inflow is not a solution of the LES model, but simply set as an initial condition without any inflow turbulence.

This was a misunderstanding: In the paper we stated:'A turbulent stably stratified regime in our wind-turbine simulations performed with open horizontal boundary conditions is verified by applying the parametrization of Englberger and Dörnbrack (2018). All parameters required to apply the parametrization are described in detail in Englberger and Dörnbrack (2018).' Instead, we should have explained the turbulent inflow in more detail rather than simply referring to a previous paper where this LES spin-up and inflow turbulence has been developed and successfully validated.

In the modified version we emphasized more clearly that inflow turbulence is applied on the leftmost boundary as a 2D slice at every time step. The inflow turbulence results from the turbulence parametrization of Englberger and Dörnbrack (2018), including turbulent fluctuations retrieved from a neutral boundary layer precursor simulation (Englberger and Dörnbrack, 2017) in combination with adjustable stratification-dependent parameters. In the modified version we explained this impression of turbulence on the inflow in detail (also adding the corresponding equation). Further we verify its applicability for this investigation. In the appendix we further show that the occurrence of a difference between a clockwise and a counterclockwise rotating actuator does not depend on the applied turbulence intensity, only that the degree of the differences is modified by the turbulent intensity. The difference in the flow pattern (amplification of spanwise flow in case of counterclockwise rotating rotor and weakening/reversion in case of clockwise rotating rotor under veering inflow in the NH) only depends on the mean inflow profile and the vortex component of the wind turbine.

In addition, the applied inflow turbulence impacts the wake recovery and the resulting velocity deficit of $\approx$0.45 at a downstream distance of 7 D. If these simulations had laminar inflow, as we think your comment suggests, the wake would persist much longer.

Secondly, the methodology of quantifying the impact of the rotational direction of the rotor on a downstream wind turbine is not sufficient and the reported gains in power are misleading because they only reflect a few specific cases that are rare with respect to all the flow cases that are typically present in an annual energy production calculation of a wind farm.

The reviewer identifies two issues: the first concerning the methodology and second that these impacts are rare. Regarding the first issue, we changed the selection of considered cases in comparison to the previous version. Further we introduce the expected results by simple analysis (superposition of spanwise component of veering inflow with Rankine vortex) in a new Section 3 and compared the simulation results to the analytical expectations (new Section 5), to make the manuscript more consistent. In the revised version, we considered cases of changing the atmospheric inflow (geostrophic wind, directional shear) and the rotational frequency of the rotor. All other simulation results from the previous manuscript version are eliminated to be more consistent. With including the expected results in the analysis section (3) and comparing the simulation results to them (section 5), we hope we have proven that our methodology is now sufficient and consistent.

Regarding the second issue, we agree with the reviewer that we specifically only considered an inflow from west to east in the northern hemispheric mid-latitudes 270° at hub height. Because this is an idealized study attempting to understand if this effect is significant in any case, we focused on this idealized inflow scenario and varied the impact factors (geostrophic wind, directional shear, rotational frequency) to investigate the impact of each of them on the difference in the wake structure between clockwise and counterclockwise rotating actuators.

It is quite common for idealized studies to focus on specific wind conditions to understand specific phenomena, and here we explore the difference of the rotational direction impact on the wake under veering inflow conditions. We certainly do not claim to address all relevant flow cases for the annual energy production calculation in a wind farm. The considered cases in our study (regarding the geostrophic wind speed and the directional shear) are chosen from measurement papers like Walter et al. (2009), Sanchez Gomez and Lundquist (2020), **?**, and Bodini et al. (2020). Of course, the measurement results are location specific. We considered three different measurement campaigns, including offshore measurements e.g. 13 months of lidar measurements in Massachusetts in (Bodini et al., 2020), as well as onshore measurements e.g. covering 3 months of lidar observations in north-central Iowa in Sanchez Gomez and Lundquist (2020) and two years of meteorological tower observations in Lubbock (Texas) in Walter et al. (2009). From these measurements we extracted the frequency of occurrence of veering vs. backing and likewise the frequency of occurrence of specific wind speeds and directional shears. In the introduction, we added a paragraph pointing out that our values are chosen in relation to these three measurement campaigns and that the percentage of occurrence of veering or specific wind speed or directional shear values is location dependent.

Maybe the reviewers reaction is related to the simple title of the study, 'Should wind turbines rotate in the opposite direction?'. This question was chosen as title for the paper as it is simple and interesting and for motivation to consider this issue. But we agree with the reviewer that our paper cannot give an answer to this question considering all relevant cases in a wind farm over a year or at any location on earth. Therefore, we change the title of our manuscript to 'Changing the rotational direction of a wind turbine under veering inflow: A parameter study.'

Thirdly, the information provided in the article is not sufficient to redo the simulations and understand the presented results.

Thank you for the comment. We listed all data to the best of our knowledge in the previous manuscript:

- $512 \times 64 \times 64$ grid points
- horizontal and vertical resolution of 5 m
- open horizontal boundaries

- 40 min simulation time
- $D = z_h = 100$ m
- inflow profiles of u, v, w, θ in Eqs. 4, 5, 7, 8
- wind veer profile in Eq. 6
- BEM with scaled wind turbine (here we refer to Englberger and Dörnbrack (2017, parametrization B))

A detailed listing of all main properties of the simulations are given in Table 1 (wind speed, directional shear both determining the mean inflow wind field and the rotational frequency determining the strength of the vortex)
For more complex simulation inputs (wind-turbine parametrization, turbulence preserving method), we referred to previous papers where all details are listed and the method are validated and explained:

- the wind-turbine parametrization including rotor properties
  'A detailed description of the wind-turbine parametrization and the applied smearing of the forces, as well as all values used in the blade parametrization are given in (Englberger and Dörnbrack, 2017, parametrization B).' (See Table 5 of Englberger and Dörnbrack (2017))
- the turbulence preserving method
  'A turbulent stably stratified regime in our wind-turbine simulations performed with open horizontal boundary conditions is verified by applying the parametrization of Englberger and Dörnbrack (2018). All parameters required to apply the parametrization are described in detail in Englberger and Dörnbrack (2018).' (See Table 1 of Englberger and Dörnbrack (2018))

In the revised version we included the following additional information:

Regarding the wind-turbine parametrization we extend the explanation, but only concisely. For more details about the parameters and applied calculation of F_{WT} from Eq. 1 we refer to Englberger and Dörnbrack (2017, parametrization B). However, we added now the very simple analytical equation showing the same effect of amplification or reduction/reversion of the spanwise wake component. Therefore, the occurrence of the effect does not depend on the turbine type, rotor diameter, radial distribution of the forces. But of course they impact the strength of the effect. As this is a parameter study, not referring to a specific wind turbine, location etc. we did not change our wind-turbine. However, we include the turbine impact by changing the rotational frequency of the rotor to show the sensitivity to the strength of the vortex in the analytical section 3 and also the numerical simulation section 4.

Regarding the turbulence preserving method, we added a much more detailed description (see comment above).

Finally, I disagree with the main conclusion. I provided an Appendix where I have performed Reynolds-averaged Navier-Stokes simulations of two NREL-5MW wind turbines with 7D spacing subjected to an atmospheric inflow with a strong wind veer. I also see a relatively large impact of the rotation direction on the power output of the downstream wind turbine for a specific wind direction. However, my simulations suggest the opposite of the present paper, where a clockwise rotating wind turbine in the Northern Hemisphere performs better than a counter-clockwise rotating wind turbine (subjected to a strong wind veer).

We really appreciate your effort of performing the simulations and your generosity in sharing your results. However, we do not agree with your statement that your results disagree with ours. We performed simulations with a veering over the rotor of $0.04^\circ\ \mathrm{m}^{-1}$, $0.08^\circ\ \mathrm{m}^{-1}$ and $0.16^\circ\ \mathrm{m}^{-1}$. According to our simulations, a counterclockwise rotating rotor results in a higher downstream velocity at 7 D in case of $0.04^\circ\ \mathrm{m}^{-1}$ and $0.08^\circ\ \mathrm{m}^{-1}$. In case of $0.16^\circ\ \mathrm{m}^{-1}$ a clockwise rotating wake has a higher downstream velocity at 7 D in comparison to a counterclockwise rotating one. Your simulation investigated it for a directional shear of $0.12^\circ\ \mathrm{m}^{-1}$ over the rotor with the same result as our $0.16^\circ\ \mathrm{m}^{-1}$ simulation. Therefore, we think your simulation results did not disagree with our results for the strong wind veer case.

To focus on the impact of the directional shear, we added a simulation with a directional shear of $0.12^\circ\ \mathrm{m}^{-1}$, corresponding to the directional shear you applied in your simulation, and likewise a simulation with a very high directional shear of $0.20^\circ\ \mathrm{m}^{-1}$ to point out the impact of the directional shear on the results. Please see Fig. 7, 10, 11, 12, and 13 in the revised version representing the results. According to our results, for low values of the directional shear ($0.04^\circ\ \mathrm{m}^{-1}$) the rotor and time averaged downwind velocity $\overline{u_A}$ at 7 D is larger for a counterclockwise rotational direction in comparison to a clockwise one. For very high values of the directional shear ($0.20^\circ\ \mathrm{m}^{-1}$) the opposite is the case. In between, there is a directional shear values with no difference in $\overline{u_A}$ between clockwise and counterclockwise rotating actuators. According to our results (and thanks to the added simulations in the new manuscript), this is the case for a critical directional shear value ds_c with $0.12^\circ\ \mathrm{m}^{-1} < ds_c < 0.16^\circ\ \mathrm{m}^{-1}$. The specific value of ds_c of course depends on the turbulent intensity, the rotor diameter, the radial distribution of the forces, the wind-turbine type, the resolution, etc. But regarding the result of ds_c very close to your result with a directional shear of $0.12^\circ\ \mathrm{m}^{-1}$, the deviation is not unexpected for us. Especially regarding the main difference, you considered two wind turbines and we consider the available power in the wind. But also further differences, smaller geostrophic wind, different size of the WT, different radial distribution of the forces, different turbulence applied as inflow condition. Therefore, your results did not show a different result of the complete rotational direction under veering inflow topic. On the contrary, we think it supports our results. It also supports our assumption that the difference is related to the mean inflow fields as predicted by the analysis, as your result is very similar to ours despite all the differences in the atmospheric conditions (different geostrophic wind, turbulence method) and the wind turbine (rotor size, radial distribution of the forces, different rotational frequency).

This is because the initial horizontal wake deflection for clockwise rotating wind turbine (without the effect of wind veer but including a wind shear) is clockwise (as seen from above). The counter-clockwise rotating wake brings fresh momentum from above towards the right side of the wind turbine, which results in a stronger deficit on the left side, and this causes the wake to deflect clockwise at seen from above, as shown by Zahle and Sørensen (2008). The addition of wind veer in the Northern Hemisphere deflects the wake even more clockwise, which is also shown in van der Laan and Sørensen (2017).

We agree with your comments here, but they are valid in case of no veering wind, as you stated. We also apply this explanation in the work Englberger et al. (2019), explaining the difference between clockwise and counterclockwise rotating actuators in the evening boundary layer in case of no wind veer. Regarding the wake deflection in dependence of the rotational direction of the rotor in combination with veering inflow, our results show that the wake deflection is larger in case of a counterclockwise rotating disc interacting with veering inflow, independent of the atmospheric parameters directional shear and wind speed. We explain this with the amplification of the spanwise flow component in the wake in case of a counterclockwise rotating rotor, resulting in a larger wake deflection in comparison to the weakening or even reversion which occurs in case of a clockwise rotating actuator.

Hence, I disagree with the authors conclusion. I have written a list of main and minor comments below. Since there are so many major concerns, I am afraid that I have to reject the article.

Regarding all the misunderstandings (insufficient description of turbulence generation method instead only referring to the corresponding paper, misleading title in a way we did not anticipate, misinterpretation of your simulation results with our results due to application of a directional shear value with is close to the critical value we detect in our results) we understand your recommendation to reject the previous version of this article. Your comments were really helpful to us to eliminate the misunderstandings via including a detailed description of the turbulence generation method applied, changing the title of the manuscript, adding additional simulations helping to narrow down the critical values of the directional shear, including the simple analytical equation which explained the differences seen in the simulation. Considering the extensive revisions in the presentation of the results we performed in this revised version, we hope we have addressed your concerns.

In the following we refer to your main comments:

1. Why do use a scaled down version of the DTU-10MW wind turbine? Wouldn't it be easier to either use the NREL-5MW wind turbine or the original DTU-10MW wind turbine (which is an upscaled version of the NREL-5MW wind turbine)? These reference wind turbines are made to make a comparison between scientific literature in wind energy more fair, and using a reference wind turbine allows other researchers to redo your simulations more easily.

We understand your point here. Our attempt was to apply the flow field modifications of a generalized wind turbine with a rotor diameter as well as a hub height of 100 m. As it is a parameter study, it is not related to a specific turbine, location, etc. In the revised version we also add the analytical model, which explains the difference and also that they are not turbine dependent (also occurring for a rather simple Rankine vortex).

Page 4, Line 26: Here you mention that you set different magnitudes of wind veer over the rotor area. How do you set these magnitudes? It seems that you specify them according to an initial profile from Eq. (6), without changing the physical parameters that actually influence the wind veer, i.e. the Coriolis parameter or geostrophic wind (for a constant inversion strength and atmospheric stability). If this is the case then all simulations will converge to the same wind veer if you run them long enough unless you have periodic conditions on all four lateral boundaries.

Here is a misunderstanding. We do not prescribe specific inflow profiles in a precursor simulation extending it until it reaches an equilibrium state, therefore, our results will not converge to the same wind veer. In EULAG, we apply the background/environmental wind profiles $u_e(z)$ and $v_e(z)$, without Coriolis force in the simulation, and superposed the turbulence on the inflow. This turbulent inflow wind field interacts with the actuator. The chosen background wind profiles determine if there is a veering or a backing wind or no wind veer at all or in case of a veering inflow the also determine the wind speed and directional shear in the simulations.

You never mention the word precursor or which boundary conditions you use, so it is unclear to me how you make sure that inflow has reached a quasi steady-state before you apply the inflow to a wind turbine wake simulation. If you do not use a precursor simulation, then the inflow will develop downstream and you cannot isolate the wake effects from the imbalance of the inflow profile. If you do use a precursor simulation for each case, then please specify all the input parameters necessary to run each case.

We should have explained the turbulence generation method in more detail. We changed this part of the paper (see general statement). We also added the stratification-dependent parameters applied in the simulations. In the revised version it says ... 'of a neutral boundary layer precursor simulation ...'. To make sure that our simulations reach steady state, we extended the simulation to 40 min, now averaging over 30 min. For the reference simulation we tested a simulation time of 1.5 h with the same result as averaging over the last 30 min.

In addition, it would make sense to plot all inflow profiles and report the wind speed and turbulence intensity (for example based on the turbulent kinetic energy) at hub height.

We thought about the comment of plotting the profiles, however, we did not include the plots basically due to three reasons: Firstly, they are idealized profiles following Eqs. 6 (streamwise component) and 8 (spanwise component). Secondly, the profiles referring to the turbulence generation method are already discussed and shown in Englberger and Dörnbrack (2018). Thirdly, the modified version of the manuscript already includes 20 figures considering the discussion of the results (as requested by the reviewers), therefore, this plot was eliminated in the end.

Furthermore, you seem to use a laminar inflow (without a roughness length), which does not make sense when modeling a wind turbine wake subjected to an atmospheric inflow.

If we understand this comment correctly, you refer to a roughness length z_0 in the inflow profile? In EULAG, we do not apply a MOST surface layer parametrization.

3. Eq. (9): What is η_{mech} ? If you intend to calculate the electric (hypothetical) power from the mechanical (hypothetical) power, one would expect to have a 6% loss for a modern wind turbine, not 36%. In addition, it is unclear where the power in the other two dimensions are evaluated (y and z), you only mention the downstream distance. Furthermore, I would expect to the power to scale with U 3 and I would take the integral of U 3 over the rotor area. Please clarify. In addition, if you are not considering a second wind turbine, you are ignoring upstream effects of the downstream wind turbine. It is worth while to mention this simplification.

Our very simple power calculation is basically proportional to u^3 (sorry we missed the 3 in Eq. 9) should represent the power available in the flow which could be extracted from a downwind turbine. And the important information is only the difference of P in percent, as it is a parameter study. Our intent with this was only to give a comparison also in case of power not only of m/s, however, it was very missleading instead of helpful and the information in % could likewise be calculated from u directly. Therefore, we excluded the power completely from our revised manuscript. In the revised version we only refer to a spanwise and streamwise velocity difference between clockwise and counterclockwise rotating simulations.

4. It would be nice to report the tip speed ratio, the thrust coefficient and the power coefficient, for each case. This information is necessary to replicate your simulations.

Our representation of the wind turbine parametrization does not rely on thrust or power coefficients but rather lift and drag coefficients are applied in the calculation of the wind-turbine forces, as explained in Englberger and Dörnbrack (2017). We recognize the importance a reader is able to replicate the simulations. For reasons of space and because it is a very long manuscript anyway, we do not include all wind-turbine values applied in the BEM method. Instead, for all other wind-turbine parameters we refer to Englberger and Dörnbrack (2017). In addition, in the revised version we included the rotational frequency of each individual simulation in Table 1.

5. Table 1: This table is confusing. Why do you have both clockwise and counter-clockwise at the same row? I also get confused with the amount of cases and labeling. You also use these labels all the time in the article and it makes it hard to follow the text. Couldn't you simplify the cases and pick those that are really important for your conclusions? The intention was to read it as 'Simulation with different rotational directions of the rotor' 'clockwise' vs. 'counterclockwise' (all in capital letters and bold). This should save us one additional column stating the rotational direction and we think it is applicable as all other parameters are the same in the clockwise as well as the counterclockwise rotational direction simulations referring to one case. In the modified version we explained this in the table caption.

Regarding your comment with the labelling, we changed it to make it more intuitive with _ds for directional shear, _u for geostrophic wind speed, and _Ω for rotational frequency with the corresponding figures following for ds and u and values for low, high and very high in case of the rotational frequency.

Following your comment, we simplified the cases and now we only consider three parameters: u_g, ds, Ω

6. Table 1: How is it possible to get a negative and positive wind veer for the same Coriolis parameter? This seems to me that your inflow profile is not in balance with your equations because these kind of effects are typically caused by (unsteady) meso-scale phenomena, which you are not modelling, as far I can understand.

Veering or backing is defined by the inflow profile. To make it more clear that veering or backing is prescribed by the background flow field, not resulting from a precursor simulation, we excluded the Coriolis force from the simulations. The Coriolis force is only relevant for determining the mean inflow profiles (veering or backing inflow) but not its interaction with the wake is leading to the differences (this was another misinterpretation of the results).

7. I would expect the wake deflects differently for different rotor rotation directions or the wake deflects simply more for a certain rotor direction. Is this correct? It might be worthwhile to discuss and show this (for example with a wake center tracking method). In addition, if you had a downstream wind turbine in unfavourable staggered position, then the additional wake deflection could reduce the power of the downstream wind turbine.

Yes exactly. Our simulation results show that the wake deflects more in case of a counterclockwise rotating rotor operating in veering inflow (or a clockwise rotating one in backing inflow). We added horizontal lines of the streamwise velocity in the lower and the upper rotor part as well as at hub height. See Figs. 6, 13 and 17. They allow a quantitative evaluation of the differences in the wake deflection angle between counterclockwise and clockwise as it was possible from the contour plots in the original manuscript version.

We thought about your comment of unfavourable positions of a downwind turbine. In the revised version of the manuscript we elevated the 90° sector and time averaged grid points with and 0 m$<$ r $\leq$ R for the top and the bottom sector directly behind the wind turbine. Than we extracted the information at 7 D and added the same information at a spanwise distance of $y = 1/2$ D and -1/2 D at $x = 7$ D. The results are not presented in the new manuscript (basically as it is an extension and not directly related to the analysis section), however, here we would like to show you the plot in Fig. 1. 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 $y =$ -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.

8. Figure 1, why not just plot wake deficit profiles as function of the cross coordinate at different downstream distances? Now you are only looking directly behind the wind turbine, while the wake has deflected laterally (and possible vertically as well), so you are missing a lot of important information.

This is answered in point 7. in detail.

9. The streamwise velocity contour plots presented of Figures 3, 5 and 6, do not seem to resemble converged statistics. If you have converged statistics, then I expect smooth plots, see for example the low turbulence intensity case in van der Laan

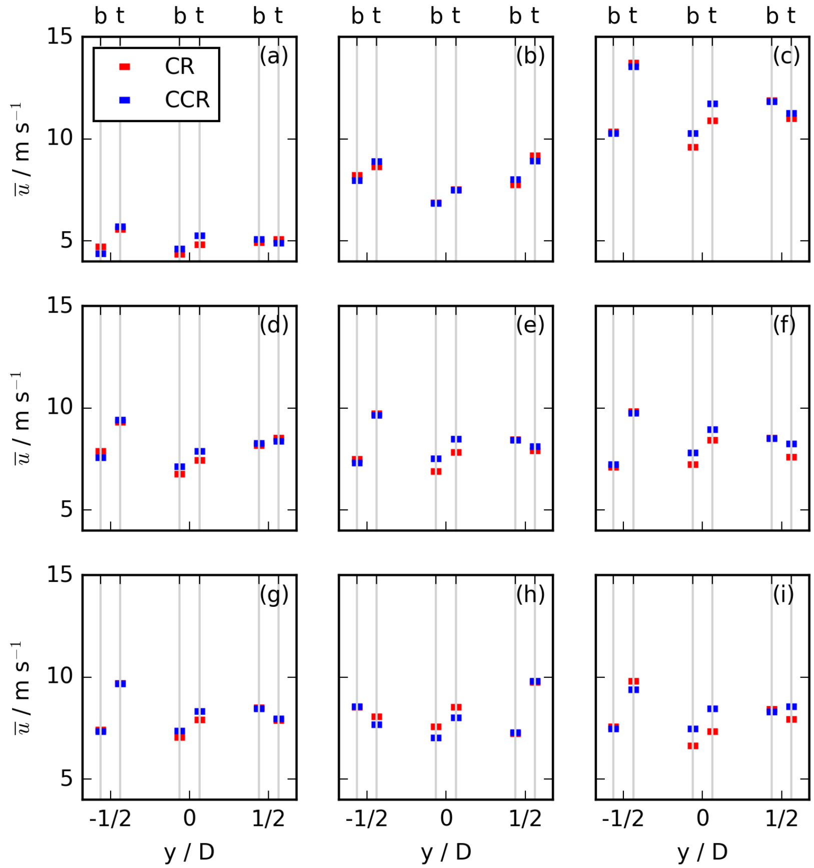

Figure 1. Sector averages of $\overline{u}$ representing the top and bottom 90°-sectors for 0 m < r ≤ 50 m for clockwise and counterclockwise rotating actuators for the same simulations as in Fig. 19 of the manuscript at $y = 0$ D and in addition shifted by D/2 in both lateral directions. The indices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

and Andersen (2018), where 1 hour LES results are presented. This could indicate that your LES data set is not large or long enough, or your simulation has not converge to a (quasi) steady-state, but keeps changing instead.

- We extended the reference simulation to 1.5 h and the wake structure did not change.

- In Englberger et al. (2019), simulations with both rotational directions are conducted with the inflow resulting from the stable regime of a diurnal cycle precursor simulation. In these simulations the spanwise flow component was 8 times it is chosen for this parameter study and the effect did not occur. Therefore, we refer the wake structure to the domain size. We cannot reproduce the simulations in this work on a larger spanwise domain as the applied NBL precursor simulation in the turbulence generation method limits the domain size.

– The effect not only occurs for counterclockwise rotating rotors. This is supposed to be the case as the spanwise inflow velocity is amplified in the wake. Considering a geostrophic wind of 6 m s^{-1} in CR_u6 it also occurs. Here, the spanwise flow component decreases in comparison to the reference case, and likewise the streamwise component. Therefore, the effect seems to be additionally influenced by the streamwise wind speed.

10. Conclusion and abstract: You have to mention that the simulated power increase of 23% only reflects a specific wind direction. In other words, if you would consider multiple wind directions, then the impact of rotor rotation direction on the power (deficit) is much smaller than you report.
We modified the introduction and listed in detail that

– This difference occurs only at night.

– There are seasonal differences.

– The percentage of occurrence is location dependent

– The occurrence of specific directional shears and wind speeds is also location and also seasonal dependent.

we modified the conclusion by: This is only an idealized parameter study (turbulence is not location sensitive or result from an SBL precursor simulation). The results are not valid everywhere.

In addition, if a full wind farm is considered, I expect that the effect of rotation direction is reduced further downstream in the wind farm because of an increase in turbulence level.
Further we added a common on wind farms: 'We have assessed the wake of an individual turbine, but these results could be extended to a large farm in which the presence of upwind turbines could affect turbulence intensity, which probably affects the magnitude.'

Finally, if one would look at the effect of the rotor rotation on the wind farm annual energy production, which also consists of many flow cases, where rotor rotation has no influence, you might find that the effect of rotor rotation direction is far less than 1%. Such the study would be necessary in order to answer the question raised in the title. If your title is Should wind turbines rotate in the opposite direction? then I expect to find a thorough answer in the article. The presented simulations cannot answer this question because we need an estimate of the rotor rotation direction on the annual energy production. Regarding your comment on wind direction, there is a misunderstanding. We do not simulate a specific wind direction (for a specific location). We only simulate different directional shear values in an idealized simulation set-up. But we agree that a wind direction change occurs mainly at night and a veering wind only represents a certain precentage of this nights. (See listed modifications of the introduction.) Further, we agree with your comment on the title and changed it as explained in the general comments.

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