Comments on the Review of Should wind turbines rotate in the opposite direction? - Reviewer 2

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Dear Reviewer 2,

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 (especially your comments asking what exactly contributes to higher/smaller $\overline{u_A}$ values), 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.

– 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.

General Comments

The research question of the article is interesting and well-motivated. I cannot comment on the technical set-up of the LES and the turbine model, as I have no experience with modeling, but some of the chosen simulation parameters seem questionable to me. There are several issues with the results:

(i) A presentation of the wake structure away from the hub height is missing.

We added several figures covering this. Contour plots representing the top rotor half at $z = 125$ m and also the bottom rotor half at $z = 75$ m (Fig. 4, 9, 11, 12, 15, 16). We also included a y-z contour plot representing the difference in the wake skewing between clockwise and counterclockwise rotating actuators (Fig. 5). Further, for a quantitative comparison we added vertical and spanwise profiles of the streamwise velocity (Fig. 6, 13, 17).

(ii) The exclusive focus on the mean streamwise velocity ignoring other quantities that affect a downstream turbine (I am not counting the power as a separate quantity here due to way it is computed).

We eliminated the power from the paper, as is leads to many misunderstandings.

In a recently submitted revised version of a previous paper Englberger et al. (2019) (attached) we apply the inflow conditions from a stable regime from a diurnal cycle precursor simulation. In that work we also focus on the turbulence in addition to the velocity components.

This work, however, is a parameter study with a very simplified setup of the numerical simulations. The applied turbulence is based on a turbulence generation method from Englberger and Dörnbrack (2018b), applying the turbulent perturbations of a neutral boundary layer precursor simulation (Englberger and Dörnbrack, 2017) in combination with adjustable-stratification dependent parameters resulting from this stable regime of Englberger and Dörnbrack (2018a), which is applied directly in Englberger et al. (2019). We apply this turbulence generation method as it provides a computationally fast testbed for wind-turbine simulations with open horizontal boundary conditions on a small domain and it also includes atmospheric characteristics in the inflow (not only random perturbation). This allows us to produces the large number of simulations in this work. We consider this method appropriate, as the occurrence of the differences between clockwise and counterclockwise rotating turbines results from the veering inflow and only the degree of the differences is modified by the turbulent intensity applied (see Appendix). Therefore, we only show velocities in the manuscript.

In the revised version, however, we also included the spanwise and vertical velocity. Further, we show vertical and horizontal profiles at different heights over the rotor of the streamwise velocity, not only the rotor averaged value as in the original manuscript version.

(iii) No physical explanation is given how the stronger rotation of the wake causes the higher entrainment, which is provided as reason for the main finding.

This is given in Englberger et al. (2019), where the turbulence profiles are presented. Here, due to the limitations of this work as listed above, it is not shown.

(iv) I am not convinced that the increased entrainment is the sole reason for the higher streamwise mean velocity across rotor of the downstream turbine and a modification of the spanwise advection influencing the shape of the wake should be investigated, too.

The main reason for the striking difference between clockwise and counterclockwise rotating rotors under veering or backing inflow presents the amplification or reduction/reversion of the spanwise flow field. To present and discuss this, we added Fig. 2. A y-z-cross section plot for veering and no veering inflow simulations at $x = 3\,\mathrm{D}$ for clockwise and counterclockwise rotating simulations. The first row presents the $(v,\, w)$ vectors in the y-z-plane, the second row the spanwise wake velocity v, and the third row the vertical wake velocity w. The figure shows a striking difference in the spanwise flow field between clockwise and counterclockwise rotating rotors and also in comparison to the difference between both rotational directions in case of $\frac{\partial v_f}{\partial z} = 0$.

The conclusions do not account for the limitations of the study and its applicability is overestimated. Therefore, the rather definitive answer to the research question provided here does not hold in my opinion (but there could be an argument to pursue the research question further).

We agree with your comment. Therefore, we added the limitations of this work to the introduction:

- Veering tends to occur only at night.
- Veer shows seasonal variability.
- The frequency of occurrence is location dependent
- The occurrence of specific directional shears and wind speeds is also location and also seasonal dependent.

and in the conclusion:

- This work is an idealized parameter study (turbulence is not location sensitive or results from an SBL precursor simulation). The results are not valid everywhere.
- Transferring the results of this study to a wind farm, the presence of upwind turbines has an effect on the turbulence intensity, which did not affect the occurrence of the difference, but its magnitude (see Appendix). Therefore, the rotational direction impact on the power production of a wind farm is another open research topic.

We also excluded any referring to a preferential rotational direction. We only stated that there are differences in the wake in case of $\frac{\partial v_f}{\partial z} \neq 0$. And added the limitations of this work, as it is not valid for every location etc. (see above)

Further we changed the title, excluding the question at all. The 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 answer this question as it is only a simplified parameter study. Therefore, we change the title of our manuscript to 'Changing the rotational direction of a wind turbine under veering inflow: A parameter study'

Specific comments

Page 2, lines 12-14: Sentence should be narrowed to the mixed layer in absence of synoptic or mesoscale forcing.

This is no longer included in the manuscript.

Page 2, lines 17-19: From the text, it could be misunderstood that the wind veer resulting from the influence of friction is directly connected to temperature advection and lifting. Therefore, I would propose to change the sentence ("This wind veer is associated with...") to something like ”Besides the surface friction, temperature advection and dynamic lifting also influence the veering of the wind".

Thank you, we changed it according to your suggestion.

Page 3, lines 9-11: Vasel-Be-Hagh and Archer, 2017 (https://doi.org/10.1016/j.seta.2016.10.004) studied counter-rotating rows of wind turbines in a wind farm and mentions different wake characteristics for the counter rotating turbines.

Thank you, we included the paper together with the 1.4% power increase of a wind farm with clockwise and counterclockwise rotating wind turbine rows in case of no wind veer.

Page 4, lines 9: The rotor diameter is a third ofthe height and the width ofthe simulation domain. Can this affect the wake development? Also the temperature inversion is 50 m above the top tip of the turbine, which corresponds to a very shallow boundary layer. Would a higher inversion layer have an influence on the results?

The spanwise extension of the wake probably has an influence on the streamwise velocity. The averaged x-z contour plots are not smooth for the counterclockwise rotating simulations. This is supposed to be the case as the spanwise inflow velocity is amplified in the wake. The effect not only occurs for counterclockwise rotating rotors. Considering a geostrophic wind of 6 m s^{-1} in CR_u6 it also occurs. Here, the spanwise the the streamwise flow component did change size in comparison to the reference case. Therefore, the effect seems to be additionally influenced by the streamwise wind speed.

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 it to the domain size. We cannot reproduce the simulations in this work on a larger spanwise domain as the applied NBL recursor simulation in the turbulence generation method limits the domain size.

The shallow boundary layer is also related to the domain size of the simulations. In Englberger et al. (2019) the inversion layer starts higher above, but the impact of the rotational direction is still present.

As it is a parameter study which requires a computationally faster method in comparison to the simulations in Englberger et al. (2019) in order to run all various simulations and as the occurring difference is in agreement with the analysis predictions, the spanwise and vertical domain size limitations are not responsible for the rotational difference in the wake.

Page 4, lines 27-29: What is the reasoning for choosing the lower rotor area in contrast to the upper rotor area to modify the type of wind veer? While it is difficult to say anything general about a stable boundary layer, at least for textbook cases the wind veer is stronger in the upper part (opposed to convective boundary layer where wind veer stronger near the surface layer). In addition, the effect is presumably larger in the upper part, because the wind speeds are higher due to wind shear.

The reason was the Ekman spiral in case it is only affecting the lower rotor region with no significant veer in the upper rotor region. See modified attached version of Englberger et al. (2019) in Fig. 3. However, we excluded the simulations with veer limited to the lower rotor area.

As you mentioned in your summary, we investigate 'the influence of the stratification, the magnitude and structure of the wind veer, and the wind speed on this result is also investigated to some extent'. To make this study more consistent, we included the analytical predictions and prepared the numerical simulations only for the corresponding cases. In the revised version, we considered cases of changing the atmospheric inflow conditions (geostrophic wind, directional shear) and the rotational frequency. All other simulation results from the previous manuscript version are eliminated (including veer limited to the lower rotor part) to be more consistent. With including the expected results in the analysis section (3) and comparing the simulation results to them (section 5), our methodology is now more consistent.

Page 5, Eq. 8: Is 00 changed for the very stable case? Otherwise, there is an unstable layer above the hub height, because the pot. temp is 306 K at 200 m 303 K above and that would influence the dynamics for this case.

Yes, sorry this was a typo. It is no longer included in the manuscript as these simulations are eliminated.

Page 5, Eq. 9: That should be $\overline{u_A}^3$ instead of $\overline{u_A}$ and since all else is constant, the available power could be used instead.

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. Our intent with this was only to give a comparison also in case of power differences in % not only of m/s, however, it was very missleading instead of helpful for reviewers. Therefore, we excluded the power completely from our manuscript. In the revised version we only refer to a spanwise and streamwise velocity difference between clockwise and counterclockwise rotating simulations. The difference in % can also be calculated from $\overline{u_A}$.

Page 6, lines 9-10: In Engelberger et al. (2019) — Fig. 8 it is shown that the consistent wake cases have a stronger rotation of the wake compared to the contrasting wake cases at x/D = 7. This means that the downwind turbine is receiving a stronger wind veer for the consistent cases compared to contrasting wake cases (beside the higher $\overline{u_A}$ shown here). That stronger wind veer would presumably impact the power of a downwind turbine negatively. Maybe the downwind turbine could be viewed as a yawed turbine for the upper / lower rotor part and Eq. (9) modified to use an adapted power coefficient for each sections of the rotor.

You are right, please see Fig. 11 and 12 of the revised manuscript version of Englberger et al. (2019) (attached). The turbulent intensity is slightly larger in case of a counterclockwise rotating rotor at 7 D in all rotor heights. His would impact the hypothetical downwind turbine.

Section 3 in general: Spanwise plots of the streamwise velocity at x/D=7 similar to Fig. 3, 5 and 6 should be shown and discussed. I understand that $\overline{u_A}$ is including values above and below the hub height, but in my opinion this is not sufficient to understand the effect of the direction of the rotor rotation and wind veer on the wake structure. Further insights into the mechanism might be gained by looking at turbulent momentum transport or turbulence production, if available from the LES. A few of the following comments reiterate this comment for the specific subsections.

We included a y-z plot of the spanwise and vertical velocity (Fig. 3) and also of the streamwise velocity (Fig. 5). We also include x-y plots also in the upper and the lower rotor half in Figs. 4, 9, 10, 12, 15, 16. Further, we included vertical and horizontal profiles of the streamwise velocity at three specific rotor heights in Figs. 6, 13, and 17. The turbulence profiles are shown in the previous study of Englberger et al. (2019), where the 2 D slices of all three wind components as well as the potential temperature are applied as upstream inflow condition at each time step. As this is a very simplified parameter study and due to the applied turbulence generation method, we decided showing the turbulence profiles is not helpful.

Page 9, lines 12 — 15: I have three questions on this. First, after a look at the model from Engelberger et al. (2019), I do not yet understand the distinction between entrainment and wake recovery and why entrainment is considered as the explanation for the observations. Second, do the authors have any notion why the entrainment is larger for the consistent wake case in a physical sense? For example, whether the consistent wake cases have larger gradients of the absolute value of the wind vector due to the rotation, which might facilitate a stronger turbulent momentum transport. Is the turbulent momentum transports from the LES available to investigate this?

To give an explanation for the difference we updated both manuscript versions (also Englberger et al. (2019)) including a very simple analytical equation, which is the superposition of the spanwise veering inflow equation with the spanwise component of a Rankine vortex (Eq. 17). This shows that the amplification of the spanwise flow component in case of a counterclockwise rotating rotor in case of veering inflow in the NH is responsible for the difference in comparison to a clockwise rotating rotor in which the spanwise flow component is weakened/reversed due to the superposition of the vortex component. This different behaviour of the spanwise flow component impacts the streamwise flow and results in larger turbulent intensity values in case of a counterclockwise rotating rotor. The larger turbulence resulting from the amplification of the wake in case of a counterclockwise rotating rotor results in a larger entrainment rate and therefore in a more rapid wake recovery in comparison to the clockwise rotating case.

Third, I wonder whether a more pronounced ellipsoidal wake cross-section might contribute to a higher $\overline{u_A}$ beside entrainment? Looking at Fig. 6 in Engelberger et al. 2019, an increase of the veer in the wake by the consistent wake cases could make the wake more ellipsoidal. This in turn could cause parts of the wake missing the rotor area of the downstream turbine and increase $\overline{u_A}$, too.

In fact there are two differences contributing. The larger wake deflection angle and the larger spanwise wake width in case of a counterclockwise rotating rotor result in larger $\overline{u_A}$ values.

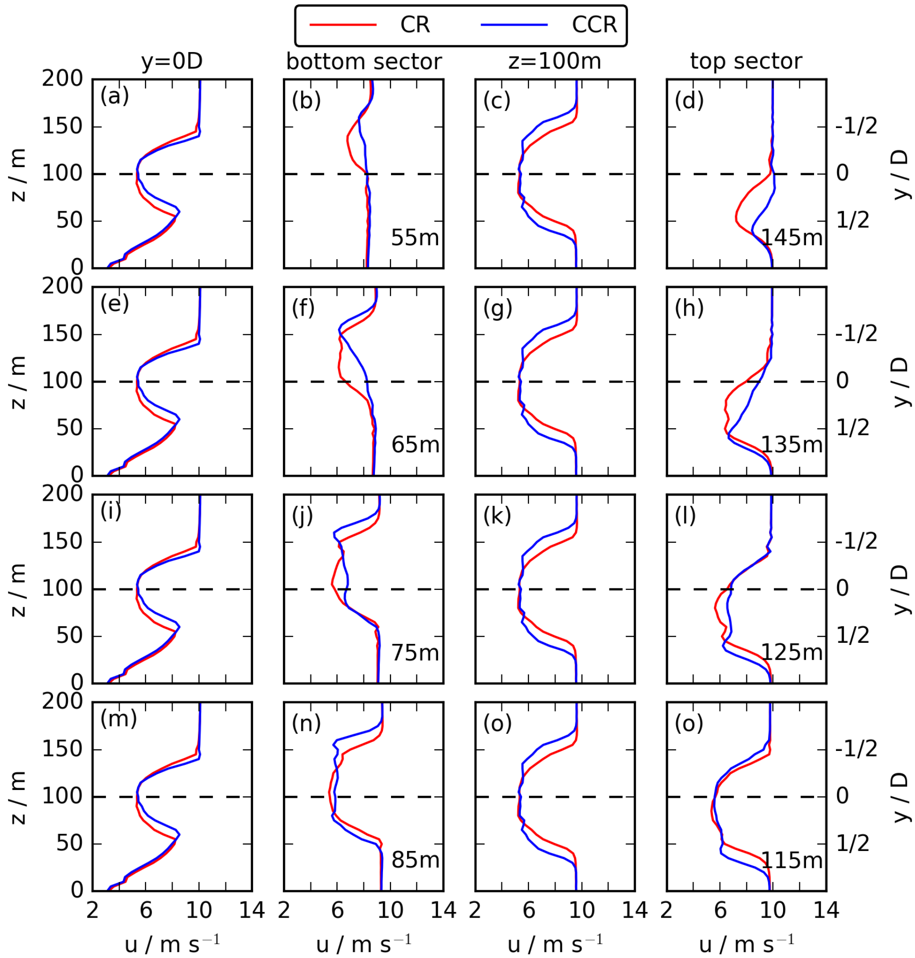

Figure 1. Vertical (first column) and horizontal profiles at different heights for the CR and CCR reference cases.

To show this in the paper, we added a similar figure as in Englberger et al. (2019) for both rotational directions in this work (see Fig. 5). The different lateral elongation of the wake can lead to this assumption for the outer part of the top and bottom sectors. To investigate it in more detail, we also include vertical and horizontal profiles of $\overline{u}$ at specific heights. According to the vertical profile (Fig. 6a), this can be assumed. Looking at the upper and lower rotor half profiles (Fig. 6b, d), the streamwise velocity is larger in case of a counterclockwise rotating rotor. Looking at the same plot in Fig. 1 (only added in this response, not in the paper) at $z = 55$ m or 145 m in the first row, at $z = 65$ m or 135 m in the second row and at $z = 85$ m or 115 m in the fourth row, the difference increases for increasing the radial distance to the nacelle. Therefore, in the top and bottom sector this will certainly contribute to $\overline{u_A}$. This increase in $\overline{u_A}$ in case of a counterclockwise rotating rotor is related to the larger wake deflection angle in case of a counterclockwise rotating wake.

Further, looking at the profile at $z = 100$ m, and also at 85 m and 115 m, the spanwise wake width is larger in case of a counterclockwise rotating rotor. This difference, which is especially pronounced in the right and left sectors, also contribute to the larger $\overline{u_A}$-values in case of counterclockwise rotating actuators.

The larger $\overline{u_A}$(a) values in Fig. 7 are therefore a result of the larger wake deflection angle and the larger spanwise wake width in case of a counterclockwise rotating simulation comparing the reference case CR and CCR.

Page 9, lines 27-29: Linking stability directly to the time of day requires the assumption of a radiation driven diurnal cycle of the boundary layer with the absence strong synoptic or meso-scale forcing. The same for page 12, lines 8-11.
We agree. As including different levels of the atmospheric stability is rather complex and it is not explained by the simple analytical equation, we postponed this results and will investigate them in more detail in the future.

Fig. 4: Panel b is quite busy. Would it be possible to make this figure a four panel figure and separate the weak, moderate and strong wind veer cases in one panel and the cases with only the lower rotor area affected by wind veer in a second panel? That would be also more consistent with the subsection structure used in the text.
We agree. As we eliminated a few of the simulation, we only result with one figure showing $\overline{u_A}$. The corresponding figure 7(e) includes the same amount of profiles as old figure 4(b), however, now the only consider a different amount of directional shear and therefore the lines are not crossing etc. as before. Due to these changings, we leave the result for all simulations with varying the directional shear in one panel as it makes it easier for the reader to see the difference in the wake if the directional shear is changed.

Page 12, lines 6-9: I believe the phrasing of this sentence is unfortunate, because it could be misunderstood that the power improvement of the downstream turbine itself becomes larger with longer duration (the percentage values from the previous sentence increase over time).
We agree that it could be misunderstood. We eliminate this sentence as the potential temperature varying simulations are not longer included.

Page 12, lines 18-21: This sentence explains the difference between CR and CCR, but not the difference between CCR_th60 and CCR_th15/CCR. The faster wake recovery for more stable stratification (and presumably a subsequently lower turbulence intensity) for the consistent wake cases is still counter intuitive to me. Do the authors have any explanation what is causing that behavior?
As the investigation with different background potential turbulent profiles is rather complex and cannot explained with the simply analytic equation, it is excluded from this paper and we will investigate it in more detail in the future.

Page 12, lines 25: As for the comment on page 9, lines 12-15, I believe it is possible that an increased ellipsoidal wake shape with increasing wind veer might have a pronounced effect on $\overline{u_A}$ beside entrainment. Vertical cross-sections of the streamwise velocity and plots of the momentum transport could be used to investigate. Maybe some insights into the curious decrease for the strong wind veer case might be gained from them, too.

To investigate this in more detail, we perform two additional simulations with a directional shear of 0.12° m^{-1} and 0.20° m^{-1}. According to our results there is a critical directional shear value ds_c with 0.12° $\mathrm{m}^{-1} < ds_c <$0.16° m^{-1}. Below this critical values, the $\overline{u_A}$-value is larger for a counterclockwise rotating rotor, whereas above it is larger in case of a clockwise rotating one.

The new figure 13 gives some insight into this. An increase of the directional shear increases the wake deflection angle. However, increasing the directional shear to high values of 0.16° m^{-1} and even very high values of 0.20° m^{-1} results in a larger streamwise velocity close to the nacelle, contributing to larger values especially in the left and the right 90° sectors (last two rows of Fig. 13). This overcomes the larger streamwise velocity values in the top and bottom sectors in Fig. 13 at 75 m and 125 m respectively. Therefore, the more rapid wake recovery for large directional shear values results in larger $\overline{u_A}$-values in case of clockwise rotating discs.

In the paper it is explained with: 'In the clockwise as well as the counterclockwise rotating actuator simulations (Figs. 10 - 12) the wake recovers more rapidly if directional shear increases. A larger directional shear represents a larger resolved turbulence source due to an increase of $\frac{\partial v_f}{\partial z}$, and, therefore, the simulations with higher directional shear values result in higher entrainment rates and a more rapid wake recovery.'

Page 13, line 1-2: Is this amplified the turbulence production occurring at specific regions of the wake? Could the terms of the TKE budget provide any insights into the cause of the higher entrainment (if they can be computed from the LES)?

This has to be tested in LESs applying precursor simulations of the SBL for different directional shears. This is one of our planned next steps.

Page 15, lines 5-6: I would always expect a larger $\overline{u_A}$ for an increased inflow wind speed if the efficiency of the upwind turbine is not changing (as it is the case here) and I am not seeing where the entrainment is entering the picture from the results. Is that sentence referring to the relative difference between CR / CCR and CR_u14 / CCR_u14?

Yes we agree with your expectation. A larger wind speed has an effect on the streamwise wake elongation. Yes, the specific sentence is referring to the relative difference between CR/CCR and CR_u14/CCR_u14.

Section 3.6 and Fig. 7: I like this section bringing everything together and the figure is very informative, but I had a hard time reading the first two paragraphs of this section due to the amount of simulation abbreviations. Since the simulations can be deduced from the Fig. 7, perhaps the text could focus on the physical meanings. E.g. "The blue square shows a power increase by 4% for counterclockwise rotating turbines compared to a clockwise rotating ones for a weakly stable stratification." instead of “The point 'th15' represents a power increase by 4% at 7D for CCR_th15 in comparison to CR_th15".

Thank you. Actually, due to all new figures we extracted this figure and also the text. See Fig. 2 (only here) as updated version.

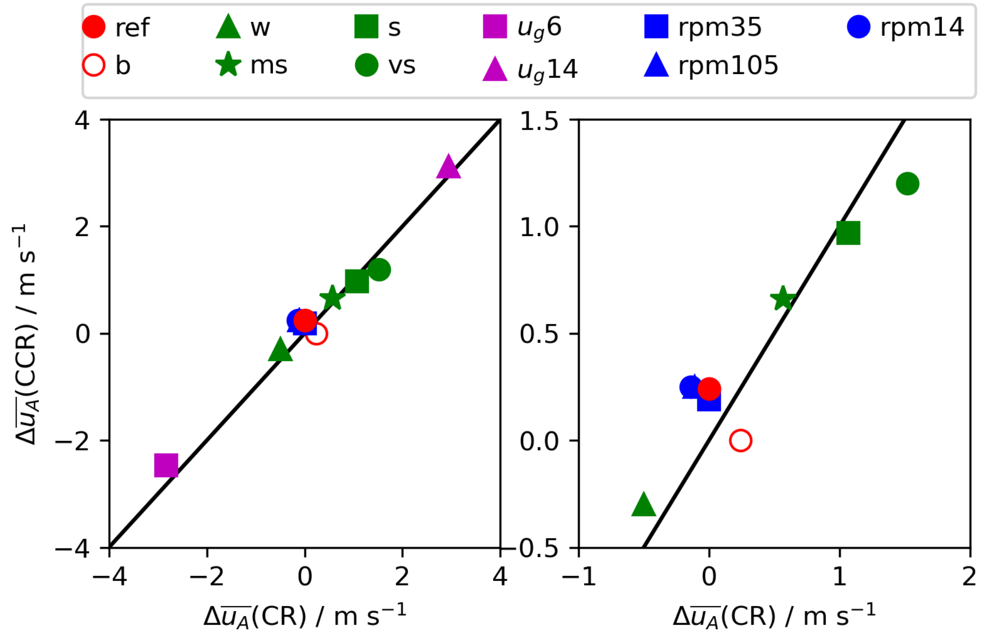

Figure 2. Coloured contours of the streamwise velocity $\overline{u_{i,j,k_*}}$ in m s^{-1} for different geostrophic winds at $z = 75$ m. The black contours represent the velocity deficit VD_{i,j,k_*} at the same vertical location.

Page 17, line 6-7: It should be specified that the power of the waked downstream turbines is considered here (it could be misunderstood that the power of the upwind turbine improves, too).

Thank your for this hint. As we excluded power and only discuss the velocity of one wind turbine, no misunderstandings like that should be possible.

Page 17, lines 22-24: How much of that cumulative capacity is located in wind farms, where wake effects can occur? (in contrast to isolated turbines where it would not matter).

This is an interesting question. In the GEWC there is no distinction between offshore and onshore. As we extracted the power and the discussion about any preferential rotational direction, we also extracted the NH and SH comparison of installed capacity.

Page 17, lines 28-29: I believe there is a need for further studies on some aspects to this question:

We agree and added are complete paragraph about this:

’To explore a more comprehensive assessment of the wake impact, further investigations would be interesting. The investigation of the non-linearity of the interaction process, numerical simulations applying the turbulence of a SBL precursor simulation for different strengths of stratification and directional shears, or even considering a low-level jet at the rotor height. Topography could influence the wake dynamic explored here. 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. However, an important point will be to prove the theoretically predicted effect resulting from superposition of inflow veer with the vortex component on the wake with measurements. '

1: This conclusion is based on numerical simulations with simplified a very simplified estimation of the downstream turbine power. A verification with experiments for real wind turbines would be a reasonable call. 2: Unstable and neutral stratification of the boundary layer is not regarded in this study, but can be subject to wind veer as well. 3: Real wind turbines have an induction zone that modify the flow further from the simulation results. 4: Besides the higher streamwise velocity investigated here, the wake structure could see further changes (turbulence intensity, veer, shear), which could impact a downstream turbine. 5: It is possible that two important categories of wind farm locations have a different veering/backing ratios then considered here. Offshore wind parks in proximity to a coast due to the baroclinicity between land and sea. Wind farms located on a ridge due to topography and baroclinicity.

1: The power is eliminated and we now stated in the conclusion: 'However, an important point will be to prove the theoretical effect resulting from superposition of inflow veer with the vortex component on the wake with measurements.'

2: We only focus on veering and backing in nighttime situations following Walter et al. (2009).

3: We agree, this will modify the streamwise velocity at the downwind turbines location. Here, however, we compare the streamwise velocities for both rotational directions with no downwind turbine in both cases. Therefore, the difference between clockwise and counterclockwise is comparable.

4: We investigated more aspects in the revised version of the paper with including the horizontal and vertical profiles of u.

5: We listed possible differences related to topography and location in the conclusion in the revised version of the manuscript.

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