Review of “Should wind turbines rotate in the opposite direction?” (wes-2019-105)

Summary

The article investigates the impact of clockwise and counterclockwise rotating wind turbine rotors on the power of a waked downstream turbine in the presence of wind veer and backing. The simulation results show an increased power of the downwind turbine if the streamwise component of the vorticity of the wake is consistent with the one resulting from the wind direction change (i.e. both have the same sign). It is concluded that changing the rotational direction of wind turbines on the northern hemisphere from clockwise to counterclockwise could increase the power of waked downstream turbines. 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.

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.

(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).

(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.

(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 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).

Language

I am not a native English speaker, but the manuscript seems to be well written and I did not notice any spelling or grammar mistakes.

Specific comments

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

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”.

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.

Page 4, lines 9: The rotor diameter is a third of the height and the width of the 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?

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.

Page 5, Eq. 8: Is Θ_0 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.

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.

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.

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.

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?
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.

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.

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.

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).

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?

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.

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)?

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?

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”.

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).

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).

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

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.