Reply to Reviewers
Preprint wes-2024-124
Title: Numerical Investigation of Regenerative Wind Farms Featuring Enhanced Vertical Energy Entrainment
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Authors' reply to comments

We thank the reviewer for taking the time to read and acknowledging our work. Regarding the interesting inquiries posed by the reviewers, we believe they have all been adequately addressed in this rebuttal as well as in the revised version of the manuscript. We also appreciate the valuable suggestions for further improving the manuscript, and the paper has been revised in light of the comments. The actions taken based on the reviewers' comments are detailed in the following.

Reviewer#1

General comments:

This paper presents a novel idea which aims to increase the AEP in a wind farm, by forcing the advection of the atmospheric wind into the wake to reenergise the incoming flow for the next wind power generator. This paper presents preliminary RANS simulations using actuator elements to represent the wind turbines.

While I have doubts that this type of system would be largely deployed in the future (due to structural integrity, robustness, reliability and control), the concept is original and it's worth doing this thought experiment.

The paper is globally well written and relatively easy to follow, despite some convoluted turns of phrase. The simulation results are interesting and seem possible to be reproduced as the settings and the code are apparently available.

I have mostly one main concern and one point that troubled me.

Reply:
We sincerely thank the reviewer for acknowledging our work and expressing interest in the concept we have proposed. The inquiries and suggestions raised have been addressed in detail in the following responses.

Specific comments:

1. My main concern is the possible large blockage of such a system. When I look at figure 1, I see quite a lot of projected surface compared with more conventional wind turbines. I would imagine that not all the flow would pass through the system and would rather deviate and go around the system. If the flow deflects, it means that there is a smaller mass flow rate through the system, so less energy can be extracted. Could the *authors confirm that in their RANS model, the flow deflection is correctly modelled? For example there are assumptions to define u_{ls}^{ele} and u_{in}^{ele}. May this assumption impact the mass flow passing through the system?*

Reply:

We appreciate the reviewer for raising this critical aspect concerning the simulation of wind turbine aerodynamics. In our simulation, MRSL is represented using an actuator disk and actuator lines for the rotor and wings, respectively. This approach models the MRSL's presence in the flow field through a body force field, which is known to effectively capture blockage effects, as reflected in our results.

Particularly, in the results of case **Without-Lifting** in Figure 11, the streamwise velocity is observed to slow down before passing through the first row of MRSLs. Furthermore, examining the second bottommost streamline with an arrow, it is clear that it is deflected upward upon encountering the first row of MRSLs, demonstrating the blockage effect noted by the reviewer. Additionally, the thrust predicted by our simulations aligns well with the predictions from one-dimensional momentum theory, indicating that the blockage effects are adequately modeled (note that we used C_T^* instead of C_T to model the exerted thrust, see the Section 3.4 for the definition of C_T^* and the second paragraph of Section 4.1 for the values of the outputted C_T/thrust). This results further support that the ratio between u_{ls}^{ele} (streamwise velocity locally sampled at the actuator element) and u_{in}^{ele} (unperturbed streamwise velocity at the actuator element) is faithfully predicted (u_{ls}^{ele} and u_{in}^{ele} are defined in Section 3.4). Thus, it can be concluded that the axial induction factor (a^{ele}) is correctly captured, indicating that the streamwise velocity passing through the actuator disk and, subsequently, the power output predictions are accurate.

However, we acknowledge that the above explanation is valid under the assumption that the effects of the supporting structures are neglected. Incorporating these structures would introduce additional aerodynamic blockage that could impact the power output of MRSL. Accurately modeling such effects would require more complex simulation setups (such as additional actuator elements, the use of an immersed boundary method, or explicitly resolving the full geometry), making the parametric study extremely computational intensive. We have addressed these limitations of the current actuator models in the revised manuscript (see the end of the first paragraph of Section 3.4).

2. The point that troubled me is the terminology with "upward-lifting" and "downward lifting". To me, it seems the upward wind in the wake is due to a downward lift (for example in figure 3, the suction side of the airfoil points downward, so the lift is directed downwards, but it would create an upward wind). This does not impact the results of the paper, but I was doubting if I understood the concept correctly. Could the authors confirm or correct my thoughts and better explain and define this concept in the paper?

Reply:

We thank the reviewer for highlighting that the naming of our cases may cause unnecessary confusion. To clarify, the reviewer's understanding is correct: MRSLs in the case previously referred to as **Upward-lifting** (former name) indeed experience downward lift. We originally named it **Upward-lifting** because the wake of it is being lifted upward.

In light of the doubt posed by the reviewer, we have revised the naming of our simulations to avoid potential ambiguity. Specifically, **Upward-lifting** has been renamed to **Up-Washing** and **Downward-lifting** has been renamed to **Down-Washing**.

I have a couple of other minor remarks:

3. Why did the authors choose this airfoil for the lifting devices?

Reply:

S1223 airfoil was selected because it is a representative airfoil profile capable of achieving a high lift coefficient (see Selig et al. 1995 and Selig et al. 1997 in References). Additionally, its moderate camber and thickness make it more practical for real-world implementation. However, we would like to emphasize that the specific airfoil choice is not critical to the performance of the MRSL. The primary objective of the MRSL's wings is to generate strong trailing vortices, which are designed to enhance wake mixing and facilitate wake recovery.

This explanation has been included in the revised manuscript (see the second paragraph of Section 2.2).

4. A Turbulence Intensity of 8% seems fair, but it could be much higher in the reality. As it is mentioned the Turbulent Kinetic Energy plays a minor role, I would interested to know whether the conclusions still hold with a higher TI (such as 20%).

Reply:

The reviewer has highlighted a critical aspect regarding the current deployment of MRSLs. To demonstrate that the concept of regenerative wind farms is robust against variations in inflow turbulence intensity (TI), we conducted additional simulations with different inflow TI values, as detailed in Appendix D. In addition to the previously tested 8%, cases **WL**, **UW**, and **DW** were also tested with inflow TI being 5% and 14%.

The results indicate that while the effectiveness of the MRSLs' lifting devices decreases with higher inflow TI, their performance remains significant. Notably, even at an inflow TI of 14%, which is at or even beyond the upper limit of typical offshore conditions (see Ref. Hansen et al., 2012), the power performance of MRSLs with lifting devices still outperforms those without lifting devices by more than 50%.

We also attempted simulations with an inflow TI of 20%. However, probably because it is unrealistically high for typical offshore environment ($z_0 = 10^{-4}$ m), the solutions do not converge well, and thus they are not presented.

5. Similarly, the difference of results between the different turbulence models seem quite large. Could the authors precise what could be the reasons for such a large difference between the k-omega and k-epsilon models?

Reply:
We thank the reviewer for raising this very interesting question. Motivated by this, we conducted a brief investigation into the cause of the deviations between turbulence models, which has been documented in Appendix B. In summary, we analyzed the eddy viscosity field ν_T) predicted by different RANS models and found that the realizable $k - \varepsilon$ model is significantly more diffusive (predicts higher values for ν_T) than the other two turbulence models surveyed (the $k - \omega$ SST and RNG $k - \varepsilon$ models) for this application.

This increased diffusivity in the realizable $k - \varepsilon$ model causes the trailing vortices generated by the MRSL's wings to dissipate more quickly, thereby weakening the upwash and downwash effects and slowing the wake recovery rates in the **UW** and **DW** cases. Conversely, the greater diffusivity also promotes the diffusion of mean kinetic energy (MKE), leading to a faster wake recovery rate in the **WL** case compared to results obtained using the other two turbulence models.

Reviewer#2

General comments:

The article describes a multi-rotor wind energy system with static lifting devices, aimed to increase the momentum entrainment and mitigate wake losses.

The paper is structured well, and the methodology is mostly clearly presented. Perhaps the paper could be shortened by not spelling out every well known concept, for example the RANS equation system with k-omega turbulence model (eqs. 1-4).

Reply:

We thank the reviewer for acknowledging our work and for the valuable suggestion to improve the readability of the manuscript. In response, we have relocated the descriptions of the governing equations for the RANS with the $k - \omega$ SST model to Appendix A (previously in Section 3.1) and the transport equations of energy to Appendix H (previously in Section 3.5). These adjustments have shortened the main body of the manuscript while ensuring it remains self-contained, as the key equations, specific definitions, and detailed methodologies are still provided in the Appendices.

Specific comments:

1. It is not clear how the MRSL appears in the modeling grid. The name indicates several rotors, but Figure 4 indicates one (square) rotor with the diameter D=300m, and 186m hub-height.

Reply:

In our simulations, the MRSL is represented by actuator disks and lines. While the system is conceived to include several sub-rotors, these are not explicitly modeled in our simulations to avoid drastically increasing computational costs and the complexity of parameter studies. In practice, the real-world system would likely resemble the depiction in Figure 1. However, to clarify how the MRSL is represented in the computational domain, Figure 4 is provided. Specifically, the sub-rotors of the MRSL are simplified into a single actuator disk, which is why it appears as a single rotor in Figure 4.

We acknowledge that the previous caption of Figure 4 might not be clear enough and could have caused unnecessary confusion. We have not adjusted the caption clearly stating that Figure 4 is a representation of MRSL in computational domain.

2. Nothing is said about the system integrity and loads on the structure. How does such a system turn into the wind? It could also be assumed that it is fixed and suitable for uni-directional wind climate. In this is the case, please state. While this device appears entirely conceptual and will highly unlikely ever be used at scale, it is however attractive to find an engineering way to capture some of the potential to double the energy density in large wind farms (e.g. Table 4). Have you perhaps tried to add the 5th wing at the lower edge of the system? What about if the wings are installed separate from the multi-rotor structure?

Reply:

We sincerely appreciate the reviewer's thoughtful considerations regarding the MRSL and regenerative wind farm concepts, as well as the valuable suggestions shared with us. Below, we address the points raised:

<u>About structural integrity and general considerations:</u>

This study primarily focuses on the aerodynamic aspects of the MRSL as part of demonstrating the regenerative wind farm concept. The primary goal of this manuscript is to provide proof of concept for the MRSL and regenerative wind farms. Consequently, structural integrity has not been investigated in detail, as it is considered beyond the scope of this work. However, we fully acknowledge that a comprehensive evaluation of structural integrity is critical for real-world implementation. This will be undertaken in future studies if the concepts prove to be feasible, as stated in the final paragraph of the manuscript.

About yawing:

We postulate that the MRSL system would yaw against wind direction changes, similar to traditional wind turbines (yawing a large structure of this scale may seem challenging but we think it might be comparable to yawing a 25 MW HAWT as the rotor diameter is expected to exceed 300 m). However, we acknowledge that detailed investigations into yawing mechanisms and their feasibility are necessary before the realization.

About the fifth wing:

We did not add a fifth wing at the bottom of the MRSL because wind speeds in that position are relatively low under realistic atmospheric boundary layer (ABL) inflow conditions (as shown in Figure 12). However, this is not a limitation of the MRSL design. A fifth wing could be added if it proves beneficial based on an overall assessment (balancing wake recovery rate, system complexity, and cost).

About installing the wings and turbines separately:

We thank the reviewer for this insightful and inspiring question. We believe that installing the wings and turbines separately could achieve similar effects to those demonstrated with the MRSL in this study. An exciting potential of this idea is the possibility of introducing the existing wind farms with wings (i.e., placing wing structures in between the wind turbines), effectively transforming them into regenerative wind farms. This idea holds significant promise and represents a compelling topic for future research. In recognition of this possibility, we have included it as a topic for future exploration in the final paragraph of the Section Conclusions and outlooks.

3. There are many acronyms in the article, but downward-lifting and upward-lifting are for some reason spelled in full over 100 times. Suggest using DL and UL instead.

Reply:

We have changed the case names of **Upward-lifting** and **Downward-lifting** to **Up-Washing** and **Down-Washing**, respectively. And in light of the suggestion given by the reviewer, we have used the acronyms **UW** and **DW** to refer **Up-Washing** and **Down-Washing**, respectively.

Some other adjustments:

We have changed the Section title of “Conclusions” to “Conclusions and outlooks”, as we think this reflects the contents of the section more precisely.