Authors’ response to Referee 2

General

This article presents a wind tunnel study of a wake control strategy named ‘Helix’. The article is of relevance to wind energy community and fits within the scope of the journal. The study is performed systematically and data quality is good. There are, however, some concerns regarding the work which need to be addressed properly. These are listed below:

We thank the referee for reviewing this manuscript, the valuable feedback, and the constructive comments. At this stage of the review process, we respond to referee #2's comments and propose improvements for the journal manuscript. The referee's original comments are printed in bold followed by the corresponding answers. A screenshot of the different versions of the updated passages from the manuscript is provided below the answer.

Specific comments

1) **My main concern is regarding the blockage effect in the experiments. As authors indicate, the blockage is about 20%, which is considerably high. To tackle this, they propose a ‘blockage-corrected free-stream velocity’. Does correcting the free-stream velocity resolve completely the effect of blockage? In principle, your turbine is placed in a confined channel, where the flow acceleration can affect the turbine power output and also affect the development of the wake due to an effective favorable pressure gradient in the flow. How is this addressed in the work? At least, the authors should mention the limitations introduced in the work due to the blockage effect to properly guide the reader.**

   Thank you for this important comment. The same concern was also raised by Reviewer #1 and Reviewer #3. We agree that the blockage is very high and was not discussed in an appropriate way. We added information about the blockage effect in section 3.2. We included a paragraph in which we use several studies investigating the blockage effect to discuss the effect that blockage is expected to have on wake development.

however accelerated, resulting in a higher velocity experienced by the turbines. ~~An estimation of the~~ To account for such an effect on the turbine performance, it can be corrected by applying analytical models, a recent review is presented in the study by Steiros et al. (2022). More information is presented by Ross and Polagye (2020) who conducted an experimental assessment of such models and their application to different wind turbine concepts. In the present study, the performance was corrected by a calculation of the Rotor Effective Wind Speed (REWS) for the upstream G1, done as described in Campagnolo et al. (2022)~~,~~. This revealed a blockage-corrected free-stream velocity $U_{\infty,corr} \approx 5.9\ m/s$, which correlates to the rated wind speed of the model wind turbine. Consequently, the turbine is operated at a tip-speed ratio of approx. $\lambda = 8.2$. ~~In the~~ Based on the authors' knowledge, there are no analytical blockage correction models that would allow us to quickly assess the influence of the wind tunnel walls on the wake. One possible way to investigate this is to use computational fluid dynamics (CFD). Zaghi et al. (2016) studied the effect of blockage on a model wind turbine with a Reynolds Averaged Navier-Stokes (RANS) simulation. They analyzed the streamwise wake velocity and found increased velocities in the area behind the rotor but also in the outer region of the wake in case a wind tunnel wall was present. In a more detailed analysis Sarlak et al. (2016) used Large Eddy Simulation (LES) combined with the actuator line technique to investigate wake velocity and Reynolds stresses for different blockage ratios up to $\alpha = 0.2$. They found a significant impact on the mean wake velocity in the case of the highest blockage ratio. Especially in the region outside of the rotor, the velocity is found to increase; this augmentation is mitigated in the rotor area but is still present. Furthermore, they concluded that blockage has no considerable effect on the wake mixing rate as maximum and minimum velocities do not differ significantly. In a combined experimental and numerical study McTavish et al. (2014) investigated the influence of wind tunnel blockage on the wake width and found that the wake compresses when blockage increases. Consequently, the wake results of the presented study are expected to be characterized by slightly higher streamwise velocities and a narrower wake compared to a full-scale test. As a result, in the analysis of the

2) The experiments are performed in an almost laminar uniform flow (Tu<0.5%). I understand that the authors intend to isolate the effect of control strategy. However, the relevancy of the control approach to field conditions with turbulence intensity greater than 5% and boundary layer shear must be discussed. In other words, does the control strategy remain effective at high turbulence intensities and in the presence of flow shear?

Thank you for raising this point. The same topic was also brought up by referee #3. We agree that it is important to address this point better. We added a small discussion about this in the literature review in the introduction chapter. We added a source of a study where the authors investigate the effect of inflow turbulence on the efficiency of dynamic wake mixing and show that inflow turbulence has a significant inflow on the effectiveness of wake mixing for power optimization. Furthermore, we updated the future works slightly to say that further investigations on inflow turbulence are needed.

approach experimentally in a wind tunnel (W/T) and should give a detailed insight in the wake aerodynamics. To provide such a detailed insight, the flow in the wind tunnel has to be a clean lab flow, which is uniform and is characterized by a very small turbulence intensity. Such clean inflow will not only highlight the effects of the control technique in the wake but also influence their effectiveness. Wake mixing techniques like Helix add turbulence to the wake. Consequently, if the turbine inflow is already characterized by higher ambient turbulence the effect of wake mixing will be mitigated. In a recent study Mühle et al. (2024) compare power gains for wake mixing by dynamic yaw for different inflow turbulence. They found a strong reduction of the effect on the power of a two-turbine setup in case of high inflow turbulence and thus confirm the findings of Munters and Meyers (2018a). Nevertheless, they suggest that wake mixing has the potential to improve the power output of a wind farm in case the wakes are strong and persistent.

Nevertheless, the Helix technique and its real-world realization ~~is~~ are just at the beginning of its development. Consequently, this study can only provide a first insight ~~on~~ into the potential and wake mixing mechanisms. ~~An~~ Additional wake analysis with different Stroual numbers is required to prove that the wake meandering is the main driver for increased mixing. Moreover, an investigation of the influence of inflow turbulence on ~~the wake mixing potential could be the next step~~ wake mixing is needed to understand its potential in more realistic inflow conditions characterized by higher levels of ambient turbulence. Further, testing the Helix technique in wind farm control studies could be promising.

3) **The authors indicate that the turbine rotation is fixed at an optimum value. How is this optimum value obtained and does it remain the same for the uncontrolled and controlled turbine configurations?**

Thank you for pointing this out. It is obtained to maintain the operational tip-speed ratio of the G1 model turbine λ = 8.2. With the corrected inflow velocity of $U_\infty \approx 5.91$ m/s this results in a rotational velocity for the turbine of 840 rpm. This rotational velocity was also maintained when the Helix control operated the turbine. We added a clarifying sentence in section 3.2.

> varied within the range $10.1 : 0.3 : 17.9\ Hz$. ~~The~~ In the controlled and uncontrolled configuration, the upstream turbine is operated with a constant rotational frequency of $f_r = 840\ rpm/60 = 14\ Hz$ and an optimal collective pitch offset of $\beta_0 = 0.4°$. With these adjustments, the turbine operates at the desired G1 tip-speed ratio $\lambda = 8.2$ at $U_{\infty,corr} \approx 5.9\ m/s$. These controls

4) **The pressure probe measurements are performed for 40 seconds. Is that time interval sufficient to give converged flow statistics?**

Thank you for this remark. We have carried out preliminary tests in this regard, which have shown that the relevant content in terms of flow statistics is recorded with a measurement time of 40 seconds. For example, with the additional frequency of 2.5Hz introduced by the helix control, 100 such events occur. We added a clarifying sentence in section 3.2.

> within the range $0 : 0.052 : 0.73$ both in CW and CCW direction. For each investigated actuation frequency, the measurement time is set to $t_s = 40.0\ s$. With this recording time and the beat frequency of $f_e = 2.5\ Hz$, the rotation of the fixed-frame moments induced by the Helix control is recorded 100 times in one measurement interval. The downstream turbine (sensor

5) **The baseline plots in figure 7 are very hard to distinguish from the background of the plot. Consider improving the figure.**

Thank you for the hint. We agree that the lines are, at first sight, hard to detect. However, we decided on purpose to have these lines not as prominent as the ones for the actuated cases, as they only represent the baseline cases and are constant anyway. The main message of these plots is provided by the thick lines depicting the data of the actuated cases, and we did not want to draw attention away from those. When the reader first sees the graph, he immediately notices them and thus the two peaks for CW and CCW rotation; when focusing on the graph a bit longer, he also sees the secondary information, which is the reference cases. Consequently, we decided to leave them as they are.

6) **The authors compare the trend in the thrust coefficient with that in the available power, and identify some differences. Is that a fair comparison? If so, what is the possible explanation for the difference? Wouldn't it be more appropriate to compare thrust coefficient with power coefficient?**

Thank you for this comment. We agree with the referee that looking at the thrust coefficient and normalized power is unfair. We changed the figure, which now shows the thrust normalized with the thrust measured for the baseline case. The thrust coefficient was calculated using an estimate of the rotor effective wind speed, which can be affected by uncertainty when the Helix is active.
We added some discussion about the observed trend for power and thrust, highlighting that additional analysis are needed.

In general, the extracted power of the actuated turbine decreases for all ~~test~~ frequencies, since it is operated in a non-optimal operating point. With an increasing actuation frequency, this effect is also increasing. The decrease is particularly significant (around 30%) for the highest actuation frequencies, and is remarkably higher than the values (few percent) noted in previous research works based on CFD (Frederik et al., 2020a) and aeroelastic (Taschner et al., 2023b) simulations. Whatever the reason for this difference may be – different performance of the G1 compared to that of a full-scale machine, wall blockage affected by the actuation frequency, physical effects not modeled in the simulation environments – further investigation is needed.

Figure 7 shows the thrust ~~coefficient C_T~~ T of the actuated upstream turbine~~for different additional excitation frequencies~~, normalized by the thrust of the upstream/actuated turbine in the baseline scenario T^*. The trend for ~~C_T is opposite to~~ T differs from the one of the ~~available power :~~ normalized power of the upstream turbine in Figure 6: the thrust is indeed reduced by the Helix but remains quite constant with increasing actuation frequencies ~~, C_T is increasing, whereas a slight ditch can be observedat $f_\beta/f_r = 1.0$ ($St_{add} = 0$). However, towards the very high actuation frequencies $f_\beta > 1.2 f_r$ this increase seems to have reached a maximum and is again decreasing slightly~~(only a minor reduction is observed). Once again, the reason for the different trends observed for power and thrust requires further analysis.

7) Is the blade pitch synchronized with the rotor rotation for all the cases?

Thank you for the question. No, it is not. We added a paragraph in section 3.3 that comments on this aspect and its impact on the results, which is not present.

~~The results of these experiments~~During each experiment, the Helix was activated a few seconds before the start of the acquisition, to allow for the downstream propagation of the new wake. The activation of the Helix, however, was not synchronized to a specific azimuth position of the rotor. For $f_\beta \neq f_r$ this aspect is not relevant and does not affect the results. In this regard, each plot in Fig. 3 shows, for 5 of the conducted experiments, the distribution of the first blade azimuth (θ_1) detected when the required pitch is within the range $0.98\beta_{max} < \beta_1 < 1.02\beta_{max}$, with $\beta_{max} = \hat{\beta} + \beta_0 = 4° + 0.4°$ the maximum requested pitch angle. The dashed black line, instead, marks the azimuth position ($\theta_{1,ini}$) at the very first time the required pitch is within the range $0.98\beta_{max} < \beta_1 < 1.02\beta_{max}$. For experiments conducted with $f_\beta \neq f_r$, the pitch is detected around β_{max} for the whole range of azimuth positions (the distribution is not completely homogeneous solely due to the discrete sampling), regardless of the value of $\theta_{1,ini}$. This implies that the fixed-frame moments produced by the Helix rotate over the entire range of azimuth positions, which leads to the expected effects on the wake.

Different is the case with $f_\beta = f_r$: the maximum pitch is indeed always detected at the same azimuth position, i.e. around θ_{ini}. Previous experimental (Campagnolo et al., 2016) and numerical (Fleming et al., 2014) investigations found that the azimuth position of maximum pitch affects the achieved amount of wake deflection. However, the study of Wang et al. (2016) revealed that the power gains observed on the downstream machine may only be a little higher than the power losses experienced by the upstream machine, at the price of a significantly increased loading of the upstream machine. In this article, it was therefore preferred to exclude the case $f_\beta = f_r$ from the following discussions. A complete analysis would, indeed, require investigating

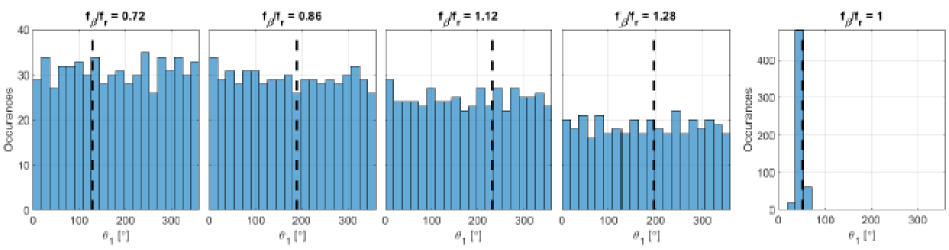

Figure 3. Distributions of the first blade azimuth (θ_1) detected when the required pitch is close to β_{max}, and with $f_\beta/f_r = [0.72, 0.86, 1.12, 1.28, 1]$. The dashed black lines mark $\theta_{1,ini}$, i.e. the azimuth position recorded at the very first time the required pitch is within the range $0.98\beta_{max} < \beta_1 < 1.02\beta_{max}$.

the effect of the azimuth position of maximum pitch. Furthermore, previous results have shown the poor performance of this specific implementation of the Helix, thus making it uninteresting.

8) For phase-locked measurements, how is it ensured that during a certain azimuthal phase the pitching phase is also the same for all the cases?

Thank you for this comment. We try to clarify our approach in the following. The situation that you describe wasn't ensured during our study. We would have needed to measure the flow within the wake for several minutes, to have a sufficient amount of such events, and thus multiple exact matchings. We see the effect of this shortcoming in the Figure below, and the corresponding discussion in the text.

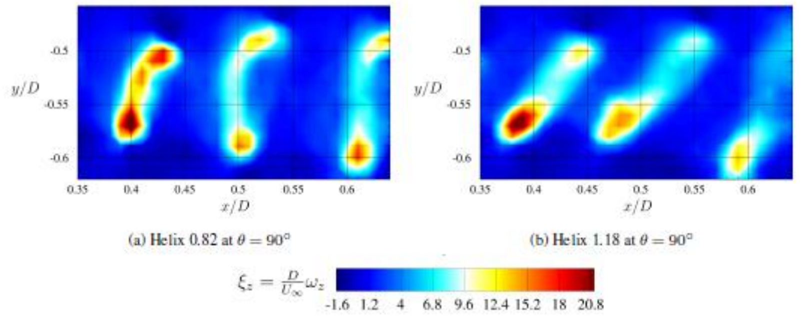

Figure 15. Normalized phase-locked vorticity in z-direction ξ_z at azimuthal position $\theta = 90°$ for a) Helix 0.82 and b) Helix 1.18. Video link: https://youtu.be/ta3KwE5yuSQ.

By applying the additional phase locking with the beat frequency, we tried to overcome this shortcoming. We added some further explanation in section 4.2.3, on how we extracted the envelope/beat frequency data (see Reviewer #1 Question 12)

a sinusoidal signal with $f_e = 2.5\ Hz$ that is used in the following to phase-lock the measured FRAP data. The process of extracting the envelope/beat frequency from the blade azimuth and pitch position is visualized in Figure 17. After extracting the time series for the beat frequency for each individual measurement point, the measured FRAP data are phase-averaged with the beat frequency. Readings of all measurement points can thereby be correlated.

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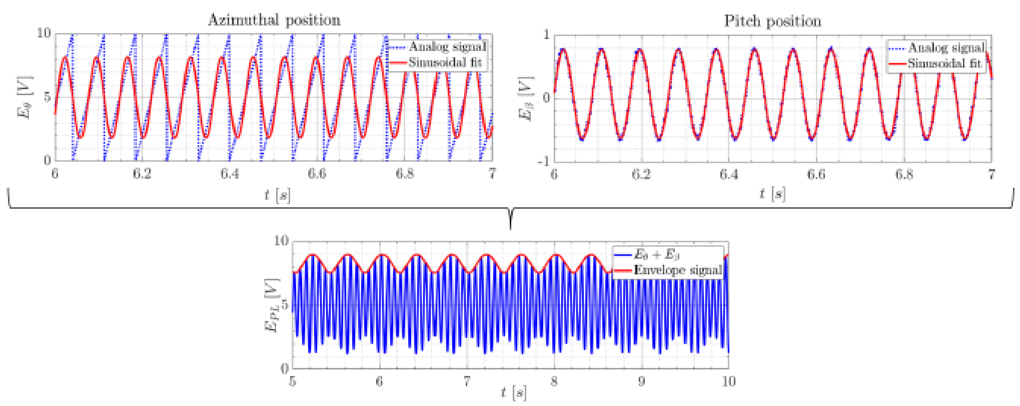

Figure 17. Schematic visualization of the process of extracting the envelope/beat frequency from the blade azimuthal and pitch position for phase-locking with the additional frequency.

Technical comments

There are several minor grammatical mistakes throughout the article, which need to be addressed: Line 308: as seen in figure 8

Thank you for pointing out this typo. We changed that.

line 31 ('turbine excitation "triggers" wake meandering')

Thank you for this remark. We changed it accordingly.

line 101 ('dynamic variation')

Thank you. We corrected it.

line 293 ('does not apply to')

Thank you for this remark. We changed it.

line 308 ('as seen in')

Thank you for pointing out this typo. We changed that.

line 396 (is 'data basis' a correct word?)

Thank you. We removed "data" from the sentence.

line 411 (sounds a bit repetitive)

Thank you for pointing this out. This is a mistake and was also clarified in the comments of Reviewer 1. We corrected the second sentence with "x/D=5.0".

line 439 (the link is missing)

Thank you for pointing this out; we added the final link.