Authors’ response to Referee 1

General

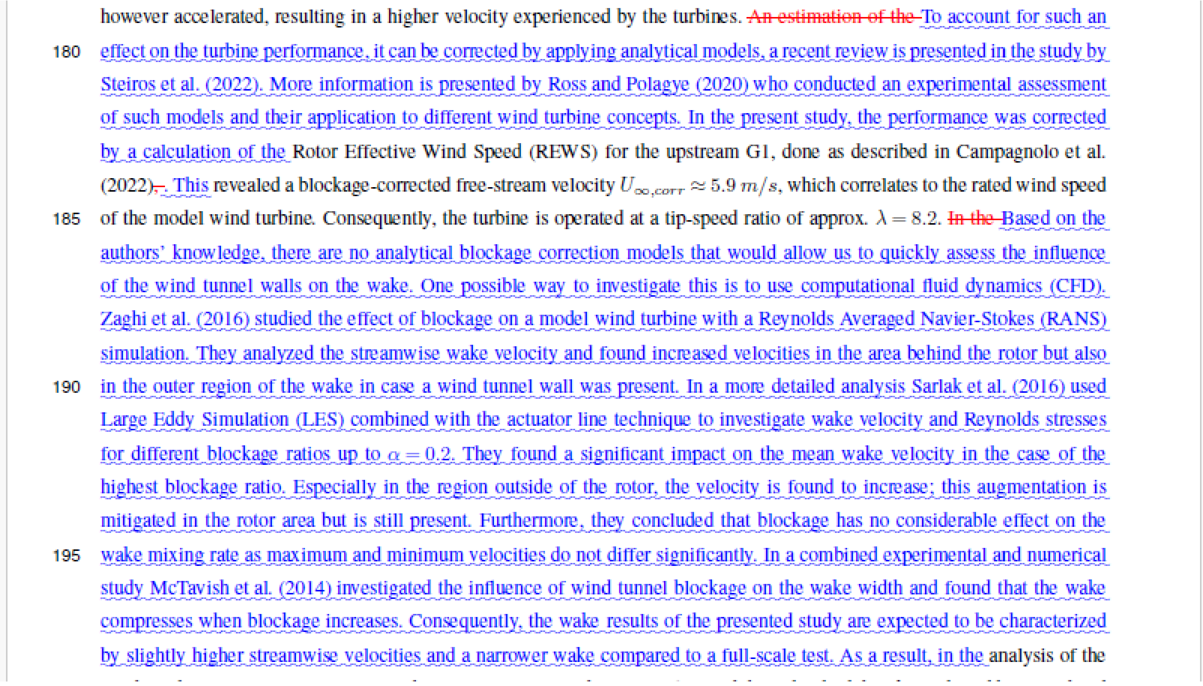
The paper „Wind tunnel investigations of an individual pitch control strategy for wind farm power optimization“ investigates the impact of the Helix approach for wake control on the total power output of two turbines in tandem configuration as well as the effect of the Helix control on the wake development. The paper is well written and the analysis presented gives good insight in what is happening in the manipulated wake. The procedure of the Helix control and the contribution of the respective Nevertheless, there are a few points the authors should address and clarify, respectively.

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 #1'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) **The authors mention, that the blockage in this experiment is rather high and present a correction for the inflow velocity. Since that correction is needed, can the authors also say anything about the possible impact of the blockage on the results of the measured wake? It would be helpful to get a feeling if these results are representative for the Helix control or if it might be an artefact. Are there aspects from other investigations with lower blockage that are comparable?**

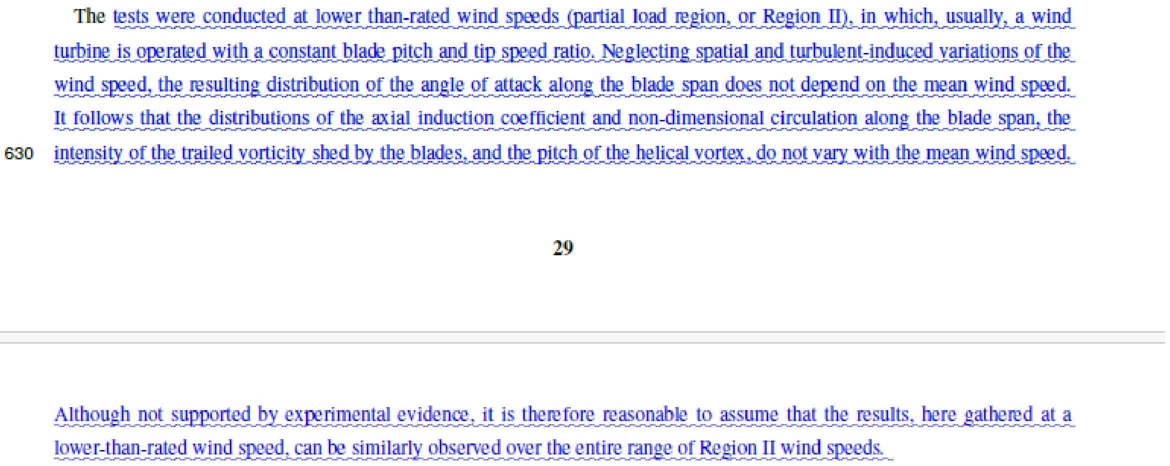
   Thank you for this important comment. The same concern was also raised by Reviewer #2 and Reviewer #3. We agree that the blockage is very high and was not discussed appropriately. We added information about the blockage effect in section 3.2. We included a paragraph in which we present several studies 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 authors mention that they expect an impact of higher turbulence intensity on the results with which I totally agree. What about different wind velocities? I assume that this control is applied in the partial load region. Do the authors expect comparable results for the complete wind velocity range in the partial load region?

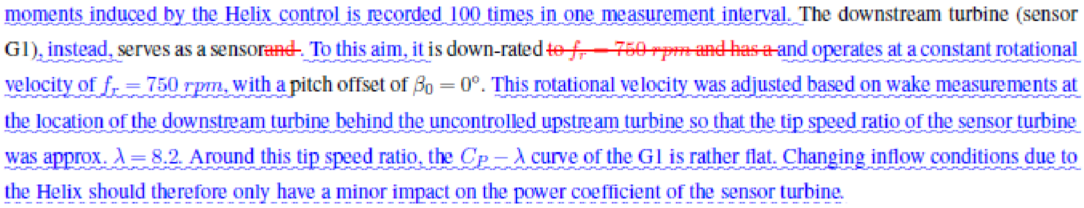
Thank you for pointing this out. We added a paragraph within the conclusions that discusses the expected behavior of Helix for the complete wind velocity range in the partial load region.



The tests were conducted at lower than-rated wind speeds (partial load region, or Region II), in which, usually, a wind turbine is operated with a constant blade pitch and tip speed ratio. Neglecting spatial and turbulent-induced variations of the wind speed, the resulting distribution of the angle of attack along the blade span does not depend on the mean wind speed. It follows that the distributions of the axial induction coefficient and non-dimensional circulation along the blade span, the intensity of the trailed vorticity shed by the blades, and the pitch of the helical vortex, do not vary with the mean wind speed. Although not supported by experimental evidence, it is therefore reasonable to assume that the results, here gathered at a lower-than-rated wind speed, can be similarly observed over the entire range of Region II wind speeds.

3) When looking at the total power of the two turbines the second turbine is also running at a constant rotational velocity. The second turbine clearly sees different and non-uniform and temporally changing inflow condition to which an activated turbine control would react to. Did the authors try to activate the „normal" control of their turbine? What effect did that have on the total power? Why did they decide to run the second turbine also at a constant rotational frequency?

Thank you for pointing this out. We agree that the sensor turbine sees varying inflow conditions. We did not activate the “normal” control of the downstream turbine because we decided to use it purely as a sensor turbine and did not want to change anything in the turbine's operation so that it has the same conditions for all investigated cases. To account for the lower wake velocity, we performed velocity measurements at the location of the downstream turbine before the tests of the tandem setup with the upstream turbine not controlled. This was then used to adjust the operational settings of the downstream turbine. We did not test it with the “normal” control active, but we think the effect on the total power would only be minor, as the C_P-Lambda curve of the G1 is relatively flat around TSR=8.2. We added an explanation for this in section 3.3.



moments induced by the Helix control is recorded 100 times in one measurement interval. The downstream turbine (sensor G1), instead, serves as a sensor~~and~~. To this aim, it is down-rated ~~to $f_r = 750\ rpm$ and has a~~ and operates at a constant rotational velocity of $f_r = 750\ rpm$, with a pitch offset of $\beta_0 = 0°$. This rotational velocity was adjusted based on wake measurements at the location of the downstream turbine behind the uncontrolled upstream turbine so that the tip speed ratio of the sensor turbine was approx. $\lambda = 8.2$. Around this tip speed ratio, the $C_P - \lambda$ curve of the G1 is rather flat. Changing inflow conditions due to the Helix should therefore only have a minor impact on the power coefficient of the sensor turbine.

4) **The traversing system and the support of the five hole probe looks pretty massive in figure 2 and can affect the measurements. Can the authors give more details about the distance of the sensing head to the support? Are they sure that the measurements are not influenced by the traverse?**

Thank you for this hint. We carefully studied the design of the wind tunnel traversing system and the probe mount. In the last 15 years, multiple studies conducted with this traversing system have been published (mostly by researchers at the Chair of Aerodynamics and Fluid Mechanics at TUM). Furthermore, more recent studies show a comparison of the FRAP/5-hole probe and a triple-wire hot wire probe with CFD results (e.g. studies by Ruhland et al. (PDF) TRANSPORT AIRCRAFT WING INVESTIGATIONS AIMED ON WAKE VORTEX IMPACT BY OSCILLATING FLAPS (researchgate.net) and Experimental and numerical analysis of wake vortex evolution behind transport aircraft with oscillating flaps | Request PDF (researchgate.net)). Here, we could show that the disturbances at the probe tip are minimal.

5) **Section 3.4 gives many details on the five hole probe and the calibration. Since these details can be found in other also mentioned publications, I recommend to leave that section out of the paper since it is not contributing to the overall story.**

Thank you for this comment. We removed this section, added the following lines in the “Measurement stages” section, and cited the respective research.

The spatial and temporal characteristics and the underlying calibration process of the FRAP can be found in the literature (see Heckmeier et al. (2019); Heckmeier and Breitsamter (2020); Heckmeier et al. (2021); Heckmeier (2022)). A high spatial accuracy below 0.2° in both flow angles and 0.1 m/s in the reconstructed velocity can be achieved. The spatial and temporal

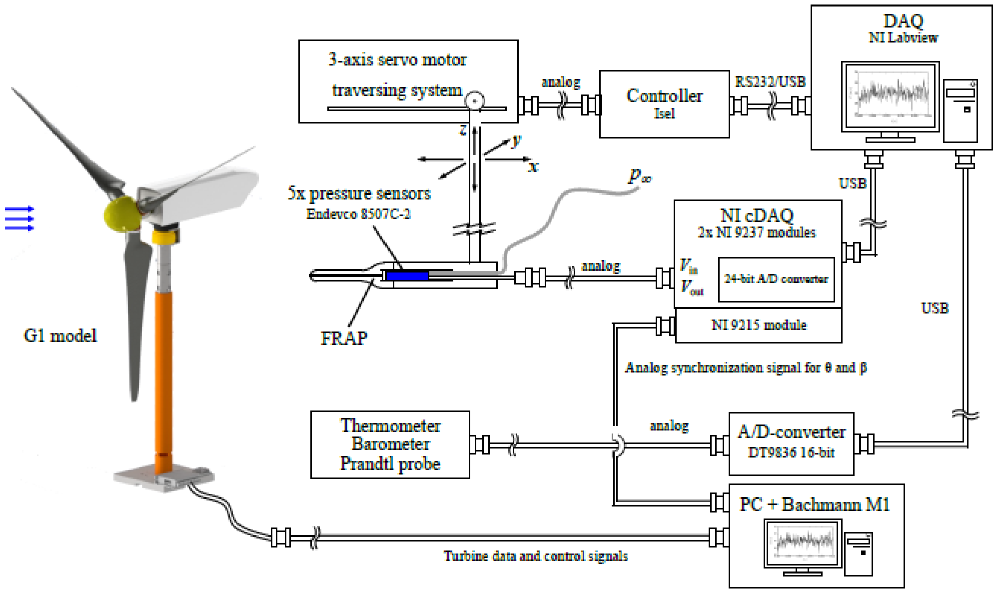
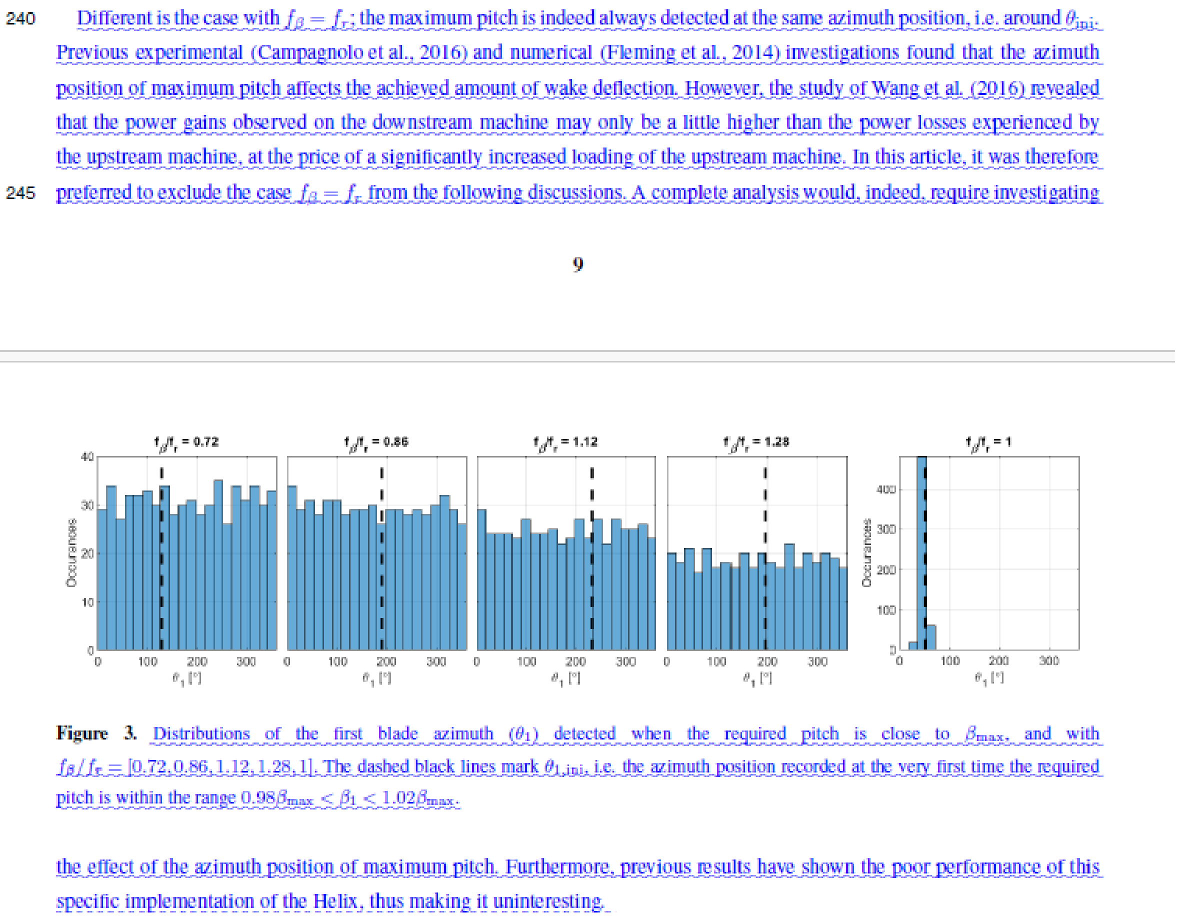


Figure 4. Schematic fast-response five-hole probe and G1 wind turbine model measurement setup for TUM-AER W/T-C

resolution of the applied FRAP has been investigated and hence, shows the suitability of the usage of the FRAP for this experiment.

6) **In figure 8, the authors observe a ditch in the thrust coefficient for St_add = 0. Do they have any idea where this is coming from?**

Thank you for this question. The case with St_add = 0 has been already studied in previous publications (Wang, Bottasso, & Campagnolo, 2017) (Fleming, et al., 2014). We added a paragraph in section 3.3 that quickly summarizes the results found therein, which showed poor performance of this specific implementation of the Helix, as well as its dependency on the azimuthal position of maximum pitch (something we did not investigate in our work). For these reasons, we decided not to include, in the rest of the paper, the data we gathered with St_add=0.



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 $\beta_{\max}$, and with $f_\beta/f_r = [0.72, 0.86, 1.12, 1.28, 1]$. The dashed black lines mark $\theta_{1,\mathrm{ini}}$, i.e. the azimuth position recorded at the very first time the required pitch is within the range $0.98\beta_{\max} < \beta_1 < 1.02\beta_{\max}$.

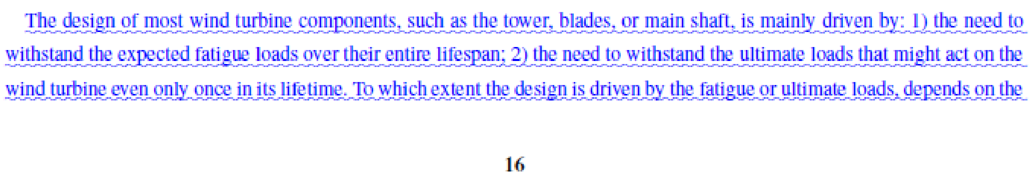
the effect of the azimuth position of maximum pitch. Furthermore, previous results have shown the poor performance of this specific implementation of the Helix, thus making it uninteresting.

7) **In figure 9 the authors leave out the results for St_add = 0. They mention, that it more or less matched the results from another investigation but without load feedback. I don't understand why this one point right in the middle of the curve should be left out. I would highly recommend to add that point also to provide the complete picture.**

Thank you for this question. See reply to point 6.

8) **The presented DELs show a clear increase especially for the first turbine. Unfortunately I have not knowledge or feeling what that would mean e.g. for a real turbine. Even though the authors state in the end, that the results can not directly be applied to real turbines they should at least discuss what an increase of the DELs would have for consequences for real turbines. Would that be a total show stopper for already existing machines or would that addition be within a range that is accounted for in the current design process?**

The design of most wind turbine components, such as the tower, blades, or main shaft, is mainly driven by: 1) the need to withstand the expected fatigue loads over their entire lifespan; 2) the need to withstand the ultimate loads that might act on the wind turbine even only once in its lifetime. To which extent the design is driven by the fatigue or ultimate loads, depends on the machine itself and the component under consideration. The impact of the increased fatigue loads induced by the Helix should, therefore, be assessed specifically for each machine on which it is to be implemented, and can be more or less significant depending on the role played by fatigue in the design process. We added this paragraph to the section 4.1 of the paper.



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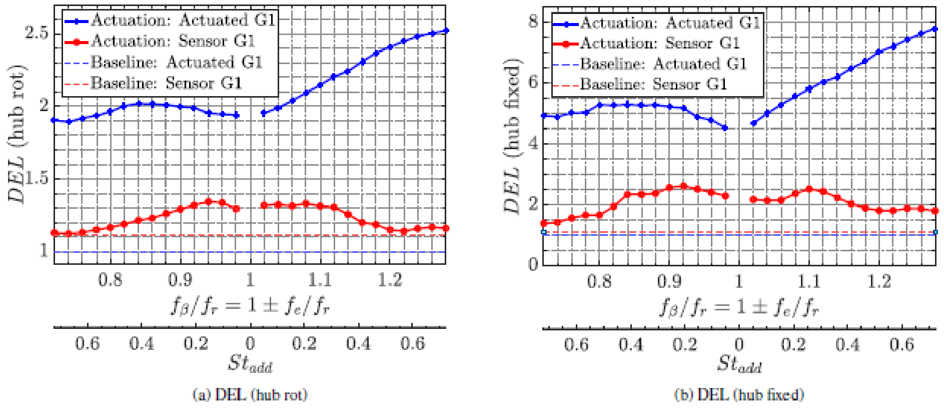
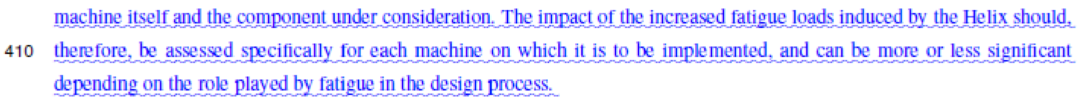


(a) DEL (hub rot)

(b) DEL (hub fixed)

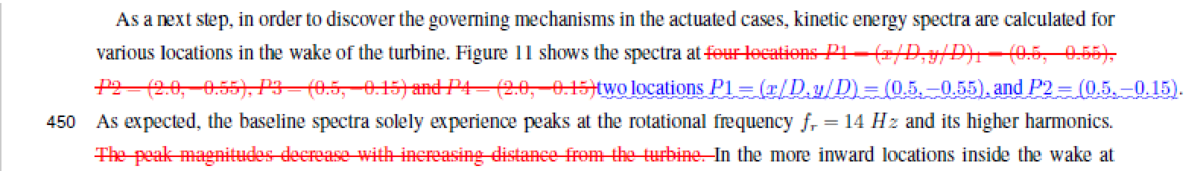
Figure 8. Normalized DELs of the upstream/actuated turbine and downstream/sensor turbine for a) rotating hub and b) fixed hub, for changing pitch frequencies $f_\beta/f_r = (0.72:0.02:1.28)$ compared to the baseline case without any actuation $f_\beta = 0$ (dashed line).



machine itself and the component under consideration. The impact of the increased fatigue loads induced by the Helix should, therefore, be assessed specifically for each machine on which it is to be implemented, and can be more or less significant depending on the role played by fatigue in the design process.

9) In the text (page 17, lower part) the authors mention four points P1 to P4 for which the spectra are presented in figure 12. Figure 12 only shows 2 points closer to the turbine x = 0.5D and not x = 2D.

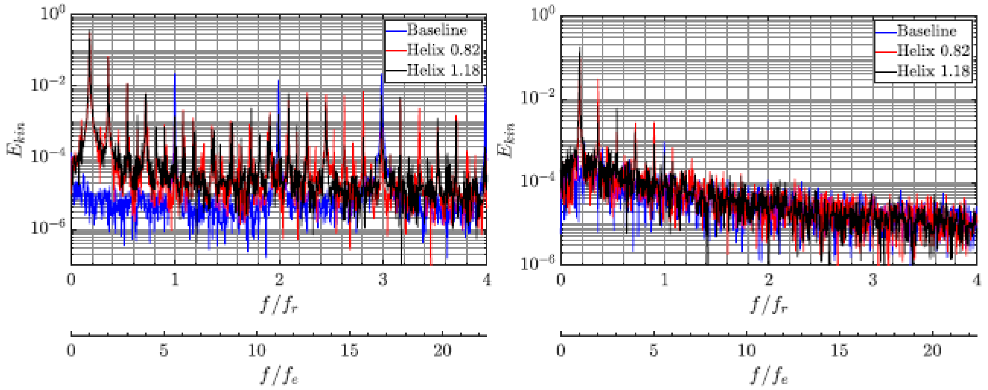
Thank you for this comment. In the final version, we opted to only show the points at x=0.5D. These two points give insight in the signal content in the frequency domain, which is further described in the text. We decided not to show the farther downstream points to reduce the length of the paper. However, the other two points are covered in the dissertation of one of our authors (Florian Heckmeier, mediaTUM - Medien- und Publikationsserver). We changed the text accordingly.



As a next step, in order to discover the governing mechanisms in the actuated cases, kinetic energy spectra are calculated for various locations in the wake of the turbine. Figure 11 shows the spectra at ~~four locations $P1 = (x/D, y/D)_1 = (0.5, -0.55)$, $P2 = (2.0, -0.55)$, $P3 = (0.5, -0.15)$ and $P4 = (2.0, -0.15)$~~two locations $P1 = (x/D, y/D) = (0.5, -0.55)$, and $P2 = (0.5, -0.15)$. As expected, the baseline spectra solely experience peaks at the rotational frequency $f_r = 14\ Hz$ and its higher harmonics. ~~The peak magnitudes decrease with increasing distance from the turbine.~~ In the more inward locations inside the wake at

10) Shifting the spectra in figure 12 vertically could help to better show the peaks for the different conditions.

Thank you for pointing this out. We adapted the y-axis range for both plots to better show the peaks:



(a) $x/D = 0.5$ and $y/D = -0.55$

(b) $x/D = 0.5$ and $y/D = -0.15$

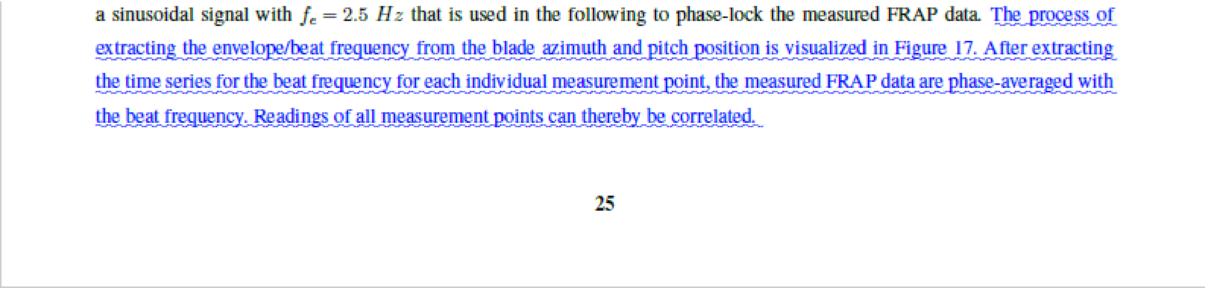
Figure 11. Kinetic energy spectra E_{kin} at various measurement locations in the turbine wake at $x/D = 0.5$ and $y/D = \{-0.15, -0.55\}$.

11) Line 465 second sentence: Why does the measured data solely consist of the azimuthal and the blade 1 pitch position? Why blade 1? Even though the data is phase-locked analysed, the resulting wake is a results of the complete rotor. Am I wrong?

Thank you for this question. Unfortunately, we think this is a misunderstanding. The acquired data has two entries for blade 1, one for the azimuthal position and one for the blade pitch. The data for the other blades can be deduced by a) the geometric properties of the 3 bladed turbine and b) by the actuation law as introduced in the theoretical part. We hope that by this answer, we can clarify your concerns.

12) In section 4.2.3 the authors explain how they got the data for the phase-locked data with the additional frequency. I think it could help to show graphically how they get the beat frequency and how they apply this to the five hole data.

Thank you for this remark. We added some explanation and a figure in section 4.2.3 showing how the envelope/beat frequency signal is extracted from the blade azimuthal and pitch position.



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.

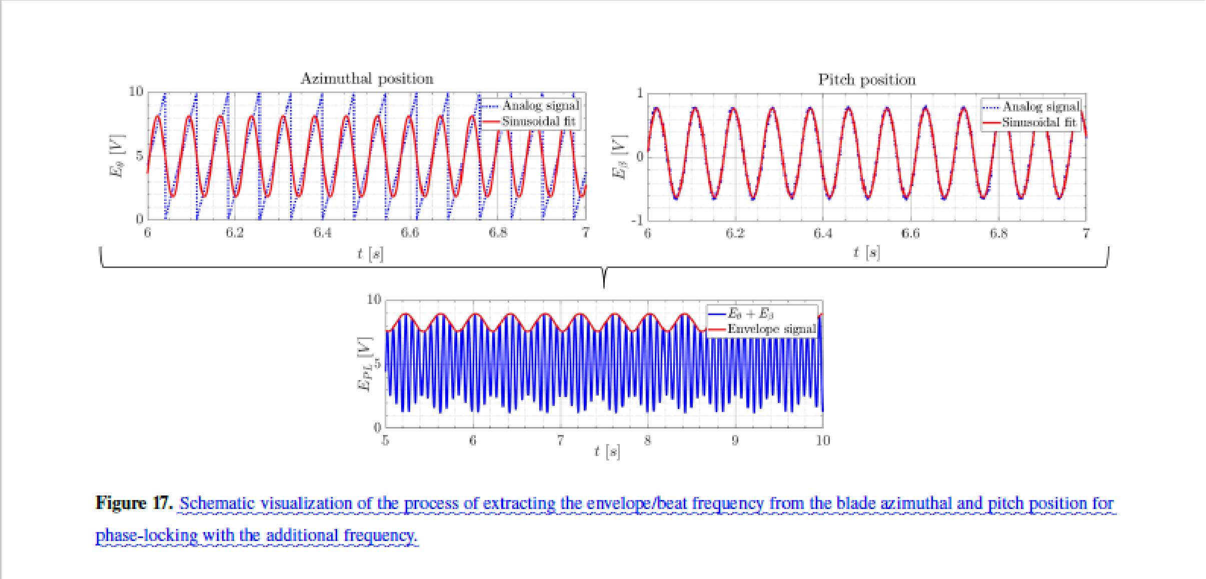
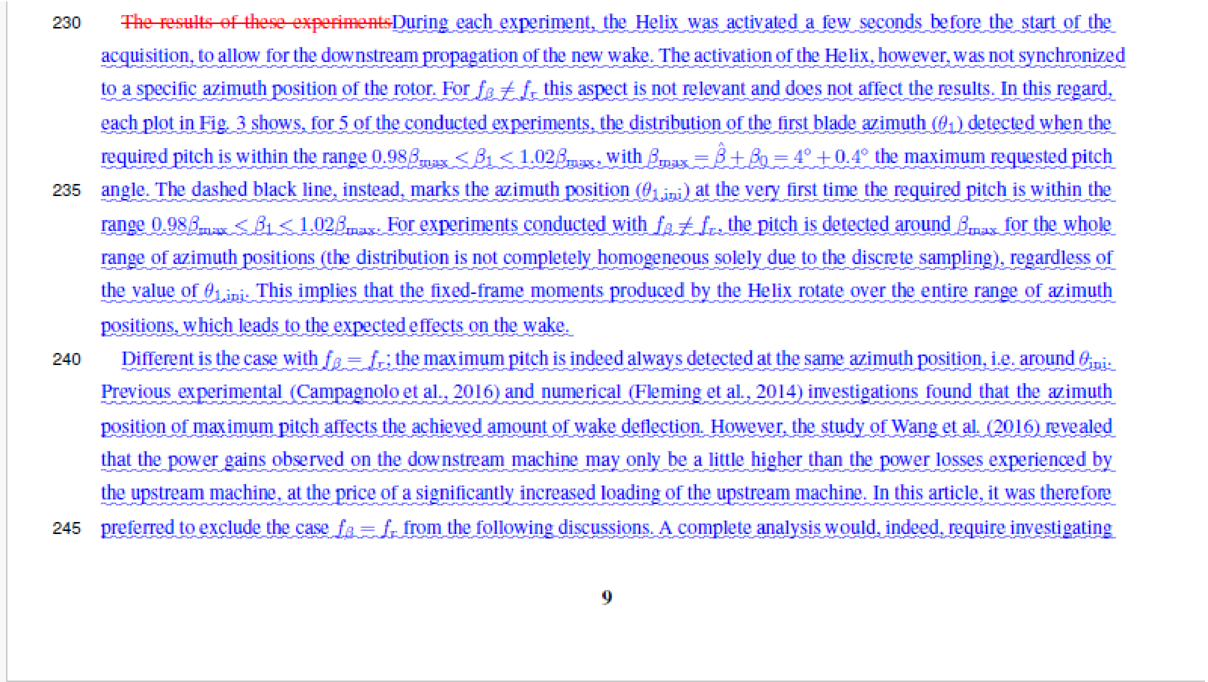


Figure 17. Schematic visualization of the process of extracting the envelope/beat frequency from the blade azimuthal and pitch position for phase-locking with the additional frequency.

13) For this analysis, how did you make sure that the pitching starts at the same position of the blades?

Thank you for this question. We added a paragraph, in section 3.3, that discusses about this aspect.



~~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 $(\theta_{1,\mathrm{ini}})$ 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 $\beta_{\max}$ for the whole range of azimuth positions (the distribution is not completely homogeneous solely due to the discrete sampling), regardless of the value of $\theta_{1,\mathrm{ini}}$. 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

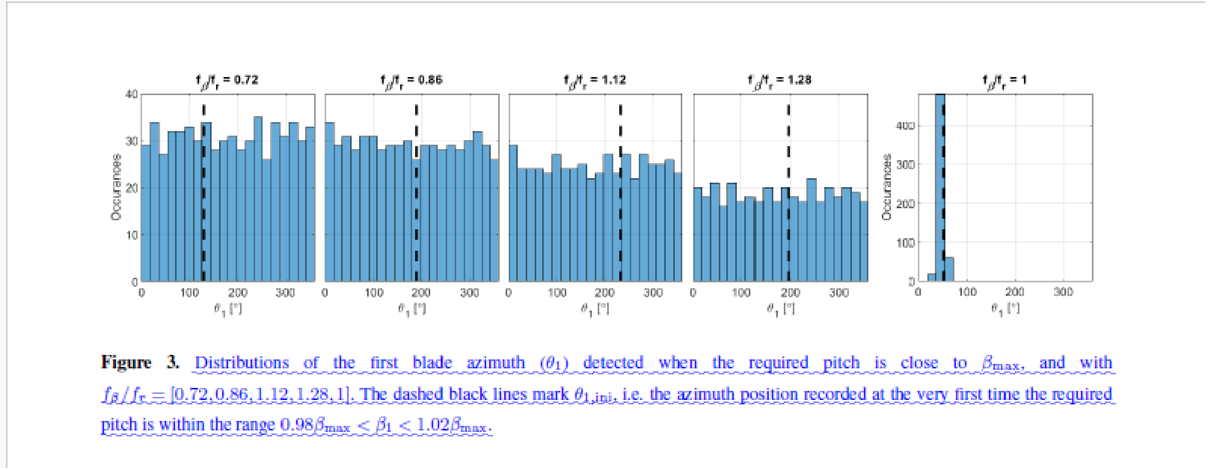


Figure 3. Distributions of the first blade azimuth (θ_1) detected when the required pitch is close to $\beta_{\max}$, and with $f_\beta / f_r = [0.72, 0.86, 1.12, 1.28, 1]$. The dashed black lines mark $\theta_{1,\mathrm{ini}}$, i.e. the azimuth position recorded at the very first time the required pitch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

14) Is there any reason, why the authors do not perform the same analysis with the fluctuations for the phase-locked with additional frequency?

Thank you for this question. We also analysed the streamwise fluctuations *u'*$_{rms}$. However, the results of this analysis did not reveal additional information to the ones of the vorticity. As you can see in Figure 1, where the streamwise fluctuations for the Helix case 1.18 are presented, the pronounced area is slightly wider. Still, they show the same meandering as the ones of the vorticity, presented in the paper. As we did not want to further extend the size of the article, we did not consider this analysis adding new information. Consequently, we decided not to show these plots and present an analysis. We added a sentence in section 4.2.3.

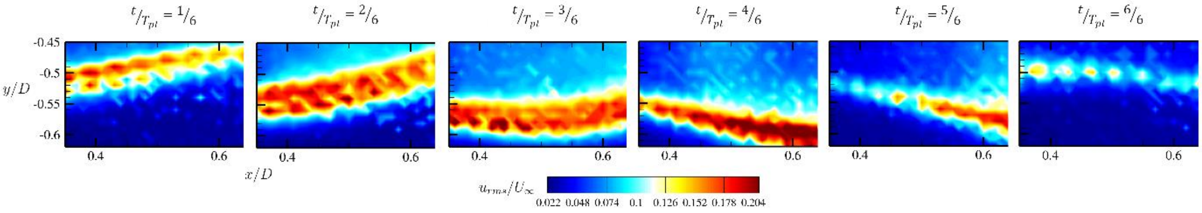


Figure 1. Streamwise fluctuations u'$_{rms}$/U∞ for the Helix 1.18 case.

which are representative ~~for~~ of the phase-locking analysis with the ~~azimuthal position.~~ azimuth position. An analysis of the streamwise fluctuations u'_{rms}/U_∞ phase-locked with the additional frequency f_e resulted in similar plots, showing the same meandering motion and are for this reason not presented here. The figures show snapshots of multiple phase-locked positions

15) **The authors state that the meandering of the tip vortices in the blade tip region is the main driver for the increase wake mixing. Following their analysis, I can only conclude that there is a meandering of the blade tip vortices. By performing the same analysis for other St_add, for wich the total power is not or just slightly increased, should provide clear evidence that this meandering is either not there or decreased. The authors should add such an analysis.**

Thank you for pointing out this. We agree that such an analysis would be very interesting and help clarify if the meandering is the main driver. When we conducted the experimental campaign, we decided to perform detailed wake measurements for the conditions where the highest effects on the wake were expected. By doing this, we hoped we could explain the mechanisms best. After such a study and getting more insight into the topic, having data for other frequencies would also be interesting. Unfortunately, it is impossible to perform another experimental analysis on this. Nevertheless, we are working on an article investigating the Helix technique for a 2-bladed rotor numerically. Figure 2 shows results of the wake, for different frequencies for CW and CCW rotation. For f_θ/f_r = 0.82 and f_θ/f_r = 1.18, which are similar to the optimum cases of this article, we see significantly stronger wake meandering than f_θ/f_r = 0.9 and f_θ/f_r = 1.10. We hope this helps to confirm our assumption and clarify your concerns. Nevertheless, as this analysis is not part of our article, we weakened the statement slightly. We wrote "…the meandering of the vortex in the blade tip region is expected to be the main driver for wake mixing…" instead of "…the meandering of the vortex in the blade tip region is the main driver for wake mixing…". Furthermore, the sentence was added to the conclusions and outlook.

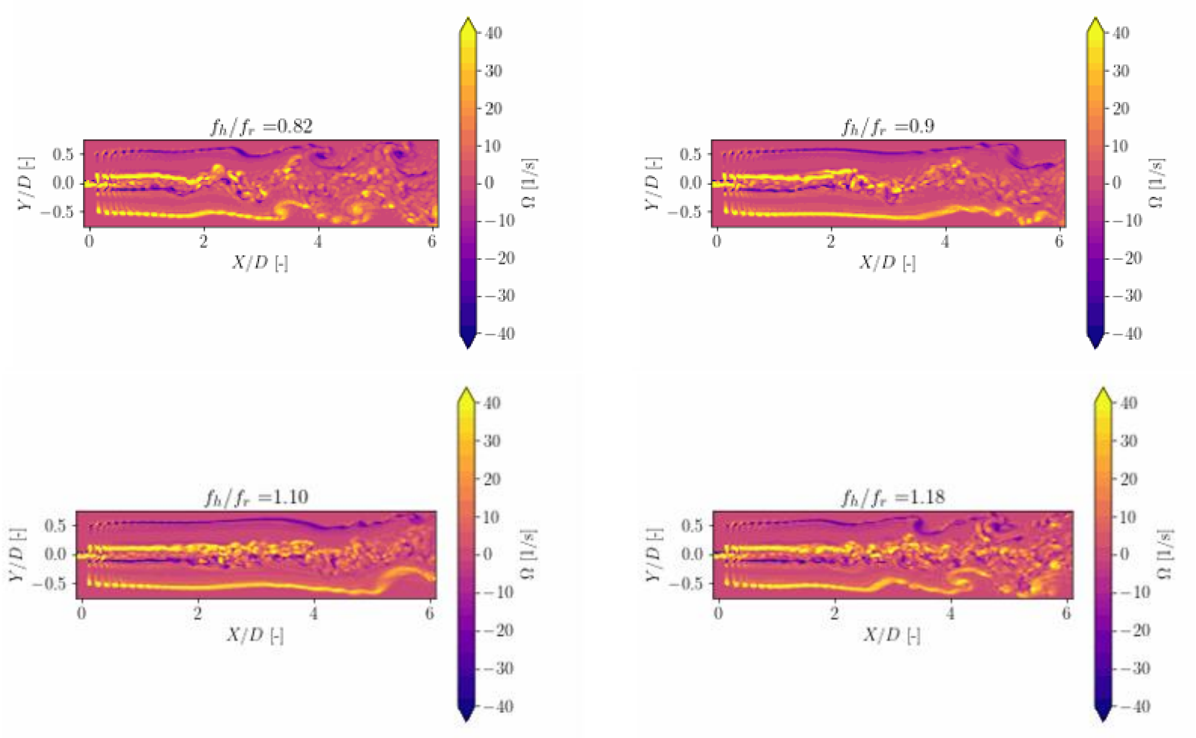
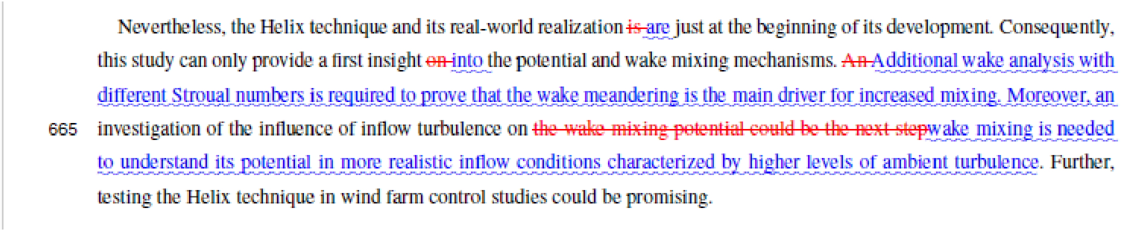


Figure 2. Results from simulations for a 2-bladed rotor operated with the Helix technique for different St_add



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.

Technical comments

The authors mix the value for the Strouhal number St_add — that value is sometimes 0.45 and sometimes 0.47. According to the definition it should be 0.47 all the time unless I missed something.

Thank you for this hint. We changed this throughout the full text to 0.47, which is, as you mentioned, the correct value following the definition of St_add.

Line 308: as seen in figure 8

Thank you for this comment. We corrected this typo.

Line 368: what does the index „1“ stand for in the definition of P1 = (x/D, y/D)_1 ??

Thank you for this comment. We intended to add this index to show that this is the x and y coordinates of point 1. However, since this could lead to misunderstandings, we removed it.

Line 411, last sentence should be x=5D for the complete merge of the tip vortices.

Thank you for this comment. We totally agree on that and changed it accordingly.

Line 439: the youtube link is not finalised.

Thank you for point this out, we added the final link.

Line 443: Isn’t the „“additional rotational frequency f_(r,a) identical to the additional excitation frequency f_e ?

Thank you for hinting at this mistake. Throughout our studies, we used several nomenclatures and this is a**n** erroneous remnant. We changed it to f_e.

Line 474: Youtube link is not finalised.

Thank you for point this out, we added the final link.

Line 493: in which an x-y plane

Thank you for this comment. We added the missing “n”.