WESD

Interactive
comment

Interactive comment on "Full field assessment of wind turbine near wake deviation in relation to yaw misalignment" *by* Juan-José Trujillo et al.

Juan-José Trujillo et al.

juan.jose.trujillo@uni-oldenburg.de

Received and published: 28 February 2016

Introduction

Dear Reviewer,

thank you very much for your comments and suggestions for improvement of this paper to which you will find answers below. Additionally you will find our proposals to take them into account in a revised version of the paper which is to be prepared after consideration of the comments of all reviewers and the editor.

Sincerely,

Juan José Trujillo and Co-authors

Major comments

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Discussion paper

Effects of thermal stability

The selected site is well documented and the measurement period is long enough to ensure statistically converged results. One could regret that no classification of data versus the thermal stability was performed. Would it be possible to do this classification and observe the consequences on the results? If not, please at least comment on it within the document. What is the average turbulence intensity at hub height?

Answer

As you point it out stability effects could be of interest for this research. Indeed at processing time we have tried to evaluate the stability at the site, however we have come to the conclusion, that the setup and the accuracy of the available data (ten minutes averages of wind anemometer and termometers ranging from 30m to 100m) was not high enough to perform a stability analysis as needed for this research. Therefore, we think that it is not sensible to perform a stability classification due to large uncertainties in its estimation. We would propose to add some explanation (see details below) in the paper about this. Finally, we will comment on the turbulence intensity. We thank you for asking about this topic which we also think should be taken into account for the preparation of future research. An early consideration of this would encourage getting the proper data for performing a more accurate analysis of the stability effects.

Background

During our analysis we performed a rough estimation of the Monin-Obukhov length (L_{MO}). As explained in the paper, the horizontal wind speed measured at different heights was fitted to Businger-Dyer model of the vertical profile including thermal effects for each ten minutes data sets. In this process the three parameters of the model, namely, L_{MO}, friction velocity and surface roughness, were estimated by means of a least squares procedure. In this way we obtained a model for wind speed extrapolation above the highest measurement height of the meteorological mast for every ten minutes data set. The obtained results for the L_{MO} showed non consistent results with

large variations and in some cases contradictory stability classification of consecutive data sets. Although this result could be due to the short averaging period of ten minutes, we interpreted this as an indication that, with the available data, the estimated stability would have a high uncertainty and therefore not contribute to an analysis with respect to stability.

Two sources support our findings about the high stability uncertainty and in some respect support our decision of not taking into account atmospheric stability in our analysis. First, Cañadillas et al. 2011 have shown that at FINO1 it is possible to obtain stability data with highly resolved sonic anemometer data with the more elaborated covariance method. Nevertheless, they found a high scatter of the results when comparing against a bulk Richardson approach including mean meteorological measurements of 30 minutes and sea surface temperature data. Second, in a personal communication with Gerald Steinfeld, Jens Tambke and Michael Schmidt, colleagues at the Energy Meteorology research group at ForWind - University of Oldenburg, who have experience with stability analysis at the FINO1 platform, they also expect a large uncertainty in the estimated stability. Moreover, they also coincide with Cañadillas that one of the main sources of uncertainty is the limitation in the setup where the lowest measurement height is too high (above 30m).

References

- Cañadillas, B.; Muñoz-Esparza, D. and Neumann, T., Fluxes Estimation and the derivation of the atmospheric Stability at the offshore mast FINO1 EWEA Offshore, 2011

Skew angle assessment

$4.1. Knowing the thrust coefficient, it is possible to assess the expected velocity deficit and so, the expected axial induction factor. One can then compare this velocity deficit with the one obtained in the present data base. One can also assess the expected

skew angle of the wake, which depends on the axial induction factor and the yaw angle. According to a rapid calculation, the skew angle of the wake should be 1.16 times higher than the yaw angle. However, this skew angle is not taken into account in the present study: why?

Answer

You are right that there is an expectation of a skew angle. As we mention in the discussion it is not possible to resolve the very small skew angles which are in consideration with the measurement setup in this research. As you point out we do not explain in detail the background of this to be concise in the paper. A solution could be to add an explanation into a short annex to the paper.

From theoretical considerations the wake deviation (χ_o) can be approximated as $\chi_o \simeq (0.6a + 1) \cdot \gamma$ (see vortex model in Burton et al. 2011) what derives into a skewness equal to $\psi \simeq 0.6a\gamma$. This behaviour is depicted in Fig. 1, where an approximate value of thrust coefficient C_T is given for the case of no misalignment. This is done taking into account that based on the vortex model the thrust coefficient is rather unaffected by yaw errors in the order of magnitude shown in Fig. 1.

The shaded area depicts approximately the yaw misalignment values which are covered during this experiment. As a consequence the highest values of skewness that could be expected are below $\psi \simeq 3°$. However, this value lies already in the order of the typical uncertainty assumed for nacelle wind vanes. Therefore we conclude that the wake skewness can not be resolved from pure yaw misalignment measurement by the standard wind vane at the nacelle in this experiment.

In regards to the lidar measurements it is not possible to assess an uncertainty, as explained in the paper, since the lidar mounting error in azimuth cannot be exactly determined. Consequently, the orientation errors cannot be diminished to isolate the systematic effect of skewness. One could be tempted to make some interpretation of the values of $\Delta\gamma$ in Tables 2 and 3 in the paper, however this would be just specula-

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tion. In conclusion, the accuracy of the experiment is not enough to resolve the wake skewness for each misalignment angle.

This is an issue which could be approached in future campaigns with the possibility to measure well defined hard targets. In this way an accurate assessment of the mounting azimuthal error of the lidar system could be performed.

Reference

- Burton, T.; Sharpe, D.; Jenkins, N. & Bossanyi, E. & Sons, J. W. (Ed.) Wind Energy Handbook John Wiley & Sons, 2001

Interpretation of very near position Figure 7: the data scatter at the first position cannot be due to a physical behaviour of the near-wake. It is unlikely that the wake changes its position in such a magnitude really close to the rotor. One might conclude that the tracking method based on a Gaussian distribution is not appropriate to capture the very near wake and one could suggest not to interpret results from this very near position

Answer

Your observation is correct in that we should not draw any interpretation out of those data, specially with respect to the variance of the position. We tried to be careful in not deriving any interpretation or conclusion from the results of position variation at that location although they are presented for completeness. Nevertheless, in Sec. 5.1.2 we do an interpretation based on the mean position at that location due to the strong consistency of the trend of all mean values to lie on a straight line. We will revise the paper for any over-interpretation based on the data at this location.

In our discussion we do mention though, that we do not have enough information to discard the Gaussian fit as an appropriate method. Mainly, one issue of the measurement method from the nacelle is the strong reduction of the interrogated area closer to

the rotor. This reduces effectively the chances of success of the fitting procedure.

Systematic shift in vane measurements $4.2. the systematic shift angle of $3°$ between the wind vane and the wake path sounds as a systematic bias in the vane measurement. This is confirmed in $5.1.2 since a bias of $3°$ is mentioned. Why not taking this bias into account from the begin- ning of the data processing in order to remove this systematic shift?

Answer Your appreciation is correct and at some point we performed such "correction" in our analysis. However, as we mention in the introductory part of Sec. 5.1.2, it is not fully accurate to claim that the wind vane is the only source of this error. Our lidar could also have some bias in its azimuthal orientation which is impossible to verify in our setup. Therefore the only choice we have is to not correct the obtained wake centre positions, but clarify the different sources of error, as we did in our discussion.

Shift/Delay $5.1.1. "Delay" sounds like a temporal characteristic whereas the authors describe a spatial behaviour. The present study does not deal with unsteady properties of the near-wake since the data are time-averaged. "Shift" might be more appropriate in the current situation. This raises also the question of the settling time of the near-wake position after a modification of the yaw angle. In the present study, the history of the misalignment is not taken into account.

Answer

As you suggest, the word "Shift" seems more appropriate in the light of the steady character of the flow which is observed here.

In relation to settling time, you are right that a more accurate analysis should take either into account only situations with no yawing at all or take careful care of the manoeuvres. However, in our case we assume that this activity is not affecting the results due to the following reasons:

- Yaw manoeuvres under normal operation are typically of very small angular val-

ues.

- Assuming bulk wake advection speeds ranging from 3m/s to 10m/s the time for covering the 1.4D distance would range roughly from 60s to 16s. We expect that these values are well below the average periods between manoeuvres.

Minor comments

Lidar probe volume Please indicate the measurement volume (or length?) of the Lidar system : the LIDAR system spatially integrates along a line-of-sight distance, meaning that when you state to measure at a certain position, it is in reality an space average of along a given distance.

Answer

The measurements were performed with a pulse of 200ns and a range gate width of 100ns. With this setup the manufacturer estimates a probe length of approximately 34m. We will add a table with specifications of the lidar setup.

Typos Line 250 : "Vortices "instead of "vortexes"

Answer

It seems that "vortices" are winning the race against "vortexes" in google. Thanks for the tip.

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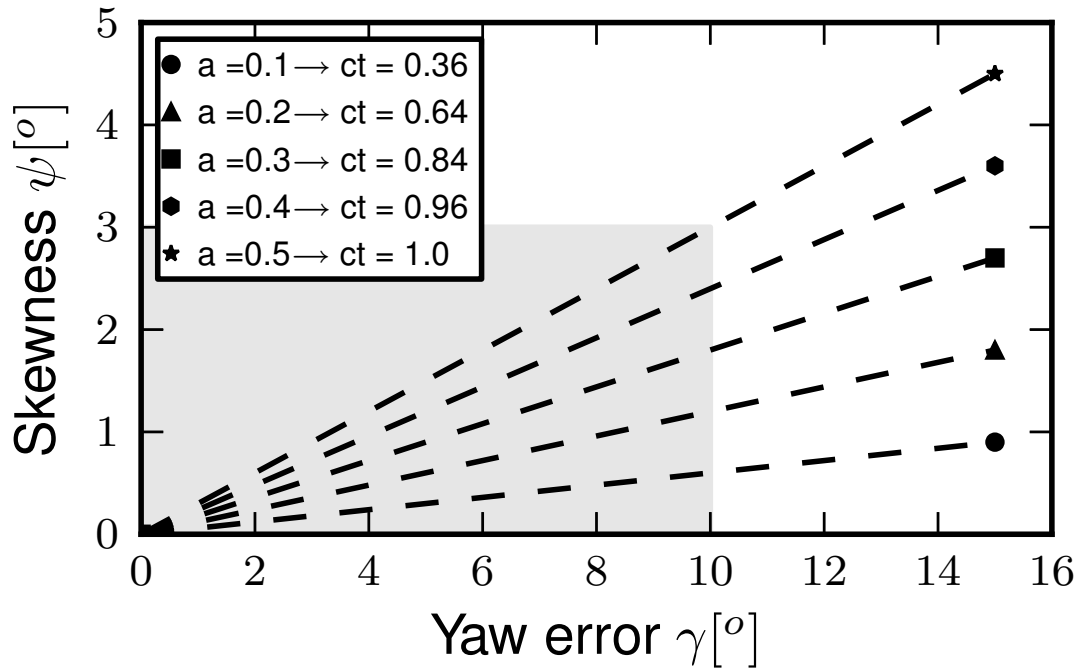Interactive comment on Wind Energ. Sci. Discuss., doi:10.5194/wes-2015-5, 2016.

Fig. 1. Approximated theoretical expectation of the effect of wind turbine misalignment on wake skewness with respect to induction (a) or thrust coefficient (Ct)