

Interactive comment on “First Identification and Quantification of Detached Tip Vortices Behind a WEC Using Fixed Wing UAS” by Moritz Mauz et al.

Anonymous Referee #2

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I agree with the comments from RC1, so I will focus on some of the other things I noted. I have written down some general comments below, followed by a list of specific comments.

General comments:

As it is, I found the article hard to follow, which is unfortunate since the results are interesting. The authors should focus on guiding the reader through their thoughts and results, in a clear and concise way, reducing the length of the paper. This will require a major revision of the paper.

I also believe the authors should spend an important amount on time correcting the language of the article. I've highlighted a couple of paragraph and sentences that

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could be improved up to page 11, after which I stopped mentioning these. The authors should still work on the text after page 11.

Regarding the title, I would probably not support using the word "first" in the title since it brings a competitive touch to it that is unnecessary in my opinion. Also in light of the following publication, it may unfortunately not be justified to claim this "first" attribute: F. Carbajo Fuertes et al - 2019 - "Multirotor UAV based platform for the measurement of atmospheric turbulence: validation and signature detection of tip vortices of wind turbine blades.". The author may also consider the studies from, Kocer et al. 2011 and Reuder and Jonassen 2012 cited in the above reference. (Please note that I'm not an author of any of these papers). Yet, I leave this up to the authors and the editor to decide whether to change the title.

I would personally prefer the equations to be closer to the text. As it is now, the equations are usually floating at the end of the paragraph which can make the discussions hard to follow.

It appears that the method presented can be attributed to Fischenberg, and the author may need to be clearer when highlighting if something is new or unique in their approach compared to what was already published (apart from the measurement campaign). The model using a regularized vortex cannot really be seen as a new contribution or method. The experimental data though are of high value.

The amount of measurements appear unclear, and some statistical analysis and information about the ensemble of results available could be valuable. Reading the article, it seems that only one vortex was analysed. Further data with different distances downstream should be incorporated since according to section 2 more data was acquired. Statistical tools should also be used to mention the uncertainty on the fitted parameters and to quantify the error between the model and the measurements.

The figures are usually clear. The authors could yet reduce the number of figures, particularly in the first 10 pages, or by combining the measurements with the fitted

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model in figures 10-17.

I hope my numerous comments will not discourage the authors, and I strongly encourage them to further work on this paper. As I mentioned earlier, the article has some great potential, it just needs some additional work to reach the point of publication.

Below is a list of more specific comments:

Astract: The statement in the abstract "the BH model can be used to describe wake vorticies" is probably too strong and would need to be moderated since this simpler model is not capturing all the dynamics. I will comment more on this later.

Introduction: I would think that bringing the context of Germany appears too specific, since the wind energy sector is growing in other countries.

p1 I21: "In research..." this sentence and the following two are hard to read and could be reformulated

p2 I8: The scaling problem of wind tunnel measurements could be mentioned here

p2 I10: make sure the acronym for UAS (and other acronyms) is made explicit in the introduction

p2 I15: Could you mention the arguments for the offshore comparison. It probably relies on arguments on the boundary layer when the flow comes from the shore, but wave exciting the turbines and the surface roughness may be different.

p2 I18: "The project aims for save helicopter flight paths in off-shore wind energy parks " needs reformulation

p2 I19, I21 *University of* Munich, University *of* Stuttgart.

p2 I23: the wind turbine also generates strong coherent vorticies, can these be considered turbulence?

p2 I30: could you highlight more the difference between the study from Subramanian

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and yours?

p3 l7: Wind speed and directions could have changed during this 15min period, do you have access to measurements to support this assumption?

p5 l11: You could mention that the ring vortex is an approximation of the wake vorticity at high tip-speed ratio.

p5 l14: This line can be reformulated to mention that this result is true under the vortex ring assumption.

p6 Figure 4: "recreation" may not be the correct word in the caption, maybe "model", or "reproduction" would be more accurate?

Figure 7: Instead of using north/east for the axis wouldn't it be easier in that case to use an orientation in the frame of the turbine, with y pointing upstream against the main wind direction? You could then remove the sentence at the end of page 6.

p7 line1-12: The potential flow assumption probably appears too early in this paragraph and the paragraph could be reformulated. The definition of circulation as function of the vorticity is independent of this assumption. Equation (3) only uses an axi-symmetric assumption. It is yet true that the circulation of a vortex makes more sense in inviscid flows where the vorticity is condensed to confined singular regions.

p10 l1-4: This paragraph needs should be reformulated, the language improved.

p10 l9-15 and p11 l1-8: While reading the text I was confused since figure 9b didn't appear to be mentioned. The explanation could be improved by clearly explaining both figures and both scenario, before mentioning figure 10. Alternatively, you could tell the reader to focus only on figure 9a for now and figure 9b will be explained later. Also, equations 8-10 could be introduced first before drawing the conclusions that there is no unique solution. Further, the way the equations are introduced can be improved by telling to the reader what is coming, e.g. "The velocities at point 1 and 2 are...". Right now, they appear in the text announced.

p11 I9: " This double peak can be lead back to passing the maximum tangential velocity at $r = r_c$ at position 1 and 2". The sentence may need reformulation

p11 I12-15: Similar to the previous remark, the equations can be introduced before drawing the conclusions.

p11: "As shown above the presence and identification of a vortex (or a pair of vortices) is measurable". The language needs to be reformulated (one cannot measure an identification). Also, the previous section seemed to show that in some cases the determination was not possible, which would imply that the identification is not always possible.

p13 I8: It would help the reader to provide some information about the measurement campaigns (how many samples were selected, what were the mean conditions), and some introduction about the samples you selected (and why you selected them) to present in the paper.

p14 I26: It is not clear why you mention the skewed vortex at this stage. You may need to guide the reader. Also, the definition of the skewed vortex on figure 15 appears unclear. From the figure it seems that the vortex is simply rotated. I'm not sure this qualifies as a skewed vortex. Also the 2D cut appears to have some 3D aspect to it, which can be confusing. Introducing a coordinate system on the 3D vortex on the 2D cut can help the understanding.

p14 I30: I am wondering if "analytical reconstruction" is the proper term and how this "reconstruction" is different from the previous section. The parameters you extracted from the measurements were fitted to an analytical model. The "reconstructed" vortex is this analytically modeled vortex, and it is intrinsically part of the results you presented in table 2. If I understand this correctly, it could make sense to have the modelled vortex directly on the figures 13-15, as "fitted vortices".

p15 I9: What is meant by "artificially induced drop" of velocity? Does this refer to drop

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of velocity in the turbine wake? If this is so, the drop in velocity should be a function of the thrust coefficient at the rotor, and a value of 65% may not be comparable to other measurements unless they are at the same operating conditions.

p18 eq20: You may have to introduce all symbols closer to the equation, even if these are obvious.

p19 l1-4: It appears surprising that the authors do not have more information about the turbine (thrust curve, pitch curve). Earlier in the text, it was mentioned that a model of the turbine was done. These quantities can then be obtained from a Blade Element Momentum code.

The argument here may simply be that most turbine have a pitch angle around +/- 1 degree below rated, and in the absence of data, you picked 0 degree. It is also not clear where the pitch angle enters in the equation. Most likely an argument of the thrust coefficient, but usually you'll have a thrust coefficient vs wind speed curve available for that turbine.

p19 l10-11: These sentences needs to be moderated. First, it appears that the study was only done on one vortex at a given operating conditions and a more quantitative analysis would be required. Second, it appear wrong to state that the wake of a turbine is described by two vorticies. You could clarify your discussions based on the following considerations. The fact that two vortices are crossed on the trajectory of the drone is due to the likelihood of crossing the tip-vortices from different blades. This likelihood increases as the number of blades or the tip-speed ratio increases. When it is such, the wake vorticity surface can be approximated to a vortex cylinder, in which case any trajectory of the drone will indeed cross the tip-vorticity surface twice. This cylindrical surface does not ressemble two vorticies spinning in opposite direction and the wake dynamcs cannot be described by assuming that it consists of two vorticies. What the author probably mean is that the velocity field accross a tip-vortex (or a tip-vorticity surface) ressembles the one of a regularized point vortex. This analogy (which is natural

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given the different analytical vortex wake models of wind turbines) cannot be used to "describe" the wake, but it can be used to "estimate" some of the wake properties, that is, the tip-vortex core radius and circulation.

p19 I21: The identification of one vortex strength do not appear to be enough to draw a conclusion, or the conclusion needs to be moderated. Also, it may not be necessary to attribute this equation to Sorensen et al and instead it can be mentioned where this formula comes from: circulation for a rotor of constant thrust coefficient (This should also be mentioned earlier in the text p18 I1-6).

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