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

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Author's response to Referee #2

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Thank you for the detailed review of the manuscript. In the following I will comment on each point. The referee's comments will be repeated in italic before the answer.

General comments

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. 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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the language of the article. I've highlighted a couple of paragraph and sentences that could be improved up to page 11, after which I stopped mentioning these. The authors should still work on the text after page 11.
I will look into that.

- *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 detect ion 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.*
  Thank you for your valuable input there. The paper by Carbajo Fuertes was put online on March 28 2019. So there was no possibility to the authors to have known from these results.
  Following your suggestion I had a look at Fuertes 2019. The results look good. It is nice to see that two different approaches and measurement systems come to a similar result and output. Fuertes also derives a circulation of $50\ m^2/s$ from his measurements. Given that he measured at a different wind turbine, the circulation strength is of the same order of magnitude. However, I could not find any information about the vortex fit he is using for his measurements as well as a detailed information about the circulation calculation.

- *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.
I will consider this point to be mainly to be an editorial problem. I will see, to put equations closer to the text. However, in the later two-column layout everything will change again. So I might need input from the editor at this point.

- *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.*
It is correct that the method and model is not new. But as I state that the model has its origin in aviation, it is new to be used to describe wind turbine wake vortices properties. I added a paragraph to highlight this new approach in WEC wake studying.
Also the described method illustrates how to obtain results from in-situ line measurements of a UAS, including systematic measurement uncertainties like skewed/canted vortex hoses etc.

- *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.*
A new subsection '2.3 Available data' has been added. I also state why only one pair of vortices could be used for an evaluation. A simple vortex measurement

in the wake is not sufficient, also the criterion $\Delta y < r_\mathrm{c}$ must be met which is a rare condition. I comment on this in the 'specific comment' section below in more detail.

- *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 model in figures 10-17.*
  I will take out some figures from the manuscript. But also some new ones will be added. Mainly to do the uncertainty evaluation (wind angles, true air speed, etc.). I will also try if some figures could be combined. But that might add to the readers confusion.

- *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.*
  Highly appreciated emphasis. Thank you!

Specific comments

Abstract

1. *The statement in the abstract 'the BH model can be used to describe wake vortices' 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.*
   I will add the simplifications that have to be made. The BH model is also only a

tool to describe the vortex geometry in a 2D cut. This point might need additional stressing.

Introduction

2. *I would think that bringing the context of Germany appears too specific, since the wind energy sector is growing in other countries.*
   This is indeed true. The introduction has been 'internationalised'.

3. *p1 l21: "In research..." this sentence and the following two are hard to read and could be reformulated*
   The abstract as well as the introduction were rewritten.

4. *p2 l8: The scaling problem of wind tunnel measurements could be mentioned here*
   Thank you for the hint. It will be mentioned.

5. *p2 l10: make sure the acronym for UAS (and other acronyms) is made explicit in the introduction*
   Has been implemented.

6. *p2 l15: 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.*
   Yes, there is definitely an influence through the surface roughness compared to the sea. It can also be seen in Fig. 5 that the horizontal wind in a certain altitude experiences much more fluctuations. So there is an internal boundary (surface) layer that shows increased turbulence with a more stable marine layer on top. This was only mentioned since for the briefly presented HeliOW project it would be desirable to fly and measure behind an actual off-shore wind turbine. But for

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Discussion paper

several practical reasons this was simply not feasible.
The influence of turbulence is also a good point for future measurements to get a large amount of vortex measurements to do some statistics.

7. *p2 l18: "The project aims for safe helicopter flight paths in off-shore wind energy parks" needs reformulation*
   Will be changed to "The goal of the project is also to determine save helicopter flight paths in off-shore wind energy parks".

8. *p2 l19, l21 *University of* Munich, University *of* Stuttgart.*
   Has been changed accordingly.

9. *p2 l23: the wind turbine also generates strong coherent vorticies, can these be considered turbulence?*
   Coherent (in this case artificial turbulent) structures are not turbulence covered by Kolmogorov 1941. The coherent vortices might be considered as a large scale anisotrop turbulence. The surrounding atmosphere in the best case scenario can be considered as isotrop. In the far wake these coherent structures decay and add to the isotrop turbulence.

10. *p2 l30: could you highlight more the difference between the study from Subramanian and yours?*
    Will do. Subramanian et al.detected tip vortices by a pressure signal along a longitudinal flight path. The vortices where also not quantified (vortex strength, core radius). I will add some more details to the manuscript.

Section 2

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

Unfortunately, we didn't have access to highly resolved SCADA data for the measurement time which could have given us precise information about wind speed and direction changes. Moreover, we were flying about 27 m behind a 114 m diameter WEC. If the wind direction would have changed significantly we might have crashed into the rotor blades. For a single vortex measurement it is also not important, if the wind direction varies a bit. The wind velocity component rotation later in the manuscript was done for the single measurement. The mean wind direction is only important to set up the flight trajectory for the UAS.

Section 3

12. *p5 l11: You could mention that the ring vortex is an approximation of the wake vorticity at high tip-speed ratio.*
    Thank you for the input. I will mention that.

13. *p5 l14: This line can be reformulated to mention that this result is true under the vortex ring assumption.*
    The line has been modified.

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

15. *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.*
    This was actual the intent when planing the flight path at the field campaign. The wind was slowly turning. So Figure 7 shows the actual flight path (direction) of the UAS. So y pointing north is almost into the main wind direction. But at the

time of the measurement the wind direction was already a bit off. Also the post-processing software of the UAS data always writes the wind velocities according to the meteorological standards u pointing east, v pointing north and w pointing upwards. So the data in the end always has to be rotated into the main wind direction. And this is necessary to later make $\overline{u'} = 0$ in the wake.

16. *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.*
   I will look into a possible reformulation and implementation of the introduction of potential flow.

17. *p10 l1-4: This paragraph needs should be reformulated, the language improved.*
   The paragraph has been rewritten.

18. *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.*
   Figure 9b is mentioned on page 11 l7. I agree that the two different cases of passing a vortex in the UAS measurements needs additional addressing to not lose the reader.

19. *p11 l9: "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
Changed to: This double peak is caused by passing the maximum tangential velocity at $r = r_c$ at position 1 and 2".

20. *p11 l12-15: Similar to the previous remark, the equations can be introduced before drawing the conclusions.*
I think this comes down to personal preference. I would like to explain what's happening when passing the vortex at $\Delta y < r_\mathrm{c}$ and follow up with what that means for the previously introduces equations.

21. *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.*
Fair point. "As shown above the presence is measurable and a later identification of a vortex (or a pair of vortices) possible when the $\Delta y < r_\mathrm{c}$ is met."

Section 4

22. *p13 l8: 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.*
This issue will be addressed with the additional subsection about data availability.

23. *p14 l26: 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.
Maybe 'canted' or 'oblique' vortex is the more precise expression. The additional 3D features are meant to help the reader. That is why the blue is more transparent. It could be removed, if it confuses more than it helps?!
I have to introduce the oblique vortex hose, since the real (measurement) world is different from the analytical approximation (ring vortex). I will try to make that more clear.

24. *p14 l30: 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 modelled 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".*
In Section 3 I want to present the theory/method how to "reconstruct" the vortex from GPS and pressure data. In section 4 the individual fit is calculated. One might consider to just remove this subsection, e.g. Fuertes 2019 never introduces any vortex model of the WEC and just "fitted" the data.
As for the second point sadly the GPS standard deviation is in the range of the vortex core radius. So the double peak is not visible and overall the accuracy is not up to the in-situ, in-line pressure measurement. Thus, to keep an step-by-step approach and to use the analytical model as a proof of concept I would like to keep it separate from the measurement.

25. *p15 l9: What is meant by "artificially induced drop" of velocity? Does this refer to drop 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.
You are correct, the statement has been changed. The actual deficit in percent or m/s is only important for the correction of the modelled v component.

Section 5

26. *p18 eq20: You may have to introduce all symbols closer to the equation, even if these are obvious.*
I will try to do so.

27. *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.
The engineering sector of wind energy converters is a highly competitive market. Although the University of Stuttgart has a generic recreation of the wind turbine, it is reserved for wake computations. The resulting aerodynamic properties of the turbine are however strongly confidential. That's why those properties, namely here the thrust coefficient, had to be estimated. As stated on p. 19 l1, we approximated the thrust coefficient vs. wind speed with an NREL 5 MW turbine. Since the E112 and NREL 5MW wind turbines both have similar rotor diameters and produce a similar amount of electricity, the approximation was judged reasonable.

28. *p19 l10-11: These sentences need 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 appears wrong to state that the wake of a turbine is described by two vortices. 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 resemble two vortices spinning in opposite direction and the wake dynamics cannot be described by assuming that it consists of two vortices. What the author probably means is that the velocity field across a tip-vortex (or a tip-vorticity surface) resembles the one of a regularized point vortex. This analogy (which is natural 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.
I agree. This paragraph needs to be more precise. I will implement your suggestions.

Section 6

29. *p19 l21: The identification of one vortex strength does 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 l1-6).*
The conclusions will be readdressed and the statement moderated. Also I want to repeat at this point (and later in the new version of the conclusions) that the fact that we hit two (!) consecutive vortices in one flight leg at the crucial crite-

rion ($\Delta y < r_c$) must also be considered lucky. That both measurements, after consideration of the non-ideal conditions (canted vortex hose) and therefore the rotation of the horizontal measurements into the vortex plane is giving two circulations strengths that match the theoretical calculation, needs an appropriate (strong) statement.