Responses to Reviewers' Comments for Manuscript

Rotary-wing drone-induced flow – comparison of simulations with lidar measurements.

Addressed Comments for Publication to

Atmospheric Measurement Techniques

by

Liqin Jin, Mauro Ghirardelli, Jakob Mann, Mikael Sjöholm, Stephan T. Kral, and Joachim Reuder

Dear Reviewers,

Please find enclosed the revised version of our manuscript entitled "Rotary-wing drone-induced flow – comparison of simulations with lidar measurements". We would like to express our gratitude to you both for providing valuable comments that have greatly contributed to the improvement of our work. In this revision, we have carefully considered and addressed the comments raised, aiming to enhance the quality and clarity of the manuscript.

Below, we provide a summary of the main modifications made and a detailed point-by-point response to your comments. We sincerely appreciate your time and effort in reviewing our manuscript, and we are grateful for the opportunity to address your concerns.

Sincerely,

Liqin Jin, Mauro Ghirardelli, Jakob Mann, Mikael Sjöholm, Stephan T. Kral, and Joachim Reuder

Authors' Response to Reviewer II

> **General Comments.** Jin et al. present a method to determine the flow around a rotary-wing UAS in the field. Such experiments are extremely valuable, and the solution to use a triple-Doppler short-range lidar setup is innovative and unique. It is very difficult to get such data in other ways. Despite the very good concept, I think the study misses a lot of opportunities for a more profound analysis. From the manuscript I cannot conclude if CFD simulations are good enough to study the flow around a multicopter well enough for sensor placement in all relevant conditions. It also remains unclear if the results can be transferred to other conditions and other platforms. I can only recommend the manuscript for publication in AMT after major revision. I give some general and specific comments below:

Response:

Dear Reviewer, we appreciate the time and effort that you have dedicated to providing your insightful comments on our paper. After reading through your comments, we implemented our manuscript including your suggestions. We will now address, one by one, your comments and suggestions.

> ### Comment 1
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> The authors motivate the work with wind energy research. The experiment is carried out at wind speeds below 4.1 m/s. Values of turbulence intensity are not even given. These are not conditions that are relevant for wind energy. I understand that these are conditions that are critical for sensor placement, but if the application is at higher wind speeds, it would be important to know if the sensor placement can be changed if low wind speeds are disregarded.

Response:

Thank you for your insightful comments. For wind energy applications, high wind speeds are essential, and our previous study concluded that high wind speeds facilitate optimal

sensor displacement upwind, as the downwash is pushed downstream. However, we believe that understanding sensor behavior at low wind speeds is equally important, especially to ensure accurate measurements across a variety of meteorological conditions. Drone-induced flow is a distinct challenge associated with low wind speeds, making them a worse scenario that must be studied in order to develop comprehensive sensor placement strategies.

In addition, while wind energy is undoubtedly a key domain for the application of our drone-based research, our overarching aim is broader. We seek to develop a multi-copter-based system capable of resolving turbulence across a wide spectrum of wind speeds and atmospheric conditions. This was not stated clearly in the previous version of the manuscript. Therefore, we have expanded the scope of our paper to include a comprehensive study of boundary layer meteorology.

We have implemented this comment in the manuscript now by modifying the introduction part. We have revised the first paragraph as The proper characterization of atmospheric turbulence is essential to understanding the structure and dynamics of the atmospheric boundary layer (ABL) [Stull, 1988, Wyngaard, 2010]. It is therefore crucial to obtain accurate wind and temperature measurements with high spatial and temporal resolution for a variety of basic and applied ABL research topics, such as weather and climate prediction [Teixeira et al., 2008], wind energy meteorology [Emeis, 2010, Albornoz et al., 2022], and atmospheric modelling [Etling, 1996].

> **Comment 2**
>
> To my understanding all the analysis is based on two short measurement periods. This seems very little in order to draw general conclusions.

Response:

Thank you for pointing this out. We acknowledge that our study is based on two relatively short measurement periods. As we mentioned in the manuscript the drone has a nominal maximum flight time of 45 minutes. As the field experiment was conducted at low

temperatures, the maximum flight time was reduced considerably. In order to maintain the same ambient conditions and calibration of the drone system, we decided to measure the flow at different heights during each of the flights, instead of doing one position for each flight. This ended in us positioning the drone at 4 different heights for 5 minutes each. Then, we analyzed all the lidar data we had during the post-processing of plan scans, involving three- to four-minute data. For the line scans, we analyzed 3-min lidar data instead of 1-min data. From the error bars in Figure 11 and 12, we found the results by CFD simulations and lidar measurements are comparable.

We understand that more extended measurement periods may provide a more robust dataset and allow for more definitive conclusions. It is also important to consider the fluctuations in wind speed, which may lead to bias if the measurements are taken over a long period of time.

> ### Comment 3
>
> One main difference between the field experiment and the CFD which it is compared to is in my opinion the turbulence. The authors say that the k-epsilon model is used in RANS, but do not give details about the parameters that are set in the model setup (i.e. boundary conditions for k and epsilon) and how they compare to the measurements (I did not find it in Ghirardelli 2023 either). How do the results depend on the turbulence settings?

Response:

Thank you very much for this comment. In our CFD analysis, we employed the standard k-ϵ turbulence model in ANSYS Fluent, utilizing its default parameter settings. We made this decision based on some important considerations that are both crucial to the integrity and relevance of our research. Firstly, the use of standardized settings offers the advantage of consistency, fostering comparability with other studies employing similar methodologies. As the default parameters provide a pragmatic balance between customization and realism, they are a pragmatic solution to the complexity of adjusting

k and ϵ values for each unique simulation scenario. Therefore, we added this point in the manuscript in **L124** as <u>Following the initial mesh setup and selection of the turbulence model, we used standard settings from Ansys Fluent to ensure consistency and reliability.</u>

Additionally, our study goes beyond the turbulence modeling to encompass more overarching flow characteristics and systemic behaviors. We have calculated the turbulence intensity (TI) based on the lidar data, which is 0.04. This means that, in achieving our research objectives, complex adjustments to turbulence model parameters are unlikely to yield significant improvements.

> **Comment 4**
>
> I think an opportunity is missed to measure the downdraft with the sonic anemometers and the short-range lidars at the same time with a lower hover altitude above the sonics. This would give more confidence in the measurements and the comparison to the simulations.

Response:

Thank you for this nice comment. We agree. However, the sonic anemometer on the small met mast used in this study doesn't work. In our previous study [Jin et al., 2023], we focused three CW lidars very close to a mast-mounted sonic anemometer and one lidar's probe length is about 1.2 m, which is slightly larger than that of this study (0.56 to 0.87 m). The 50 Hz times series of wind velocity measured by lidars and the sonic anemometer agree very well in Figure 11, Table 4 and 5. The velocity differences relative to the sonic velocity could be 0.18% at no precipitation period. Therefore, as validation method, we used three lidars to compare wind velocity on horizontal planes and lines with CFD simulations, which is not possible with single-point measurements from sonic anemometers.

> ## Comment 5
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> Is it really necessary to describe how wind is retrieved from Doppler lidar spectra in this study?

Response:

Thank you very much for pointing this out. We agree with this comment and delete the wind velocity retrieval process in the manuscript.

> ## Comment 6
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> Section 2.2 I think that the WindScanner system is well described in the literature and since it is only used for validation measurements here, a specification of the uncertainty and reference to the literature would be sufficient. The data processing description could be omitted in my opinion.

Response:

Thank you very much for this comment. We agree with this point and deleted the detailed introduction of WindScanner system. Instead, we explained the reason why we use three lidars and the potential measurements errors. The new paragraph from **L128** to **L136** is The ground-based, short-range WindScanner system developed by DTU Wind and Energy Systems consists of three synchronized coherent CW Doppler lidars (Fig. 3), which are capable of accurately retrieving wind vectors and measuring turbulence [Sjöholm et al., 2009, Mikkelsen et al., 2020, Jin et al., 2023]. A single CW Doppler lidar can only measure the one-dimensional projection v_{LOS} of wind velocity vector along its line-of-sight beam direction. Therefore, by combining the independent and simultaneous measurements of v_{LOS} from three Doppler lidars, a full three-dimensional wind vector can be retrieved.

The use of CW Doppler lidars is beneficial for a variety of wind energy applications. In spite of this, CW Doppler lidars are susceptible to moving objects away from the intended focus point, such as flying birds. Besides, their spatial resolution decreases

as the focus distance increases, which may deteriorate the accuracy of wind velocity measurements by CW lidars [Jin et al., 2022b]. Therefore, we placed the three lidars as close to the intended scanning positions as possible to compact the measurement volume [Angelou et al., 2012] minimize potential biases resulting from volume-averaging [Clive, 2008, Sjöholm et al., 2009, Forsting et al., 2017]. To improve the accuracy of flow velocity retrieval, we discard spectra containing Doppler shifts caused by hard targets and out-of-focus moving objects during the post-processing [Jin et al., 2022a].

Because the other reviewer asked us to provide clearer explanations for critical aspects such as sampling frequency, sampling time, and numerical simulation parameters. Therefore, we would like to keep he data processing description. Hope this can be accepted by you.

Comment 7

Is a 5-m distance to the UAV a realistic position for a sensor in flight? What does that mean for the weight of the system, the flight time, the stability?

Response:

Thank you for your insightful comment. We acknowledge that positioning a sensor 5 meters away from the UAV is not practical for in-flight operations due to significant implications on the system's weight, flight time, and stability. This recommendation stems from CFD simulations and lidar measurements under very low wind speeds conditions, aiming at achieving less than 1% velocity difference relative to the free flow, both horizontally and vertically. As wind speed increases, the aerodynamic interference from the drone diminishes, allowing for closer sensor placement without compromising the accuracy significantly. Therefore, in more typical conditions with higher wind speeds, the required distance for accurate measurements would be considerably less.

It is also important to mention that the 1% distortion threshold is particularly stringent, especially when considering the inherent measurement inaccuracies of state-of-the-art sensors in case of low wind. This level of precision, although ideal, is beyond the capabilities of current technology in most practical scenarios. Thus, while our study

provides a theoretical basis for optimal sensor placement under various conditions, the real-world applicability must consider a balance between ideal accuracy and operational feasibility.

We have implemented this comment in the revised manuscript in **L283**, which is It is worth noting that there is still a disturbance in the vertical wind component of about 1% even at an upwind distance of 5 m, with the drone's diameter of 1.88 m and a propeller size of 0.71 m. Mounting a 5-meter boom on such a drone is virtually impractical. However, this considerable distance is the result of selecting a stringent threshold of 1% disturbance. For wind speeds lower than 4 ms^{-1}, 1% velocity deviation is less than many full-size sonic anemometers' nominal sensitivity. Therefore, by using a less strict threshold, this 5-meter distance can be substantially reduced to 2 m, which is also possible at high wind speeds due to the propeller-induced flow shifting downstream.

> ### Comment 8
> p.1, l.6: maybe introduce the CFD simulations first, before comparing to measurements.

Response:

Thank you very much for pointing this out. We agree with this point and have introduced CFD simulations first in **L4**. The new sentence is However, propeller-induced flow may deteriorate the accuracy of free wind velocity measurements by wind sensors mounted on drones, which needs to be investigated for optimal sensor placement. Computational fluid dynamics (CFD) simulations are an alternative to experiments for studying characteristics of the propeller-induced flow, but require validation. Besides, we also changed the structure in Section 2 by introducing CFD simulations setup first.

> ### Comment 9
> p.2, l.41: what is a reference for the extensive CFD studies?

Response:

We appreciate that you point this out. We have unintentionally used the wrong phrase here. We acknowledge that CFD is a prevalent tool in UAV research. However, the specific exploration of PIF, particularly in the context of sensor placement, is not as extensively studied. We have revised the introduction section of our manuscript to more accurately reflect the current state of research in this area. This amendment clarifies that our study addresses a unique aspect of UAV research, specifically focusing on optimizing sensor placement in relation to PIF, while majority of studies have primarily examined how PIF impacts drones' flight stability for design purposes [Zheng et al., 2018, Guillermo et al., 2018, Lei and Lin, 2019, Guo et al., 2020].

We believe this revision provides a more accurate representation of the literature and better contextualizes the contribution of our work by filling a research gap in this field. We are grateful for your insightful comments, which have undoubtedly helped improve the quality and accuracy of our manuscript.

> **Comment 10**
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> p.2, l.45f: I think it would be better to give a distance relative to the rotor diameter, like in the text before.

Response:

Thank you very much for this comment. We agree with this point and add the information of the distance relative to the rotor diameter. Now the new sentence in **L50** is Vasiljević et al. [2020] presented a proof-of-concept drone–lidar system and concluded that the lidar should be placed out of the drone's disturbance zone stretching between 1 and 2 m (1.9 D and 3.7 D with rotor diameters of 0.53 m) from the center, based on measurements of radial wind speed.

> **Comment 11**
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> p.6, l.130f: It would be nice to know upfront what is the purpose of plane and line scan.

Response:

Thank you very much to point out this comment. We agree and have added one sentence to explain the reason to have plane and line scans in **L160**, which is The flow-field evolution in a horizontal plane at a certain distance below the drone was measured for comparison with the CFD simulations, while fast line scans enable detailed comparisons in the upstream direction, which is the most promising region for a boom-mounted sonic anemometer attached to the drone Ghirardelli et al. [2023].

> **Comment 12**
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> p.7, ll.134ff: I do not quite understand the reasoning for that pattern. If the lidars had just scanned a simple square with 20 lines (corresponding to the 20 orange dot lines) successively in y-direction, the scan would only last 20 s in my understanding. Would that not be better?

Response:

Thank you very much to point this out. It would have been good to have a shorter scanning cycle. However, if design a square with some lines successively in y-direction, we have to rotate the dual prisms very fast, which is explained in **L168** as The three laser beams followed a path that started from one side of the line, passed through the disc center to the other end of the line, and then returned over the same route to its starting point. This allows the dual-prism lidars to complete the pattern in a reasonably short time, if the symmetry axis of each lidar crosses the disc center and the trajectory in the vicinity passes exactly through the same crossing point.

> **Comment 13**
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> p.7, l.142: Was RTK GPS used in that case as mentioned in the drone description? I would not expect the drift in that case.

Response:

Yes, RTK GPS was indeed utilized as part of our drone's setup, as mentioned in the

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description. We appreciate your observation regarding the unexpected drift despite using RTK GPS. It appears that there might be some issues within our setup that led to this drift. The positioning of the drone can of course be improved by a better position estimate but it depends to a large degree on the tuning of the drone, which has admittedly not been optimal for this purpose. Ensuring accurate and reliable data collection is our top priority, and as part of this, we are committed to improving the drone's tuning.

> ### Comment 14
>
> p.10, l.176 and Fig. 7: what is the "certain threshold" and how is it determined. The numbers in Fig.7c are much higher than in 7d. While it seems obvious for 7c that hard target hits are possible, why are the areas in 7d so broad?

Response:

Thank you for this comment. The "certain threshold" is determined based on the sum of 1-min averaged power spectral density. From Figure 1(a), we can clear see that when the lidars hit the drone, the sum of the spectrum is much higher and the relatively lower values are obtained from aerosol-induced spectra. Therefore, we apply the threshold as 2 to remove spectra with very high peaks and the result is shown in Figure 1(b).
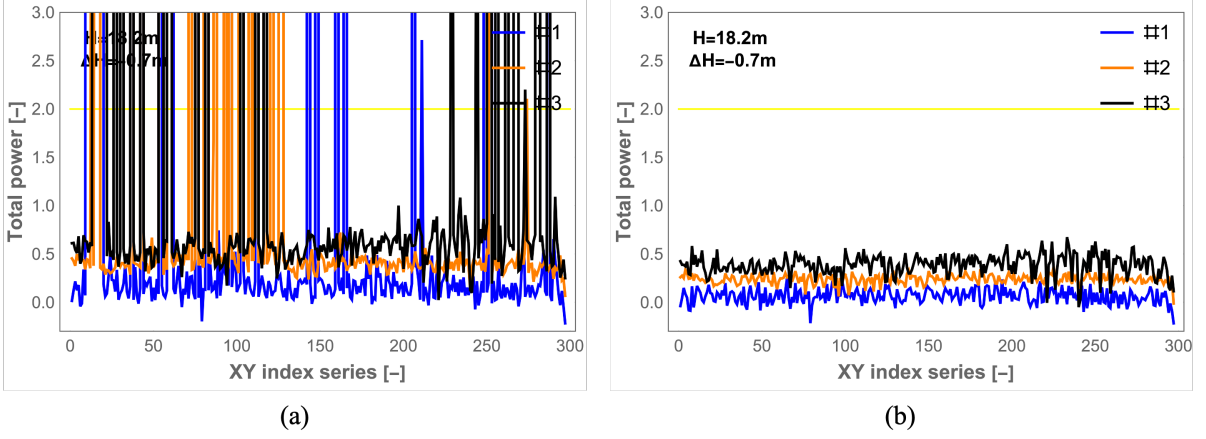
Figure 1: Sum of 1-min-average spectrum of three lidars at height $\Delta h = -0.7$ m. **(a)** Before applying threshold to remove spectra with very high peaks. **(b)** After applying threshold to remove spectra with very high peaks. The yellow indicates the threshold value, which is 2 in this case.

The numbers in Fig.7c are much higher than in 7d is because the drone in Fig.7d is much more out of focus than that in Fig.7c. Considering the probe length of the three lidars ranges from 0.56 to 0.87 m, the laser intensity drops sharply from the scanning plane and the drone in Fig.7d is further away from the focus point. The Doppler spectrum with the highest peak value and the sum of all the spectra on the scanned plane measured by lidar 3 at height $\Delta h = -0.7$ m and $\Delta h = -2.1$ m are shown in Figure 2. For Figure 2(a) where the drone is only 0.7 m above the scanning plane, the maximum spectral power is about 3000, while for Figure 2(b) where the drone is 2.1 m above the scanning plane, the highest value is about 30.
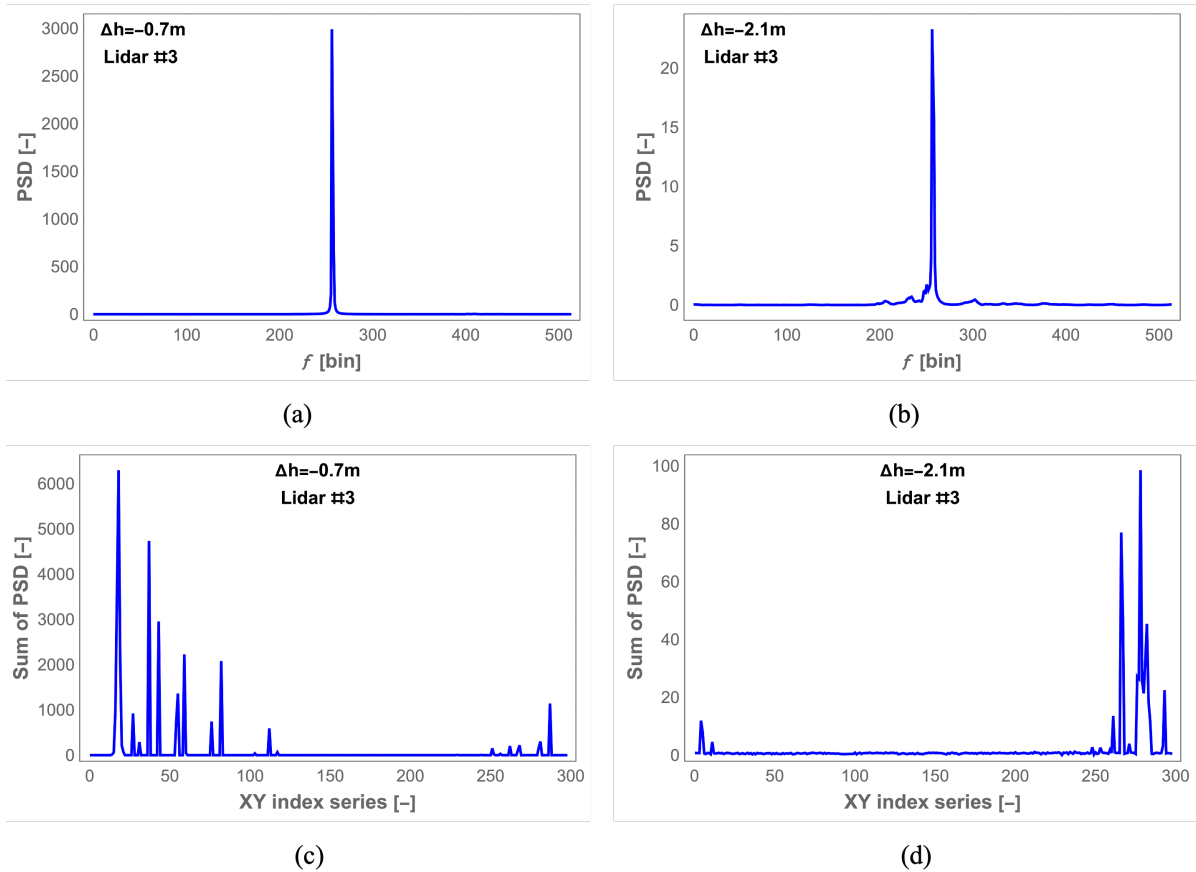
13

Figure 2: Spectrum with the highest peak value and the sum of all spectra on the scanned plane measured by lidar 3 at two heights. **(a)** Spectrum at height $\Delta h = -0.7$ m. **(b)** Spectrum at height $\Delta h = -2.1$ m. **(c)** Sum of all spectra at height $\Delta h = -0.7$ m. **(d)** Sum of all spectra at height $\Delta h = -2.1$ m.

Comment 15

p.10, l.188: Can you also give information about turbulence intensity?

Response:

Thank you for this comment. We understand the importance of this parameter in comprehensively analyzing wind patterns and their impact on our study. In response to your request, we have calculated the turbulence intensity for the plane scan and have included it in our report. You can find the detailed information in the revised manuscript

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L217. The updated text reads: The free-stream wind velocity was 4.09 ms^{-1} along the positive x-axis with a turbulence intensity (TI) of 0.04 and the drone was yawed about 21.2° to the incoming wind.

Comment 16

p.12, l.206: can the differences in the comparison between CFD and measurement also be due to effects of unmodelled atmospheric turbulence?

Response:

Thank you for this comment. Yes, we agree with this point and have implemented it in the manuscript now. The new sentences in **L224** and **L234** are It should be noted, however, that lidar measurements did not capture the very detailed flow feature due to the effects of unresolved atmospheric turbulence, the averaging effect of lidars' measurement volume, and the drone drifting approximately ±0.5 m along the free flow direction from its average position (the central black dot in Fig. 8). and This is probably due to the aforementioned effects of unsolved atmospheric turbulence, the averaging effect of lidars, and the drones' drifting.

Comment 17

p.15, l.227: Please also provide the distance relative to the drone size.

Response:

Thank you for pointing this out. We agree with this comment and incorporated this suggestion in **L264**. The revised sentence is With stronger wind speeds, the drone's downwash becomes more tilted. When a sonic anemometer is placed 0 m below the drone, the horizontal distance should be 4.8 m (6.8 D) upstream to achieve less than ±5% distortion of both horizontal and vertical wind velocities at lower ambient speed. We also added all the distance relative to the rotor diameter in the manuscript.

> **Comment 18**
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> p.15, l.235: Maybe repeat what is the criteria for the disturbance zone.

Response:

Thank you very much for this suggestion. We agree with this comment and have added the criteria for the disturbance zone in the manuscript. The revised sentences are In the case of the plane scan at 0.7 meters (1 D) below the drone, both methods demonstrate that the disturbance zone (1% difference relative to the free-stream wind velocity) for the horizontal wind velocity stretches 2 meters (2.8 D) from the drone center and more than 5 meters (7 D) for the vertical velocity component in the Conclusion part and Both methods conclude that the disturbance zone (defined by a relative deviation from the mean free-stream velocity by more than 1%) on a horizontal plane 1 D (rotor diameter D of 0.71 m) below the drone, extends about 2.8 D upstream from the drone center for the horizontal wind velocity and more than 7 D for the vertical wind velocity in the abstract.

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