

Answers to the comments of the reviewers for the manuscript “Investigation of onshore wind farm wake recovery with in-situ aircraft measurements during AWAKEN”

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by

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The authors would like to thank the reviewers for their detailed comments and suggestions to improve the manuscript. In the following, the comments are provided in *italic* letters. Each point is answered in **blue normal letters**. Changes to the text are indicated by quotation marks.

RC1: 'Comment on wes-2025-113', Anonymous Referee #1, 16 Aug 2025

The paper provides a study of wind farm wakes at two wind farms in the midwestern USA using data acquired from a test aircraft. The data collected is quite interesting and the authors investigate some nice features of the data, producing some valuable insights. This type of study, using data collected from an aircraft, is unique and will be helpful to the community.

We would like to thank the reviewer for this positive judgement of our data set and the analyses.

However, in my opinion some of the authors' conclusions are a bit too strong given the limited data available. I suggest the authors reduce the certainty of some of their conclusions. Please see my specific comments as follows:

We agree with the reviewer that our data set is limited, and weakened our conclusions as suggested.

p. 5, line 125: "low altitudes (close to hub height, between 100 m and 1000 m a.g.l.) for measurements in the ABL (Lampert et al., 2024), allowing high-resolution measurements of atmospheric and surface properties." The hub height is 89 m, so how would altitudes in excess of 100 m be "close to hub height"? An altitude of 1000 m agl would not be considered low altitude for a general aviation aircraft like the F406.

To improve clarity we changed the sentence accordingly, highlighting the low altitude at 100 m a.g.l. without describing the flight pattern which is flown up to 1000 m a.g.l.:

“The twin-engine aircraft is well suited to fly at an air speed of 65 to 70 m s⁻¹ and low altitudes of 100 m a.g.l. – close to hub height for offshore applications (Lampert et al., 2024) – allowing high-resolution measurements of atmospheric and surface properties at low altitude.”

p. 5, line 125: The authors describe how the wind vector is derived, but more information is necessary. For instance, what is meant by high precision position measurements? Is only GPS used, or tightly coupled GPS-INS? Deriving wind speed from airspeed means that you need precise ground speed measurements (to solve the wind triangle). The use of a standard aircraft-grade GPS may not be suitable for solving this problem accurately, particularly at a sample rate of 100 Hz. What make/model GPS-INS was used? Is there a reference which describes how the aircraft is instrumented,

how wind speed and turbulence was estimated, and how the data integrity/accuracy (particularly for wind speed) was verified? If not available from an external reference, that information should be provided here. The reliability and accuracy of the wind speed measurements must be established given its importance for this study.

We added Appendix A describing the wind measurements on board the research aircraft and a citation of Appendix A, see p.5 l. 128.

The term "radio altitude" should be changed to "radar altitude" throughout the paper as this is more common terminology in the aviation industry.

We changed as suggested.

Can the authors comment on the velocity deficit at 11:35 am in Figure 7 (c)? It seems there is a ~30% velocity deficit at an altitude of 350-400 m, which is over 2x tip height. I do not think this is mentioned in the text. Do the authors think this is due to the wind farm wake, or perhaps not caused by the turbines due to its height?

To improve clarity we have rewritten this section:

“First, lidar data from Site A1 (upwind of the King Plains wind farm for southerly flows) and the lidar data from Site H (downwind of the King Plains wind farm for southerly flows) are analyzed. While lidar data are generally a valuable tool for analyzing wind farm wakes, at this measurement site the lidar data lack vertical resolution, with the lowest value occurring at 110 m a.g.l., which is above hub height. The lidar data from Site A1 (see Fig. 7a) display an LLJ at around 200 m a.g.l., which dissipated after 10:05 CDT. This corresponds with the results from the aircraft data displayed in Sect. 3.1. While the lidar data from Site H (see Fig. 7b) also display an LLJ, the LLJ nose is slightly elevated compared to the LLJ at Site A1. A similar phenomenon is described in Krishnamurthy et al. (2025), where the lifting of an LLJ due to the wind farm’s internal boundary layer is described. In this study, this phenomenon will not be discussed further, as this study focuses on wind farm wakes. Figure 7c compares the lidar data at Sites A1 and H by displaying the velocity deficit of the wind speed. In order to focus on the wind farm wake, only the velocity below 200 m a.g.l. is considered. A velocity deficit above 50 % at 110 m a.g.l. at 09:05 CDT was detected. The velocity deficit is elevated between 07:35 CDT and decreases after 10:05 CDT. In connection with the vertical profiles obtained by the aircraft (see Sect. 3.1), this can be linked to a transition from a stable to an unstable boundary layer with increasing turbulence. The diurnal cycle, and in this case the morning transition, influences the wind and the behavior of the wind farm wake. “

Figure 8 is really interesting. Can the authors comment on why the sharp peak in the TKE is so far to one side of the wind farm in the 500 m plot? The velocity deficit is more "centered" on the wind farm, while the TKE plot shows a sharp peak but really far off to one side. Could this have to do with the number of turbines on that side, or the location of the turbine closest to the aircraft as it passed downwind of the wind farm (i.e., it passed close to a turbine near the edge of the farm, but those in the center were farther away)? It might be good to add a sentence or two about this feature of the plot.

Thank you for your comment, we agree. We added three sentences to improve clarity:

"In addition, Fig. 8e illustrates a significant peak in TKE at the west end of the wind farm King Plains. This structure is also visible in the wind speed measurements, displayed as gray dots, (see Fig. 8 a) and is likely linked to the layout of the wind farm. The turbines at the west end are located closer to the horizontal flight legs compared to the easterly wind turbines (see Fig. 2 b). The TKE peak can also be associated with higher wind shear at the edge of the wind farm as observed for offshore wind farms in Cañadillas et al. (2023)."

End of p. 14: I am a little uncertain about the authors' conclusions regarding Figure 10. The number of data samples is very low, yet the authors seem to draw some fairly big conclusions ("This raises the question if the wind speed is the best indicator for identifying onshore wind farm wakes.") For instance, the authors state " In an unstable stratification, the velocity deficit decreases at 5 km downwind of the wind farm and increases again at 10 km distance of the wind farm". Looking at Figure 10 c, I see 6 data points equally spaced about 0 at 5 km, and 4 points above zero and 1 well below zero at 10 km. This is not very much data in total and the effect is not statistically significant in my opinion. Overall, based on the results in Figure 10 I think the authors' claims in this paragraph are too strong. TKE and the velocity deficit both show the expected trends in terms of stratification and distance, although admittedly the data in the velocity deficit is noisier.

Thank you for this valuable comment; we weakened the claims made in this paragraph by highlighting the lack of statistical significance of the data available:

"To highlight the effect of stratification on wake recovery, Fig. 10 displays the difference in TKE within the wake and within the free flow and the velocity deficit for flights in stable, neutral and unstable ABL stratification. The stratification was derived from the TKE of a ground-based sonic anemometer at Site A1 (see Sect. 2.1). The statements presented in this section are only based on the flight measurements but are not statistically significant because of limited data availability. Figure 10d–f260 highlights the trend of a wake recovery, based on the TKE difference, with increasing distance from the wind farm and a strong wake during stable conditions close to the wind farm, a weaker wake close to the wind farm for neutral conditions and no distinct wake for unstable conditions. Previous studies of offshore wind farms have used the velocity deficit of the wind speed downwind of the wind farm and in the free flow (Krishnamurthy et al., 2017). Figure 10a–c displays the velocity deficit for the same flights and stratification. The limited amount of data weakens the conclusions drawn from these data. While the trend for the velocity-deficit-based wake recovery downwind of the wind farm is noisier compared to the TKE difference, the magnitude of the velocity deficit decreases from stable to neutral to unstable conditions. The TKE difference for stable and neutral conditions in 5 km displays higher values compared to the TKE difference in 10 km; this cannot be observed in the velocity deficit for the same conditions. The velocity deficit in 10 km is generally higher than the velocity deficit in 5 km. Because of limited data availability, more measurement flights are needed to investigate the TKE difference in addition to the velocity deficit as an indicator for wind farm wakes and their recovery."

p. 16: "The velocity deficit at 0.5 km (Fig. 12 b) and 2 km (Fig. 12 c) downwind of the wind farm is similar to the values of the homogeneous terrain at this distance from the wind farm,". The meaning of the sentence is unclear - should it be "values of velocity deficit"? In fact, the whole section following this sentence is written poorly and is difficult to follow. Please rewrite to improve clarity. Can the values in the paragraphs at the bottom of pp. 16 and 17 be presented in a table perhaps?

To improve clarity, the paragraph has been changed to:

“In contrast to the homogeneous terrain, the terrain downwind of the King Plains wind farm is characterized by a slight hill, on which the wind turbines are located, a valley at 260 m above mean sea level at approximately 5 km downwind of the wind farm and an upward slope at 320 m above mean sea level at 10 km distance downwind of the wind farm (see Fig. 12a). As described in Stull (1988) and Kaimal and Finnigan (1994), hills alter the flow by accelerating and decelerating it. Depending on the stratification of the ABL, the flow downwind of the hill will follow the terrain in stable conditions or will be lifted by buoyancy in unstable conditions. Fig. 12b–k displays the wind speed at different distances downwind of the wind farm. The wind speed displays clear wakes in all distances downwind of the wind farm. To isolate the potential influence of the terrain on the wind farm wake, Table 3 compares the velocity deficit of the legs in similar distances downwind of the King Plains wind farm (KP, semi-complex terrain) and the Armadillo Flats wind farm (AF, homogeneous terrain). The first leg of the flight in homogeneous terrain was conducted at 07:40 CDT, and the first leg of the flight in complex terrain was conducted at 07:21 CDT. The velocity deficit in 0.5 km and 2 km is of similar magnitude for the homogeneous and complex terrain. While in the homogeneous terrain the velocity deficit decreases at a distance of 4.5 km, it increases at a distance of 5 km in the complex terrain. This trend strengthens as the velocity deficit in the homogeneous terrain decreases further at a distance of 9 km from the wind farm, whereas it increases even further in the complex terrain case. In both cases the wind farm wake is not fully recovered at these distances, as the threshold for recovery is <5 %. For these two flights, the aircraft data might be able to detect an effect of the terrain on the wind farm wake, displaying an amplification of the velocity deficit with increasing distance in the semi-complex terrain.”

p. 19 line 312 sensible→sensitive

We changed as suggested.

p. 19: "As 1000 values per wind component are factored into the TKE, the TKE has more statistical significance compared to the raw wind speed measurements which are very sensitive to gusts." Why are the TKE measurements not also sensitive to gusts? This gets back to my question about how the wind speed was actually computed from the instrumentation on the aircraft. As it stands I am not convinced that the TKE measurements were necessarily more accurate than the wind speed (also not clear what is meant by "raw" wind speed - no distinction between "raw" and processed is ever defined).

Thank you for the valuable comment - indeed, some clarification is needed here.

The 3-D wind vector was derived from a tightly coupled GNSS/INS solution (iMAR RQH system) combined with air data from a calibrated 5-hole probe. The turbulent kinetic energy (TKE) was computed from the 100 Hz wind components after applying a 0.1 Hz phase neutral second-order Butterworth high-pass filter to isolate turbulence from the wind speed. This corresponds to averaging over approximately 10 s of data (about 1000 samples per component).

While instantaneous (“raw”) wind speed values (calculated as mentioned above) exhibit strong natural variability regardless of the presence of a wind farm wake, the TKE represents the variance of these fluctuations over the defined period and is statistically more significant for the detection of a wind farm wake.

At a typical airspeed of 65 ms^{-1} to 70 ms^{-1} , the 0.1 Hz cut-off corresponds to a spatial scale of roughly 600-700 m, which is within the expected integral length scales of turbulent eddies in wind-farm wakes. Hence, the chosen filtering effectively separates the mean flow from turbulent fluctuations

without averaging out the physically relevant structures in the wake.

We decided to clarify the text with replacing the phrase:

“The turbulence representative, TKE, was derived from the variance of 100 Hz wind components filtered with a 0.1 Hz cutoff, representing the energy of turbulent fluctuations over about 10 s (approximately 650 m along-track distance). This approach reduces the influence of the natural background wind variability and provides a statistically robust measure of turbulence and hence wind farm wakes.”

Conclusion: In my opinion, the authors' conclusions about topography are a bit too strong given the very limited data presented. Given the various factors at play, can the authors really say conclusively that terrain "can lead to an amplification of the wind farm wake"? They appear to have data based on one or two flight passes. These are very strong conclusions based on one or two small data sets. I suggest changing "can lead" to "may lead" in order to suggest that it is possible, although not conclusively proven in the paper. Also, "can amplify wakes and reduce wind speed for wind farms further downwind under certain conditions" should be changed to "may amplify" as I don't think it is wise to make such important conclusions without more data. The authors seem to acknowledge this in the last sentence of the conclusion.

We changed as suggested.

While the paper is well-written, there are numerous typos and grammatical errors throughout. The paper would really benefit from a thorough proofread.

We thank the reviewer for this comment. The entire manuscript has been proofread carefully to correct any typographical or grammatical errors. We have also revised the text to improve clarity and consistency.

RC2: 'Comment on wes-2025-113', Anonymous Referee #2, 26 Sep 2025

The article “Investigation of onshore wind farm wake recovery with in-situ aircraft measurements during AWAKEN” presents results from the analysis of aircraft measurements over the wake flow of two onshore wind farms situated in the Southern Great Plains region of Oklahoma. The in situ measurements are also compared with the ground-based lidar measurements, and some interesting conclusions are drawn based on the interaction between wind farm wakes, diurnal variability, and atmospheric stability. Overall, the article possesses high scientific quality and relevance. The problem statement is clearly defined, and the results are explained and discussed in detail. However, there are some minor issues in the article that need to be addressed:

We would like to thank the reviewer for the comments and the judgement of the scientific quality and relevance.

1. The article would benefit from a clearer explanation of how lidar data from sites A1 and H were processed to obtain the wind speed and TKE profiles shown in Figures 5 and 7.. What methods are used to calculate TKE from the lidar data? Figure 7 shows the lidar data only above 110 m, and if the hub height is only 89 m, and the blade length is about 64 m, then there is a lot of missing wind data inside the rotor region. Moreover, the lidar data is not very conclusive about the velocity deficit profiles in Fig 7. If you look at the LLJ nose in the vertical profiles, you may see a shift of about +50 m in the upwind and downwind profiles. What could be the reason behind this upward shift? Can this be explained by the topography?

Thank you for your suggestion. As this paper merely uses the LIDAR measurements to introduce the aircraft measurements, we added a sentence citing two other papers which give further information on the processing of the LIDAR data.

“The processing of the lidar data is described in Krishnamurthy et al. (2025) and Newsom and Krishnamurthy (2022).”

As also requested by RC1 the section describing Figure 7 was rewritten (see p.11 l.216ff)

2. There is an absence of discussion regarding the uncertainty and random errors in the results. For example, in Fig. 8, what led to the selection of taking the median average of wind speed and TKE over 2.1 km and 5.25 km segments? The authors argue that TKE may be a better indicator of wakes downstream of an onshore wind farm as compared to the wind speed. Therefore, in Figures 9 and 10 description of random errors or standard deviation could be useful.

We thank the reviewer for this valuable comment. There has been an error in the previously stated segment length: the black dotted line used 1000 values and therefore covers a distance of 0.78km and the red dashed line used 7500 values and covered a distance of 6.07km. We changed the caption accordingly. The choice of this averaging length reflects a compromise between smoothing out small-scale variability and retaining wake-related structures. The median average was applied to minimize the influence of outliers and this reduces the impact of random errors. As also requested by RC1 a detailed section about the accuracy of the aircraft’s wind measurements (Appendix A) and the use of the aircraft measurements to derive TKE (p.20 l.325ff) was added.

3. Can the authors say something about the effect of wind turbines’ layout in the two wind farms on the downstream wakes? Even though flights were conducted slightly above hub

height, turbine layout (spacing and alignment) may still affect the wind speed and TKE profiles. We have observed this in offshore wind farms already.

The influence of turbine layout on the downstream wake structure was indeed observed, particularly for the King Plains wind farm, where the staggered turbine arrangement led to spatial variations in both wind speed deficit and TKE, as shown in Fig. 8a and 8e. Upon request of RC1 a sentence to further highlight this was added on p.13 l.240ff.

4.The conclusions regarding the amplification of wakes due to topography are important. However, they are based on a limited number of flights. While the authors address these limitations in the Discussion section, a detailed discussion about the applicability of these conclusions in relation to different kinds of terrain and large wind turbines with higher hub heights would give a meaningful context.

We thank the reviewer for this insightful comment. We fully acknowledge that the conclusions regarding the amplification of wakes due to the topography are based on a limited number of flights. The conclusion is based on the aircraft measurements which have in itself many degrees of freedom, such as the stratification of the ABL, seasonal variability, wind direction and more. However, due to the current data availability, we cannot make further claims regarding wakes for higher hub heights or different terrain types. This paper aims to describe a phenomenon which needs to be further investigated by more research flights in the same location as well as different terrain and which claims need to be supported by models.

Some minor comments:

1.Line 291: “approximately”

We changed as suggested.