Comments to the first revision of WES-114-2023

Thanks for implementing the suggested changes. The statistical uncertainty part needs additional work, as shown below.

The main criticality, however, remains the fact the calculated uncertainty bounds are far larger than some difference observed in the mean velocity fields. The justification that the error due to the vertical velocity may be overestimated is not compelling. Generally, one provides a maximum error value to estimate a cap to the uncertainty and show that "despite the error being overestimated the uncertainty is below the acceptable value etc…" This is not the case for this work where a lot of speculations are made to interpret differences in the mean flow way smaller than the uncertainty bands. Please remove all those parts discussing effects that do not pass the uncertainty test such as the non-symmetric induction zone. The paper is already quite long and the readers will benefit from the improved conciseness.

New comments

Figure 7: please use either always percentage errors (e.g 5%) or non-dimensional error (e.g 0.05).

L 570: "A deeper analysis into the propagated uncertainties indicated that the error in the u,v component estimation was primarily because of the volume averaging effect, beam-intersection angles and beam-pointing errors while the w = 0 m/s assumption was the most dominant source of error. ": so are the volume averaging, beam angles and pointing accuracy the main sources of error, or is it the vertical velocity assumption?

L 665: please remove that this paper shows how one "should analyse" field as it sounds too bold.

Main comments

2: Thanks for adding some statistical uncertainty quantification. This is however not enough for two reasons:

- The use of $1.96\ \sigma/\sqrt{N}$ is not justified. This simple formula relies on the assumptions of large N (central limit theorem and use of estimator instead of true variance) and independent samples [1]. While the 71 scan repetition can be considered a large enough sample size, in turbulent flows the samples are never independent due to presence of a non-0 integral time scale. Please ensure that the scan repetition time is slow enough to assume that the samples are separated in time by several integral time scales.

- In the conclusion, it is stated that the statistical uncertainty is smaller than the uncertainty of dual Doppler. Statistical uncertainty indeed seems to be ignored after section. Statistical uncertainty being smaller does not mean negligible however, as its magnitude is still a few percent of the freestream velocity. Please add the statistical uncertainty also to the experimental results. It is usually ok to consider it independent from the instrumental uncertainty to that it can be squared-sum to the propagated uncertainty already present in the paper.

Specific comments

6: The comment was more intended to point out the missing justification for the values of z/L used to define the stability classes. More specifically, why is $\frac{z}{|L|} < 0.4$ chosen as the neutral class?

11: the error on the location was not corrected in the new version, there is still $\frac{x}{D} = -0.16$.

12: please add that stable stratification also suppressed vertical displacement of air parcels thus enhancing flow blockage as possible explanation of the stronger induction.

13: Thanks for pointing out Bastankhah's explanation for this asymmetry. In the text, it should be made clearer that this is a dynamic aerodynamics or stall effect, but that the angles of attack are perfectly symmetric in a quasi-static sense.

15: every time the "uncertainty bounds" are cited in the text, the confidence level should be mentioned too. E.g. $1.96\ \sigma$ would correspond to 95% confidence level if Gaussianity is assumed, and so forth.

24: it is still confusing. It is first stated that the $y < 0$ sides shows faster deceleration. This implies for positive yaw offset, more deceleration in the freestream, for the negative yaw offset more deceleration in the wake. Then is it stated that for the negative offset "**similar** effects of the induction are seen where the wake at y/D < 0 decelerated faster compared to the freestream" which is in contradiction with what happens for the positive offset where the freestream part has stronger deceleration.

References

[1] V. Bayley and J. M. Hammersley. The "Effective" Number of Independent Observations in an Autocorrelated Time Series. *Supplement to the Journal of the Royal Statistical Society*, Vol. 8, No. 2 (1946), pp. 184-197.