Within this author's response, referee comments are printed in black and our answers in blue. Concrete changes for the revised paper are printed bold.

Author's response to Anonymous Referee #3 (Report 1)

We want to thank anonymous referee #3 for his extensive comments.

Ingenhorst et al. present a study about a multicopter equipped with a sonic anemometer to measure wind speed and wind direction at flexible points in space, and in particular in complex terrain. They introduce the system as a possible replacement for site assessment with CFD. Unfortunately I can not see the innovation in this study, since multicopters equipped with sonic anemometers have been reported and multiple times before and the analysis and validation that is done in this study does not go beyond what has been done before.

As we have pointed out, multicopters have been evaluated with various sensors throughout the time concerning wind speed and/or direction. Within our publication, we do show, that even turbulence intensity measurements as well as measurements of wind inclination angles are possible with promising results. Furthermore, we are evaluating accuracies with a focus on typical metrological time scales (10 min) and achieve very good measurement accuracies compared to earlier publications. This underlines and extents the possible metrological applications for UAV based measurements.
One of those applications is the presented measurement of several distributed points of speed and direction and, furthermore, turbulence intensity and inclination above complex terrain. Ultimately, we want to merge these points, measured at different times, into a single wind field. The question arises, whether speed changes throughout the 2h measurement campaign have a relevant impact and to what extent those for example can be compensated with a low-level reference. Based on our knowledge of the relevant literature, this was not yet answered with this application in mind. We consider it to be the first step towards more sophisticated measurement strategies. Nevertheless, we agree as stated inside the paper, that there are still some open questions to be answered until UAV based measurements become a reliable validation tool or even replacement for CFD simulations.

I think the authors have also not done a good job in reviewing the state of the art, because references to very similar systems are missing (e.g. Shimura et al., 2018; Nolan et al., 2018; Reuter et al., 2020; Thielicke et al., 2020, list not complete...).

For sure, literature offers a few more similar UAV based measurement systems since they are around for several years now. We assumed, that Palomaki et al. (IMU based and ultrasonic measurement) and Vasiljevic et al. (LIDAR based measurement) in combination with the overview given by Abichandani et al. allow a representative insight. **We have now added three more comparable systems, although their measurement intention might be different to ours (which is measuring a closed field of wind speed, direction, turbulence intensity and inclination above complex terrain). Shimura et al., who also are already mentioned by Abichandani et al., will now be cited directly because of their comparable measurement purpose (lines 67ff.).**

The publications of Thielicke et al. and Reuter et al., although showing promising results, are still in preprint (initially published months after our initial submission) and therefore not taken into account.

I believe that multicopters as wind measurement systems are a very valuable tool, but I do not at all agree that they can replace CFD in any way and think that suggesting this idea is very misleading for the broad audience. Airborne measurements can be a validation tool for CFD or lidars, but this is not included in this study. I think the authors are missing a good understanding of atmospheric boundary layer flow if they believe that some short measurement flights can give enough insight for a site assessment, especially in complex terrain. Again, this is reflected in a lack of suitable references and the missing discussion of atmospheric conditions during the measurement campaign.

The measurements of wind speed, direction, turbulence intensity and inclination angle within the paper are examples for two different wind situations above complex terrain. They are the basis to discuss the potential of a ground-level reference to overcome the influence of wind changes throughout the measurement. In-depth discussion of atmospheric conditions was not conducted, because those examples were never mentioned to be representative for the overall yearly wind situation at that site. Neither would two single CFD simulations for different wind directions at that location be sufficient to perform a successful site assessment.
Within the revised paper, we further explain, that the wind situations are only exemplary measurements. In future, several of such measurements might be combined to achieve a sufficient accurate wind estimation for a site assessment (lines 332 ff.). This will be part of a future publication.

I believe that the authors have a good instrument for wind measurement, but I strongly suggest that they reconsider what the original scientific contribution is that they can make with this study. I think the development of suitable measurement strategies / flight paths for the analysis of flow structures in complex terrain could be of interest, especially in combination with CFD, but this is not evaluated well enough to be published in WES at this point.

As it is mentioned within the paper, specific measurement strategies and their application will be part of future publications. To this point, we intended to investigate the achievable accuracies of single-point measurements as well as the impact of diurnal changes and whether those can be compensated by a stationary low-level reference

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Author's response to Anonymous Referee #2 (Report 2)

We want to thank Anonymous Referee #2 for his positive feedback.

Author's response to Anonymous Referee #4 (Report 3)

We want to thank Anonymous Referee #4 for his positive feedback **and have added the suggested technical corrections below to the revised paper.**

Figure 11: Please re-plot with a smaller ordinate range (max of 10m/s)
Figure 15: The color bar legend for TI should not be in %

Typos
L40: this has been successfully performed …
L40: as Stawiarski points out, … - please put a reference not just the name
L80 near end: to decide to what extent such a system

95: which are then autonomously followed.