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Atmospheric Measurement Techniques Discussions

Interactive comment on "3D Wind Vector Measurements using a 5-hole Probe with Remotely Piloted Aircraft" *by* Radiance Calmer et al.

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General comments

We thank the reviewer for his comments. The manuscript has been restructured in order to address the reviewers' concerns. The main modifications are the calibration of the 5-hole probe, the use of full wind equations instead of simplified equations (Lenschow et al., 1989), and a more detailed analysis of the PSDs to understand the origin of differences between the RPA and mast measurements. We also show that difference in vertical wind distributions does not deeply affect the calculation of cloud droplet number for aerosol-cloud study.

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Specific comments

Comments from the reviewer appear in italic, response from the authors follows. As the manuscript has been mainly rewritten, we invite the reviewers to directly refer to the updated version of the manuscript for specified sections.

**The manuscript is confusing at times, and will require a careful rewrite to be suitable for publication.*

Response: We have reworked the manuscript to clarify the sections, in particular related to the calibration and the validation of the results with sonic anemometers.

**The spectral levels, "spikes" and slopes found in the frequency spectra off the three components of the motion compensated winds computed from the RPAS (and compared against the anemometer ground truth) shown in figure 7 are very concerning and not discuss in depth in the text.*

Response: The Power Spectral Density (PSD) functions have been updated. The analysis now uses the Welch's method to calculate PSDs. The spikes have been suspected to originate from the INS. We agree that there are still issues to address turbulence or fluxes measurements (particularly calculations of divergence and convergence), as these studies require highly accurate wind measurements. However, for the purpose of aerosol-cloud interaction study, the accuracy of the updraft measurements conducted in the present work has been shown to be sufficient (Section 6).

I went back to Reineman et al. 2013, there found slight spectral level differences for the lowest wavenumbers, but nothing as significant as what is presented here.

Response: We thank the reviewer for pointing this statement out. There is actually a good agreement in spectral level for vertical wind in Reineman et al. (2013). However, a difference of level of spectral energy for the vertical wind component is found in Reuder et al. (2016) and Båserud et al. (2016) for the SUMO RPA compared to sonic anemometer. The TKE measurements from the M^2AV in Lampert et al. (2016) is higher than TKE from sonic anemometer on a mast during the afternoon and the night, which also implies higher energy levels of the PSDs. The manuscript has been updated to assess the source of difference in PSDs between the RPA and the mast. We found higher energy in ground speeds provided by the INS for frequencies lower than 0.3 Hz, which influence the wind calculation.

I would encourage the authors to go back to the data processing and ensure the algorithm is motion compensating the relative winds computed from the 5-hole probe correctly.

Response: The calibration of the 5-hole probe has been updated, using polynomial instead of linear coefficients for α the angle of attack and β the angle of sideslip. Polynomial coefficients from Treaster et al. (1978) method have been determined for static and dynamic pressures, and then used in the calculation of V_a the airspeed. As the RPA operates in the quasi-linear regime of the 5-hole probe (pitch and roll angles < 10 deg), no significant modification of the wind results has been observed with the updated calibration. The full wind equations have been used instead of the simplified equations (Lenschow et al., 1989). A comparison between the wind results has been

conducted to show a difference less than 0.03 $\pm$ 0.05 m/s on straight-and-level legs. However, to avoid any confusion, the full wind equations have been employed in the analysis, except when specified (uncertainty analysis on w, section 3.2).

**Figure 8 further demonstrates the disagreement between the sonic and RPAS wind measurements.*

Response: We disagree; actually, Figure 8 shows agreement. Uncertainty at low wind speeds related to noise – and the peak at near zero-winds is expected. The limit of detection is $\pm$ 0.1 m/s (also quantified in Figure 5), which is largely sufficient for studying aerosol-cloud interactions. In the manuscript, a section has been added to quantify the influence of the updraft in term of cloud droplet number concentrations.

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