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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Simultaneous multicopter-based air sampling and sensing of meteorological variables" *by* Caroline Brosy et al.

Anonymous Referee #1

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Within this manuscript, the authors demonstrate the use of a multicopter to take measurements of meteorological quantities (wind, potential temperature) and methane to study and quantify greenhouse gas emissions from a nearby dairy farm. While the use of an unmanned aerial vehicle (UAV) to take atmospheric measurements is not new, the technique to measure profiles of methane is novel and could be useful in future studies. Thus, the presented measurements and method has potential value for a wide audience. The authors use data from a single night to demonstrate the detectable changes in methane profiles up to 50 m as the evening progresses. While only a small amount of data is shown, much of the analysis is scientifically sound and valid. As this paper is intended to be a demonstration of the capabilities and technique used, this reviewer believes the presented results are sufficient to meet these objectives. Still, there are several comments, which are mostly minor by nature, that need

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to be addressed before this manuscript is acceptable for publication in Atmospheric Measurement Techniques. Therefore, I recommend this manuscript is acceptable for publication after minor revisions, in which the comments listed below are addressed.

Specific Comments:

a) p. 1 line 11: Define high spatial and temporal resolution. What is the resolution exactly?

b) p. 1 line 13: Scanning lidar observations (of various kinds: Doppler, DIAL, Raman) can measure winds, temperature, moisture, and some trace gas quantities (such as ozone) with high-vertical and spatial resolution (on the order of meters to 10s of meters) at low elevations angles and over large horizontal areas close to the ground. Thus, the broad statement that 'remote sensing techniques . . . are challenged to achieve sufficient detail near the ground' should be modified.

c) p. 1 line 14: Change 'horizontal sounding' to 'horizontal transect'. A 'sounding' implies a vertical profile by definition.

d) p. 1 line 24: Clarify that the UAV measures a continuous profile, in comparison that towers only measure at discrete levels where instrumentation are installed.

e) p. 2 line 9: Again, there have been studies using scanning lidar to make profiles where the minimum height above ground is 10-20 m (see Langford et al., 2015 (ozone profiles), Banta et al., 2013 (wind profiles), Yabuki et al. (water vapor), and Hammann et al., 2015 (temperature)). Data shown in these studies contradicts the statement that ground-based remote sensing cannot measure near the surface.

f) p. 2 lines 10-14: This paragraph seems out of place here. I recommend it gets moved and integrated into the paragraph at line 33 or before that paragraph.

g) p. 2 lines 16-32: Most of the cited works here are within the past 2 years, however UAVs for atmospheric research have been more widely used for $\sim$10 years. I recommend that the authors provide more details on the pioneering (first) uses of UASs for

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atmospheric measurements.

h) p. 3 line 20: Add a statement ‘location of instruments is shown in Fig. 1’.

i) p. 3 line 24 & 27: Remove references to Fig. 1, seems odd here since the figure does not show the resolution. Seems like odd placements.

j) p. 4 line 14: Here, the authors discuss night flights. In the next paragraph, it states that the aircraft has permission to fly in the daytime. Did the authors fly at night when there was no permission? Alternatively, if they did get special permission for the night flights, this should be explained so that it is clear the flights were legally conducted.

k) p. 4 line 30: What are the specifications for the pressure sensor? This is important to identify the accuracy of the potential temperature calculation.

l) p. 5 line 12: Can this method be used to retrieve ‘w’ as well as u/v? If not, specify that the horizontal wind speed is measured.

m) p. 5 line 30 / Fig. 4: Was the RMSE of the wind speed measurement a function of speed? Specifically, were high or low wind speeds measured more accurately? In Fig. 4, it may be more useful to show errorbars for each data point separately, if the statistics are robust enough to calculate the RMSE at each speed bin. Similarly, was the RMSE for the tilt angle constantly 0.4 deg at all speeds?

n) p. 6 line 7: What qualifies as ‘windy’? Give specifics.

o) p. 6 line 15: Since the inlet for the methane measurement was only 30 cm above the propellers, how was it ensured that the measurement was not disturbed by the flow above the spinning propellers? The methane measurement was taken as a 60 s average; during that 60 s, surely the measurement was distorted as air is transported downward by the propellers.

p) p. 6 line 26: Looking at Fig. 5 (and the standard deviations given on lines 23-24), it is clear that the multicopter does not capture the full range of variability that the sonic anemometer measures. The extrema measured by the tower are much larger (1.5 m/s and 6 m/s) compared to the hexacopter. Presumably the hexacopter measurement is some kind of an average, possibly because the volume the hexacopter takes up is much larger than the sonic measurement volume and the hexacopter has inertia. Have you tried running a smoothing filter (or low-pass filter) over the sonic data to quantify these effects? These effects should be noted in the manuscript, as the wind speed deviation should not be used as a measure of turbulence (since it does not compare favorably, at least here, to the sonic anemometer measurement).

q) Fig. 6: To increase readability and for ease of comparison, I suggest averaging the lidar and EC data over the profiles. To convey the variability, error bars could be used.

r) p. 7 line 8: Could topography cause the observed wind speeds at the sodar site and multicopter site to be higher? Without a map of the topography, this is difficult to determine.

s) p. 7 line 22 and Fig. 7: When was sunset? I suggest adding a vertical line on Fig. 7 denoting sunset time. Also, keep units consistent for CH4 (either ppm or ppb). For the caption of Fig. 7, clarify what the error bars show exactly (standard deviation of what, the 1-min timeseries)?

t) Fig. 8: i) I suggest changing this to a two panel plot, one panel each for the methane and one for the potential temperature with separate lines for different profile times. With having all of the profiles on one plot, it will be much easier to see the change in the profile over time, even the small changes and increase in stabilization. ii) Can error bars be added for each measurement? It may clutter the plot too much, but it is something to consider. iii) It would be better to change potential temperature units to Kelvin, as it is usually presented. iv) Also, please explain in the text why there is a discontinuity at each height where the multicopter hovers. Is it due to the fact that the temperature is evolving over the time it is hovering, or some kind of hysteresis in the sensor?

u) p. 7 line 28: How could the gradients be both intensifying and weakening over time? Please clarify.

v) p. 7 line 31: There was an increase at 50 m as well, it simply was not as large of an increase.

w) p. 8 line 23: With regard to the statement ‘although the multicopter does stir air with its propellers’, are there any tests you can do to quantify these effects on the inlet? Measure the flow disturbance at the inlet itself to infer any vertical transport during the 60 sec hovering periods?

x) p. 9 line 8: Given these values were made using only 5-min of data under a small range of values, the robustness of these statistics is questionable. I suggest adding a qualifying statement here to emphasize these limitations. Also, the RMSE of the wind direction is highly dependent on the wind speed. At low wind speeds (<1 m/s), the RMSE of the wind direction measurement would be much larger.

y) p. 9 line 13: This statement should be modified. The vertical wind profiles were not in good agreement. The wind speed from the mulitcopter was systematically larger, and the wind direction was also biased high.

z) p. 9 line 16: Were the differences that Lothon et al (2014) systematically different (biased), or were the differences more scattered (inaccurate)?

aa) p. 9 line 23: Should ‘in the west’ actually be ‘to the southwest’?

Technical corrections:

a) p. 3 line 2: Should this be Sect. 2.2 (not 2.4)?

b) p. 3 line 9 and p. 4 line 29: ‘at’ instead of ‘with’ 10 Hz.

c) p. 4 line 22: ‘approximately’ instead of ‘approx.’

d) p. 4 line 23: Use ‘At 50 m length’ instead of ‘In 50 m height’.

e) p. 5 line 17: ‘simultaneous’ instead of ‘simultaneously’

f) p. 6 line 6: ‘a’ instead of ‘an’

g) p. 6 line 7: ‘at’ instead of ‘@’

h) p. 7 line 29: Remove ‘of these gradients’ and ‘respectively’

i) p. 7 line 31: Remove ‘remarkably’

j) p. 9 line 20: Change ‘is’ to ‘if’

k) p. 10 line 11: Add missing word, ‘hence infer dispersion and mixing processes’.

References:

Banta et al., 2013: Wind energy meteorology: Insight into wind properties in the turbine-rotor layer of the atmosphere from high-resolution Doppler lidar. Bull. Amer. Meteorol. Soc. 94, 883-902.

Hammann et al., 2015: Temperature profiling of the atmospheric boundary layer with rotational Raman lidar during the HD(CP)2 Observational Prototype Experiment. Atmos. Chem. Phys., 15, 2867-2881.

Langford et al., 2015: An overview of the 2013 Las Vegas Ozone Study (LVOS): Impact of stratospheric intrusions and long-range transport on surface air quality. Atmos. Environment. 109, 305-322.

Yabuki et al., 2016: A scanning Raman lidar for observing the spatio-temporal distribution of water vapor. J. Atmos. and Solar-Terr. Phys. 150, 21-30.

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