R1: amt-2023-143

We thank the reviewer for their thorough review of the manuscript. Author's comments are in blue.

Reviewer comments for "Observing Low Altitude Features in Ozone Concentrations in a Shoreline Environment via Unmanned Aerial Systems" Josie K. Radtke et al. Atmos. Meas. Tech. amt-2023-143 Recommendation: Reject General Comments This paper presents a summary of uncrewed aircraft system (UAS) flights performed during the CHEESEHEAD19, PEcorINO, and WiscoDISCO20 campaigns that collected vertical profiles of atmospheric ozone measurements. The need for these types of measurements in general is well motivated, but the objectives of the paper are not well stated and in general the paper lacks discussion on the unique aspects of the sensor integration, sophisticated comparisons with reference measurements, and outlooks on future applications. While reasonably appropriate in scope for the journal Atmospheric Measurement Techniques, in my opinion there are issues with this study's presentation, experimental procedure, and scientific significance that would require substantial revisions before publication. Fatal Flaw In my opinion, this study is limited in regard to its overall contribution to the scientific literature, particularly in the realm of atmospheric observations with UAS. While the authors did a commendable job discussing the need for low-level observations of ozone in coastal environments, the primary results (a collection of vertical profiles to 120 m altitude with some comparisons to tower-based sensors) of the study are mostly proof-of-concept measurements collected with commercially available UAS airframes and sensors. Typically, these types of studies in AMT are focused on the design of custom-built UAS or unique sensor package integration (e.g., Altst¨adter et al., 2015; Segales et al., 2020; Hamilton et al., 2022), but that does not seem to be the focus of the present article. Otherwise, to my knowledge, there have already been a handful of studies collecting observations of atmospheric trace gases (including ozone) with UAS in a more systematic manner than the limited selection of cases presented here (e.g., Schuyler and Guzman, 2017; Schuyler et al., 2019; Krautwurst et al., 1 2021; Bretschneider et al., 2022, and references therein). Considering these factors, in my opinion the revisions necessary to improve this paper's contributions to the existing literature are too substantial at this time such that the submission should be rejected. However, I do believe the content of this study may warrant submission to a data journal such as Earth System Science Data to complement the data repositories cited at the end of the article.

It is heartening to hear that the reviewer finds this article "reasonably appropriate in scope for the journal Atmospheric Measurement Techniques" despite their feeling that this study contains what they deem as a fatal flaw. To speak to the state of where this manuscript lies within the context of other observations using UAS:

Alstadter et al (2015) describes PM2.5 observations on UAS, not O_3.

Seagales et al. (2022) describe thermos-hygrometer sensors on UAS, not O_3.

Hamilton et al, (2022) describes the DataHawk2 UAS as an atmospheric thermodynamic observation system for measurements of T, RH, P and wind speed and direction, not O_3.

The reviewer suggesting that a paper for AMT must require engineering of a new platform instead of careful study of the appropriate measurement strategy for combining commercially-available systems requirement for publishing a technique in this journal. The aim and scope of the journal, as stated, is "The main subject areas comprise the development, intercomparison, and validation of measurement instruments and techniques of data processing and information retrieval for gases, aerosols, and clouds. Papers submitted to AMT must contain atmospheric measurements, laboratory measurements relevant for atmospheric science, and/or theoretical calculations of measurements simulations with detailed error analysis including instrument simulations." The papers must contain atmospheric measurements, which we present in this manuscript. Ultimately, we hope that this experiment aligns with the scope by describing a method for investigating vertical ozone profiles in the atmosphere using UAS backed up by evaluation of precision and accuracy of those observations.

We argue that this manuscript presents an analysis of intercomparison of UAS platform measurements with ground- or tower- based measurements which indicate pitfalls of putting a POM on any commercially-available UAS (as shown from the results of the tower comparisons during CHEESEHEAD) and the improved flight and calibration parameters which lead to improved accuracy and precision (During WiscoDISCO2020).

The articles in which the reviewer reflects on the other measurements:

Schuyler, T. J., S. C. C. Bailey, and M. I. Guzman (2019) measured CO_2 CH_4 and NH_3.

Schuyler, T. J., and M. I. Guzman (2017) propose techniques for measuring CO_2, CH_4 and NH_3 on UAS. This paper is not a comprehensive look at measurements of O_3 on UAS.

Krautwurst, S., and Coauthors, 2021: Quantification of CH4 coal mining emissions in Upper Silesia by passive airborne remote sensing observations with the Methane Airborne MAPper (MAMAP) instrument during the CO2 and Methane (CoMet) campaign. Atmospheric Chemistry and Physics, 21 (23), 17 345–17 371, https://doi.org/ 10.5194/acp-21-17345-2021.

Looked at CO_2 and CH_4 by remote sensing.

Bretschneider, L., and Coauthors, 2022: MesSBAR—Multicopter and Instrumentation for Air Quality Research. Atmosphere, 13 (4), 629, https://doi.org/10.3390/atmos13040629.

This manuscript does go over the measurement of O_3 with a POM and with AlphaSense electrochemical sensors. However, this manuscript does not address issues of the POM measurement accuracy in comparison to ground observations.

Major Comments

1. While the motivation for the study is reasonably established, it is not immediately clear in the introduction what the objectives of this study are outside of generally assessing some UAS vertical profile measurements of ozone. At the end of the introduction, please explicitly outline the relevant scientific questions, hypotheses, and/or novel concepts this paper will present.

The goal of WiscoDISCO2020 was to investigate the vertical profiles of ozone at a shoreline location impacted by high ozone episodes. The lake breeze phenomenon at that specific Location in Chiwaukee Priarie WI hosts a regulatory site at a shoreline state natural area, which is one of the few in Wisconsin which regularly exceed federal ozone standards. The large sources of emissions for ozone precursors are mainly concentrated in the Chicago metro area and the presence of Lake Michigan provides an inverted atmosphere at times in which to trap said pollutants. The role of the inversion over Lake Michigan, the advection of pollutants over Lake Michigan and then back on land during the meso-scale meteorological phenomenon of the Lake Breeze is the focus of the WiscoDISCo field campaigns. This manuscript firstly outlines how the instrumentation was tested in a non-lake shore environment (during CHEESEHEAD19) and improvements to the experiment improved instrumentation performance for the first WiscoDISCO field campaign in 2020.

See lines 87-95 in tracked changes document.

2. Although not the primary focus of the study, I am rather concerned with the quality of meteorological observations collected by the iMet sensors due to their siting onboard the UAS airframes. In particular, there have been numerous investigations on the placement of temperature sensors to mitigate the influences of solar radiation, heat from the UAS motors, and heat from the body of the UAS itself while still maintaining adequate ventilation (see the discussions in Greene et al., 2018, 2019; Barbieri et al., 2019; Islam et al., 2019; Kimball et al., 2020). While the iMet-XQ2 mounting onboard the Typhoon H (Figure S1) seems reasonably well sited, the position on the MJ600 (Figure S2) likely resulted in biases due to lack of ventilation and exposure to direct sunlight and heat from the black aircraft body. While this is not something that can be corrected for necessarily, please at least include a discussion on this in the results.

   Our results from comparisons with other monitors show that the performance on the Typhoon UAS was reasonable but there was improved performance on the DJI M600 (see Figure 3b). A discussion of the relevant studies on iMET-XQ2 and the observed performance during this study has been added to the paper.

   See lines 213-222 in tracked changes document.

3. In Sections 3.1 and 3.2, UAS measurements are compared with tower-based and groundbased references. However, this is mostly presented as single cases summarized in tables 1 and 2 as well as figure 3. For a calibration procedure, I think a more thorough analysis is warranted, especially to contextualize the cases presented in section 3.3. For example, how many individual data points were collected on each day and at each level?
   A table with flight numbers, time windows and number of data points per instrument or platform has been added to the SI to outline the data presented in Section 3.3. The finalized dataset in Zenodo is discoverable which contains the data averaged to the 5-minute hovering periods.

   See P 3 and p 6 of SI.

   Additionally, bulk statistics across all days such as the mean, median, and standard deviation of the differences between UAS and tower measurements of each variable would be pertinent. What conditions are present where the largest biases are observed?
   If this is with regards to tower comparisons in section 3.1, an additional table has been added to address mean and standard deviations from the UAS -tower observations as a companion to Figure S3 in the SI in the revised manuscript.
   See P3 in SI.

4. The entire paper is building up towards the results from the cases in Section 3.3, with a lot of emphasis on the vertical distribution of ozone and temperature versus height across multiple days. These results are provided in Figure 4, with the discussion focusing on features of these vertical profiles. The current presentation and layout of this figure, however, make it difficult to follow the discussion in Section 3.3. For example, with emphasis on changes in the vertical, I recommend changing the layout of the subpanels to be organized horizontally instead of being stacked vertically so that the subpanels are taller than they are wide (or at least with an aspect ratio of 1:1). Additionally, it is difficult to tell the difference between the AM and PM profiles for 2 the June 16, 17, and 18 cases; please consider using different shapes (circles, squares, crosses, etc.) for the different profiles on the same day. I also strongly urge the use of a colorblind friendly color palette that is also uniformly perceptive in place of the current rainbow color bar(see Stauffer et al., 2015).

The suggestion of the reviewer here is a good one, and an improved figure will be added to a revised manuscript which addresses this comment.

See Figure 4 on p 17 of Tracked Changes document.

Finally, why was the choice made to use the HRRR PBL height as a reference in this figure? You mention there was a Doppler lidar present for the WiscoDISCO20 campaign, was this capable of producing PBL height estimates more locally? Otherwise, consider omitting the earlier discussions on the instruments not used for this current study.

HRRR PBL height is a metric which addresses how photochemical models are treating vertical profiles when computing photochemical ozone production. The use of the HRRR PBL height highlights the sub-grid scale of the vertical profiling. Also, the Doppler lidar instrument has a dead zone at low altitudes (<100 m AGL) in which no observations are made. The PBL heights at this location specifically lie within that dead zone during lake breeze times, so the vertical profile measurements and HRRR PBL height outputs help to highlight the scale of these lake breeze phenomena (not observable by Lidar to low altitudes). As per the response to Reviewer 2, much of the lidar discussion has been removed from the SI. Some comments about the utility of the HRRR PBL height have been added to a revised manuscript.

See lines 332-335 in tracked changes document.

Minor and Technical Comments

1. L26: Please remove the period at the start of the line.
   Done.
2. L40: Please define the acronym UAS.
   Done. Now Line 43 of tracked changes document
3. L42: Please remove the extra period between the citations and the start of the next sentence.
   Done. Line 44 of tracked changes document
4. L57: Spelling error: should “crate” read as “create”?
   Done. Line 61 of tracked changes document
5. L86: Please define the acronym “UW” in UW-Eau Claire
   Done. Line 101 of tracked changes document
6. L93: Spelling error: remove the “F” at the start of the word “and”.

Done. Line 108 of tracked changes document

7. Section 2.2: Here I have a handful of suggestions for breaking this long first paragraph up into logical sections. First, at L138, the stentence starting "The main goal of this campaign..." could start a new paragraph. Similarly, break a new paragraph as L147 starting with "During WiscoDISCO20 UAS...".
   Done (Line 155) and Done (line 164 of tracked changes document).
8. 148: Please define the acronym "DNR."

Done. Line 164 of tracked changes document

9. L148–154: Did you use these instruments specifically in this study? Consider omitting this portion (see major comment 4).
   As per the Reviewer 2 comments, with removing discussion of the Doppler Lidar, we will omit comment on the Pandara and Doppler Lidar instrumentation.
10. L165: This would be a good place to highlight the total number of flights conducted in each period.
    A table for flights and flight times has been added to the SI. See Pages 3 and 6.
11. L177: Add a space between "electrochemical sensors" and the following parenthetical citation.
    Done. Line 197 of tracked changes document
12. L194: Should this read "an intercomparison..." instead of just "n"?
    Yes. So changed. Line 225 of tracked changes document
13. L202–204: The sentence beginning "The UAS gradient observations..." is a bit hard to follow, please consider rewording.
    In combination of reviewer 1 and 2 comments, the statements here are being revised in the final document to the following:
    "Technically the overall comparison between tower gradients and UAS gradients show agreement; however the considerable uncertainties make both indistinguishable from zero (See Table 1). This evaluation demonstrated a likely source of inaccuracy with POM ozone observations, with significant offset from the absolute tower observations."
    Lines 233-237 of tracked changes document.
14. L208: I recommend breaking a new paragraph starting with the sentence "Improvements to the UAS sensor package..." 3
    Done. Line 242 of tracked changes document.
15. Table 1 and surrounding discussion: This is perhaps semantic, but these are not necessarily gradients but rather just differences. Please consider changing the wording throughout, or computing the gradients by dividing the differences by the height between the sensors.

    Changing all language over to 'differences' may make the entire paper more difficult to distinguish what an observed vertical distribution was per platform and a comparison between two instruments.

16. Table 2: Please include the total number of flights and/or individual samples that go into each mean and standard deviation presented here (see also major point 3). 4
    Ns have been added to the table.

Reviewer 1 References

Altst¨adter, B., and Coauthors, 2015: ALADINA – an unmanned research aircraft for observing vertical and horizontal distributions of ultrafine particles within the atmospheric boundary layer. Atmospheric Measurement Techniques, 8 (4), 1627–1639, https://doi.org/ 10.5194/amt-8-1627-2015.

Barbieri, L., and Coauthors, 2019: Intercomparison of Small Unmanned Aircraft System (sUAS) Measurements for Atmospheric Science during the LAPSE-RATE Campaign. Sensors, 19 (9), 2179, https://doi.org/10.3390/s19092179.

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Greene, B. R., A. R. Segales, T. M. Bell, E. A. Pillar-Little, and P. B. Chilson, 2019: Environmental and Sensor Integration Influences on Temperature Measurements by Rotary-Wing Unmanned Aircraft Systems. Sensors, 19 (6), 1470, https://doi.org/10.3390/s19061470.

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Hamilton, J., G. de Boer, A. Doddi, and D. A. Lawrence, 2022: The DataHawk2 uncrewed aircraft system for atmospheric research. Atmospheric Measurement Techniques, 15 (22), 6789–6806, https://doi.org/10.5194/amt-15-6789-2022.

Islam, A., A. L. Houston, A. Shankar, and C. Detweiler, 2019: Design and Evaluation of Sensor Housing for Boundary Layer Profiling Using Multirotors. Sensors, 19 (11), 2481, https://doi.org/10.3390/s19112481.

Kimball, S. K., C. J. Montalvo, and M. S. Mulekar, 2020: Assessing iMET-XQ Performance and Optimal Placement on a Small Off-the-Shelf, Rotary-Wing UAV, as a Function of Atmospheric Conditions. Atmosphere, 11 (6), 660, https://doi.org/10.3390/atmos11060660.

Krautwurst, S., and Coauthors, 2021: Quantification of CH4 coal mining emissions in Upper Silesia by passive airborne remote sensing observations with the Methane Airborne MAPper (MAMAP) instrument during the CO2 and Methane (CoMet) campaign. Atmospheric Chemistry and Physics, 21 (23), 17 345–17 371, https://doi.org/ 10.5194/acp-21-17345-2021.

Schuyler, T. J., S. C. C. Bailey, and M. I. Guzman, 2019: Monitoring Tropospheric Gases with Small Unmanned Aerial Systems (sUAS) during the Second CLOUDMAP Flight Campaign. Atmosphere, 10 (8), 434, https://doi.org/10.3390/atmos10080434. Schuyler, T. J., and M. I. Guzman, 2017: Unmanned Aerial Systems for Monitoring Trace Tropospheric Gases. Atmosphere, 8 (10), 206, https://doi.org/10.3390/atmos8100206. 5 Segales, A. R., B. R. Greene, T. M. Bell, W. Doyle, J. J. Martin, E. A. Pillar-Little, and P. B. Chilson, 2020: The CopterSonde: An insight into the development of a smart unmanned aircraft system for atmospheric boundary layer research. Atmospheric Measurement Techniques, 13 (5), 2833–2848, https://doi.org/10.5194/amt-13-2833-2020. Stauffer, R., G. J. Mayr, M. Dabernig, and A. Zeileis, 2015: Somewhere Over the Rainbow: How to Make Effective Use of Colors in Meteorological Visualizations. Bulletin of the American Meteorological Society, 96 (2), 203–216, https://doi.org/10.1175/ BAMS-D-13-00155.1.

AMT-2023-143 Author's Response to Reviewer 2:

We thank the reviewer for their thorough evaluation of the submitted manuscript and value the suggestions provided to strengthen the paper. Author's comments are in blue.

We can absolutely address the reviewer's concerns regarding the structure of the manuscript to emphasize the techniques described in the manuscript to better highlight the utility of this approach and the improvements to using UAS for atmospheric measurements as was a result of this work and outlined in the document.

Specifically:

In a revised manuscript, we can expand upon the utility and benefits of using a POM and also the difficulties of mounting this instrument on a UAS platform.

See p194-196 of tracked changes document. See also lines 285-289 of tracked changes document.

We can highlight the literature on iMET on UAS and how this informed the work done here.

See lines 213-223 of tracked changes document.

We can also address the choice of UAS for these two campaigns. The first UAS, the Typhoon H, was chosen as an inexpensive commercial UAS with capability of holding the payload of the POM. The second UAS, the DJI M120, had an increased payload capacity with its camera removed and the ability to put a top-mount for the sensor package, thus both increasing the stability of the payload and also an increased flight time.

See lines 129-130 and 173-175 of the tracked changes document.

This paper describes an ozone and meteorological measurement system mounted on two different hexacopter UAS, flown over land and near water, and compared with fixed sensors. UAS measurements in the atmosphere are rapidly becoming more common (as demonstrated in the references cited here), and though they have limitations from instrument weight and power consumption, they could potentially make important measurements of meteorology, atmospheric trace gases and aerosols. This work can be considered as a step toward progress in this area, especially in terms of the high spatial resolution measurements useful in studies of the boundary layer and lake breeze/land breeze events. However, the manuscript in its current form is not as informative as it could be, and could use a bit of rewriting. To make a more meaningful contribution to the literature, I recommend the following changes:

The paper should be restructured to emphasize the measurement technology aspects, both since it is in review at Atmospheric Measurement Techniques, but also because this is really much of the new and useful information contained in the manuscript. (Although I also agree that section 3.3 has some interesting science in it.) First, I would move the paragraph about the Personal Ozone Monitor (POM), now 2.3, to the start of the Materials and Methods and make it 2.1. The iMet sensor could go right after that. The authors can decide whether to have a section of its own for the UAS used in the three studies, but right now there is just the bare minimum of description of the two UAS. Can anything be added to describe why these were chosen, what the necessary characteristics of a UAS for this research are, how they worked as an airframe/sensor package, and how they could be improved? Also, the introduction could be changed to emphasize more the potential for UAS measurements in the boundary layer or near-shore environment to add to our understanding of chemical composition and atmospheric structure there. This might only require a few sentences added or rewritten, but it would help the paper become more coherent and targeted. Finally, in the Results and Discussion section, how do these results compare with the previous experiments of Li et al. 2020 for ozone?

This can be addressed in a revised manuscript by adding the following language:

The work by Li et al. (2020) described use of POM and particle observation on a fixed-wing UAS flying at a speed of 150 km/hr and compared measurements from those instruments to regulatory instruments on a tethered airship and addressed intercomparison with the POM and a regulatory ozone measurement instrument ($O_3$42M from ESA). They used an insulated box for the POM and were able to show high correlation with a regulatory monitor, but with an offset. Their conclusions are that the POM measures atmospheric variability consistent with a regulatory monitor but demonstrates a negative bias. Here, we flew the POM at a much lower flight speed, and only averaged data from a single hovered point at which we stayed for 5 minutes each flight. This was to address the duty-cycle limitations of the POM with the on-off in series subtraction of the water vapor absorption. Li et al address only that the regulatory monitor they used for comparison did a heating method for removing water vapor interference, instead of a dual-cell active subtraction in parallel as is typical for other regulatory monitors. While Li et al 2020 demonstrated some correlation between RH and variability between the UAS-mounted POM and tethered-airship-platform regulatory monitors, they do show that vertical gradients can be captured by UAS and tethered airship, but with discrepancies in location of PBL. This is consistent with our observations that the gradient observations from UAS are consistent (with high variability) with tower-based observations in the lowest 120 m AGL. What we cannot account for here is the difference in POM variability on a UAS which hovers for 5 minutes in comparison to a fixed-wing travelling at 150 km hr^{-1}, which may also lead to additional variability in the measurement due to inlet pressure changes and optical cell vibrations.

See lines 351-367 in tracked changes document.

The POM (because of its very low weight and power consumption) is a very attractive sensor for UAS use, but did it work? Can it work? If not, what sensor (from 2B or elsewhere) would be needed, and how much extra weight does that require? What would need to be changed to optimize the UAS for this kind of experiment? Again, this should not take a lot of space, but would improve the impact of the manuscript.

We think that Figure 3a from this figure shows a reasonable agreement for POM measurements made aloft and a ground-based observation. Certainly, that agreement is improved from the tower-based comparison. Therefore, the POM on a slow-moving UAS with a high flight-time and inside an insulated box (as described in Li, et al. 2020 and Wang, et al. 2017) is likely the best solution to placing an O_3 sensor on a UAS. The electrochemical sensors for measuring ozone have not been shown to be as robust.

Specific comments:

P.1, l. 30 “organic decomposition”? Some biogenic VOCs are emitted through decomposition processes, but other natural sources like isoprene, terpenes, and some alcohols are emitted directly from plants.

We have edited the manuscript to just say “biogenic processes” as organic decomposition is a sub-section of processes by which there are VOC biogenic sources.

P.2, l. 42-44 There is nothing in the Beekman et al., 1997 reference about tethered balloons over water (it does discuss tropopause folding events). Is there supposed to be a different reference for the first part of this sentence? But really, the two parts of this sentence don’t go together (ground to 1500 m vs. upper troposphere).

That reference was incorrect. The references have been updated and the sentence edited. I use Endnote software for reference management – and have checked the document for inconsistencies. This does lead to some weird tracked changes in the document as I re-format the revised manuscript.

l. 48 I think this reference should be to Li et al., 2020 (comparison with the airship), not 2021 (primarily VOCs, and I saw no mention of an airship in the manuscript). Is Li et al., 2020 the most closely related paper to this manuscript (or perhaps that is Guimaras et al., 2020, or Gronoff et al., or several of them)? It does use a fixed-wing UAS rather than a hexacopter though. But it seems to have a thorough evaluation section of the instruments and measurements. It seems like the discussion section of this manuscript might need to include a bit more related to this paper. Are your results comparable or similar to Figure 5a (or 7a, or 8) in Li et al., 2020? In addition, please take a look at papers citing Li et al., 2020. A few relevant ones are cited here (such as Q. Chen et al., 2020), but I think there are a couple of others that might be cited as well. How about L. Chen et al., 2022? I did not do a thorough search; the authors should do that.

You are correct, the reference should be Li et al 2020. We have looked into more references that the reviewer suggests. Wu 2020, Chen 2019, Chen 2022 are all articles which can be described in a revised manuscript.

See lines 370-372 in tracked changes document

l.49 What is the correct reference here?

This should be Li et al 2018 ("Three-dimensional analysis of ozone and PM2.5 distributions obtained by observations of tethered balloon and unmanned aerial vehicle in Shanghai, China" *Stochastic Environmental Research and Risk Assessment*) instead of Li 2020. I did not notice anything in either Li et al. paper about Generalized Additive Models, but I did not read either of them thoroughly.

See Line 54 in tracked changes document

P.3, l.82 That is great that there is "improved performance and viability" but is that shown or demonstrated in the following sections? How can you do that without referring back to the results in the cited literature?

This sentence refers to the improvements to performance between the Park Falls, WI experiment and the Lake Michigan shoreline experiments outlined in this manuscript. This sentence has been edited for clarity in the revised manuscript.

Lines 87-95 in the tracked changes document introduce the idea.

Lines 240-245 in the tracked changes document refer to the specific improvements between the two campaigns.

P.5, l. 125 A 15 minute flight time is not ideal. Is there any way to get a similar platform with longer flight duration? (Again, this can be addressed in the discussion section.)

Yes, we are able to accomplish longer flights with different UAS (namely in experiments conducted in 2021 and 2022 with DJI M300 UAS). As this is referring to the experiments that occurred in 2020, we have added comments to the discussion with regards to improvements which could be made.

Lines 367-370 of tracked changes document

P.6, l. 180 Why does the filter need batteries or power? Perhaps I don't understand what the filter is, or what it is used for.

The filter does not need batteries. The sentence has been modified for clarity.

P.7, l. 184 Are these the actual accuracy and precision (considering the comparisons with other instruments) or just calculated from the formula from 2B? line 202 would suggest that the accuracy is not as good in flight. And compare with l. 245-246 and l. 252. Seems like the text needs to be made consistent on this.

Line 184 is referring to the calculation from 2B Tech and the rest of the analysis in the paper is to test the accuracy of the instrument in flight against a) tower observations or b) ground observations made at a similar inlet height to a hovering altitude for the UAS. A revised manuscript has addressed the clarity in line 201 of the tracked changes document.

P.8, Table 1 The gradients measured by the POM were generally not distinguishable from zero. So the statement on l. 201 is technically true, but not very helpful. Glad to see that the results led to the subsequent improvements described later in that paragraph.

We agree that the comparison with tower observations are not great. The goal of sharing this table is to address the discrepancy with the absolute ozone measurements, the high noise of the observations and the understanding that if the gradients were closer to correct, accuracy could be improved by correcting for a zero-offset.

P.11, Figure 3a How does this figure compare with a similar one in Li et al. 2020? (See earlier comments above.) Again, this can be addressed in the discussion section or wherever it makes the most sense.

This figure has some similarities for the Li et al 2020 figure 5a, where they saw a linear fit of 0.7x – 7 for a POM correlation to a regulatory ozone measurement instrument standard. The difference between our measurement and theirs is that we see more observations along the 1:1 line with higher ozone concentrations deviating the most from the center line, whereas the Li et al 2020 paper showed a consistent linear response at ~70% of the regulatory O_3 measurement. Language about this comparison has been added to the revised manuscript in lines 285-290 of tracked changes document.

P.14, l. 313 This sentence is a little confusing, with both tethered balloons and UAS. I think it can be changed slightly to make it clearer.

So modified. See p 373-375.

P.15, Figure 4 I find it hard to distinguish the two profiles on June 18. By adding a top axis for ozone, you would have 4 traces on panels b, c, and d, so that might be confusing too. Perhaps just making the traces line+symbols (by adding reasonably thick gray and black lines for the two profiles, respectively, to the color-coded circles) it would be easy enough to follow. Right now, I had to examine this figure very closely while reading the text on P. 13-14 in order to understand it.

As per the reviewer 1 comments, the panel can be made differently to make the AM and PM flights more distinguishable. A new figure will replace this one in the revised manuscript. Lines will be added to the figure in the revised manuscript. See Figure 4 on p17 of tracked changes document.

P.16 After editing the rest of the paper, perhaps the conclusions section could be strengthened and made more useful to readers.

With the increased focus on the measurement techniques discussed in this paper, the conclusions have been edited to align with the manuscript revisions. See lines 391-392 for more specificity in the improvements.

P.17-24 There is an extensive reference section, but a few of the references I checked do not seem to correspond to what is in the main text of the manuscript. Is it possible to check at least the most important references against the text? Maybe all of them?

Will do. The references will be corrected in the revised manuscript.

Figure S3 I can't tell the difference in the symbols between the two tower instruments. But that's probably OK (if they agree with each other); the colors clearly mark the different elevations. In the legend, can you put the two 122 m symbols next to each other? The figure clearly shows the data from both the tower and the UAS.

Yes, this figure can be made more distinguishable. See SI p2.

I don't think you really need all the Figures S4-S10. Just one or two for reference would be fine.

We'll keep one figure as a demonstration of a lake breeze.

Perhaps the same comment for Figures S12-16, though these are at least related to the data shown in Figure 4.

OK, they have been removed from the final manuscript.

I definitely think that some of Figures S17-21 could be dropped.

OK, they have been removed from the final manuscript.

In Figure S22, are the dashed lines a running average? Perhaps that should go into the caption.

Yes. We have added a description to the legend. See SI p 7. This is now figure S6.

Technical and proofreading comments:

P.2, l. 57 "create"?

So edited.

P.3, l. 93 "and"? "on land"?

"And" was edited. Not sure where "on land" is in reference to.

P.5, l. 115 Are the times correct for 2020 flights? Just wondering, because 6 pm is later than 11 am. Maybe just reorder the two times.

So edited.

l. 130-132 This sentence is a little odd-sounding. I assume the UAS measurements were just a small part of the overall campaign. (It's fine up to "shoreline", but then rest of the sentence implies that the UAS was the purpose of the project.)

The campaign was just the UAS measurements with some additional ground observations (namely the wind-pro lidar).

P.6, l. 168 Please add a comma after "spectroscopy".

So edited.

P.13, l. 308 What do you mean by “fumigation”? (This may be OK, I’m not sure.)

We mean vertical missing from pollutant emissions at the surface. We replaced fumigation with “transport” to simplify the statement. Line 349 of tracked changes document.