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.

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

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.

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.

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.

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 202, Chen 2019, Chem 2022 are all articles which can be described in a revised manuscript.

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.

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.

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.

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 184.

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 O3 measurement. Language about this comparison has been added to the revised manuscript.

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 to: "Some researchers have successfully used UAS for vertical ozone profiles up to 1000 m using tethered balloons (Li et al., 2020) and a UAS-mounted thermally-insulated POM in the winter (Chen et al. 2020) "

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.

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.

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.

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

We'll keep Figure S8 as it is a nice 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.

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.