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.

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.
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 peroids.
   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?
   So if this is with regards to tower comparisons in section 3.1, an additional table has been added to address mean, median and standard deviations from the UAS -tower observations as a companion to Figure S3 in the SI in the revised manuscript.
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.

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.

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.
3. L42: Please remove the extra period between the citations and the start of the next sentence.
   Done
4. L57: Spelling error: should "crate" read as "create"?
   Done.
5. L86: Please define the acronym "UW" in UW-Eau Claire
   Done.
6. L93: Spelling error: remove the "F" at the start of the word "and".
   Done.
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 and Done.
8. 148: Please define the acronym "DNR."

Done.

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.
11. L177: Add a space between “electrochemical sensors” and the following parenthetical citation.
    Done.
12. L194: Should this read “an intercomparison...” instead of just “n”?
    Yes. So changed.
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.”
14. L208: I recommend breaking a new paragraph starting with the sentence “Improvements to the UAS sensor package...” 3
    Done.
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.

    Ok. 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. This can be addressed in a revised manuscript.

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

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