Reviewer comments for “Observing Low Altitude Features in Ozone Concentrations in a Shoreline Environment via Unmanned Aerial Systems”

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Atmos. Meas. Tech.

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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., Altstädter 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., 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.

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.

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.

3. In Sections 3.1 and 3.2, UAS measurements are compared with tower-based and ground-based 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? 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?

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

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). 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.

Minor and Technical Comments

1. L26: Please remove the period at the start of the line.

2. L40: Please define the acronym UAS.

3. L42: Please remove the extra period between the citations and the start of the next sentence.

4. L57: Spelling error: should “crate” read as “create”?

5. L86: Please define the acronym “UW” in UW-Eau Claire

6. L93: Spelling error: remove the “F” at the start of the word “and”.

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...”.

8. 148: Please define the acronym “DNR.”

9. L148–154: Did you use these instruments specifically in this study? Consider omitting this portion (see major comment 4).

10. L165: This would be a good place to highlight the total number of flights conducted in each period.

11. L177: Add a space between “electrochemical sensors” and the following parenthetical citation.

12. L194: Should this read “an intercomparison...” instead of just “n”?

13. L202–204: The sentence beginning “The UAS gradient observations...” is a bit hard to follow, please consider rewording.

14. L208: I recommend breaking a new paragraph starting with the sentence “Improvements to the UAS sensor package...”

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.

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).

References

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