Review of "Drone CO_2 Measurements During the Tajogaite Volcanic Eruption" by Ericksen et al. (2024)

The manuscript by Ericksen et al. (2024) applies Unpiloted Aerial System (UAS) platforms to measure carbon dioxide (CO_2) concentrations and carbon isotope ratios during the 2021 eruption of the Tajogaite Volcano in Spain. This study used a Dragonfly UAS outfit with systems for measuring CO_2 concentrations and carbon isotopic ratios for 10 transects through volcanic plumes during the eruption. Using measured CO_2 concentrations and winds, applying gaussian assumptions, led to emission rate estimates of 1.19×10^6 to 2.80×10^7 t day^{-1} (1190 to 28000 kt day^{-1}). These are very large emission rates compared to recent literature estimates and 1-2 orders of magnitude larger compared to those derived using ground-based measurements in this study (1.4×10^4 to 3.6×10^5 t day^{-1}). The study conducts no attempt to derive uncertainty/errors of these estimates or compare their results with past literature. The lack of description of the methods used in the study made it challenging to understand where errors could be coming from. The paper was very short so there is plenty of space to provide significantly more detail about the methods and results applied in this study. My suggestions for this are below. The paper has issues with grammar and typos and overall reads more as a report and less like a manuscript. The one results of the study I agree with is the fact that UAS systems are vital for measurements of trace gases in volcanic plumes, but this has been shown before (e.g., Xi et al., 2016). As is, this manuscript is not sufficient for publication in *Atmospheric Measurement Techniques*.

Major Comments

1. Line 36-37. I think the authors need to discuss more of the challenges with using satellite XCO2 to monitor volcanic plumes and estimate CO_2 fluxes. Satellites provide improved spatiotemporal observational coverage compared to ground-based and aircraft measurements for volcanic plumes. However, satellite XCO2 retrievals are associated with error due to aerosol and water vapor interference, measuring cumulative fluxes instead of direct volcanic emissions, and overall retrieval uncertainties. The work by Johnson et al. (2020) describes these issues in detail.

2. Line 92. These CO_2/SO_2 ratios are at the high end of reported values in the literature. The authors should compare their measurements to other studies in the literature and discuss this comparison in the paper.

3. Carbon isotope data. The authors need to describe what this data is and what it is used for to better understand volcanic sources of CO_2. Readers in atmospheric science, such as this reviewer, and other fields outside of volcanology will be interested in this manuscript; therefore, the authors need to better describe some of the data/terms used in this study.

4. Methods Section. This section needs to come before the results and discussion of the study. It is impossible to follow the results of the work if the reader has no idea about the tools, methods, uncertainties, etc. associated with the results.

5. Line 121. The study derives a maximum CO_2 flux of 4730 kt day^{-1}. This value seems pretty large. Can the authors compare this value to other studies from other volcanoes? Some context for all the results in this study is lacking. Upon reviewing this manuscript further, it appears the emission estimate results presented in the abstract (1.19×10^6 to 2.80×10^7 t day^{-1} (1190 to 28000 kt day^{-1})) don't match what is presented in the body of the text (1.65×10^4 to 4.73×10^6 t day^{-1}). I don't understand the discrepancy between the presented flux estimates. The manuscript discusses the results in such little detail it is challenging to follow.

6. Line 131-134. There are other studies that have applied UAS platforms to measure SO_2 and CO_2 in volcanic plumes (e.g., Xi et al., 2016) that are not discussed in this manuscript. It would be useful to expand upon the importance of UAS platforms for monitoring volcanic plumes and compare the results of this study to past work. Overall, there appears to be a lack of literature review and comparison of the author's findings with past work discussed throughout this manuscript.

7. Line 150-151. What does this sentence mean? How does the UAS drift with the plume? Is this done physically or using near-real-time concentration measurements to remain within the regions of maximum CO_2 concentrations? Much more information and details are needed about the UAS capabilities and how it was used in this study.

8. Line 166. What is the ambient/background CO_2 concentration derived in this study? Also, how was it derived? This value is critical for estimating ΔCO2 concentrations and the corresponding emission rates. Also, what are the uncertainty levels associated with these ambient/background CO_2 concentration estimates?

9. Line 167. Is the gaussian assumption for the volcanic plume shape appropriate here for these flux estimates during all 10 transects? The authors should plot the CO_2 concentrations throughout the transect of the volcanic plumes monitored here. This could easily show whether or not the plumes measured in this case were in fact close to gaussian in shape. If not, the gaussian assumption might not be appropriate for these flux estimates.

After reading the rest of the paper I found Figure A1. Why was this figure not referenced in the text of the manuscript? This figure is probably more important than figures such as Fig. 4. Maybe move this figure to the main body of the text or make sure to reference it clearly.

That being said, transect 2 and maybe 6 measure volcanic plumes that are close to gaussian in shape. However, some of the other transects clearly show non-gaussian characteristics. How does this impact the estimated emission rates presented in this study?

10. Line 175. What are the uncertainty levels associated with the measured wind speeds and direction? Errors associated with measured winds can be large. Also, how variable were winds throughout the plume? How was this variability treated in the flux calculations? This was not discussed at all and is needed in order to reproduce and assess the results of this manuscript.

11. Uncertainty. This study lacks any discussion, or attempt to determine, uncertainty levels associated with the flux results derived in this study. The emission rates are quite high in some of the transects and these results need to be assessed for error/uncertainty and more thoroughly compared to past studies. Errors/uncertainty from wind speed measurements (wind speeds aren't displayed or discussed in the study), gaussian assumptions, plume extrapolation methods, CO_2 concentration measurements (sensor uncertainty and the lack of observational coverage of the sensor in each transect), and other potential error sources can be large. These values need to be quantified in order to understand these results.

12. Derived emission rates. This study estimated very large CO_2 emission rate estimates (those provided in the abstract) of 1.19×10^6 to 2.80×10^7 t day^{-1} (1190 to 28000 kt day^{-1}). These estimates are 1-2 orders of magnitude larger compared to those derived from ground-based estimates in this study (1.4×10^4 to 3.6×10^5 t day^{-1}). The authors state that these differences could be due to emission variability, underestimate of the SO_2 flux, or the lack of validity of the 2D gaussian assumptions. The authors need to look into these discrepancies much closer in order to trust the results of the CO_2 emissions stated in this study. Also, these derived values are much larger than other estimates in the literature from other volcanoes. The authors need to carefully investigate the literature to see if they can find CO_2 emission rates at the magnitude of those derived in this study.

Minor Comments

1. Line 9-10. A forecasting signal for what? An impending eruption? This sentence seems incomplete.

2. Line 11. Remove "gas" from the beginning of this sentence.

3. Line 11. Instead of "steam" I think you mean "water vapor".

4. Line 17. This is the first time "UAS" has been used in the body of the text, therefore you should define the abbreviation here.

5. Line 67-68. Are the authors referring to the TROPOMI sensor data onboard the Sentinel 5 Precursor satellite?

6. Line 66-79. It seems like it would be easier for the readers to follow the emission rate estimate discussions if the authors used consistent units. Can the authors just use kt day^{-1} for both SO_2 and CO_2 instead of using $\times 10^6$ or $\times 10^4$ t day^{-1}?

7. Line 79-80. What does this hover-drift test wind speed value mean? Is this an average wind speed measured during the 10 flights? This needs some further explanation.

8. Line 79. Do you mean Fig. 1? Same thing when referencing Table 1 for the first time in Line 81.

9. Line 82. Just use the actual CO_2 values in ppm and not the scientific notation of the values.

10. Line 86. Figure 5 should be Figure 2. The authors should reference tables and figures in sequential order.

11. Line 93. I think some values are missing in this sentence.

12. Line 144. “To” instead of “TO”.

13. Line 150. Is “(2)” trying to make a reference to Figure 2?

References

Johnson, M. S., Schwandner, F. M., Potter, C. S., Nguyen, H. M., Bell, E., Nelson, R. R., et al. (2020). Carbon dioxide emissions during the 2018 Kilauea volcano eruption estimated using OCO-2 satellite retrievals. Geophysical Research Letters, 47, e2020GL090507. https://doi.org/10.1029/2020GL090507.

Xi, X., Johnson, M. S., Jeong, S., Fladeland, M., Pieri, D., Diaz, J. A., et al. (2016). Constraining the sulfur dioxide degassing flux from Turrialba volcano, Costa Rica using unmanned aerial system measurements. Journal of Volcanology and Geothermal Research, 325, 110–118. https://doi.org/10.1016/j.jvolgeores.2016.06.