Review of "A multi-purpose, multi-rotor drone system for long range and high-altitude volcanic gas plume measurements"

Submitted to Atmospheric Measurement Techniques (AMT)

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Review by Peter Kelly, USGS, Jan 20, 2021

Summary

This article describes a small, electric-powered multi-rotor drone and several payloads that were used for volcanic gas sensing and sampling at volcanoes in Papua New Guinea between 2016-2019. The authors focus on technical descriptions of the payloads (DOAS, multi-GAS, a denuder system, and gas-bag collection system) and modifications that were made to the drone platform to improve its endurance. This contribution appears to serve as a technical companion paper to (Liu et al., 2020), who discuss the volcanologic significance of the obtained gas composition and emission rate results from the 2019 campaign.

Some of the payloads used in the experiments have been described previously (e.g. the DOAS system, denuder system, and 'Sunkist' instrument; Rüdiger et al., 2018), but the manuscript does include descriptions of a new multi-GAS unit developed by Chalmers U. that includes the innovative integration of a mini anemometer to obtain windspeeds, as well as a plume sampling unit for collecting bagged samples for posterior carbon-isotope analysis. To me, the most novel aspect of the manuscript is the presentation and analysis of the two methods for determining plume speed; most of the other instruments and techniques have been in use for some time. Accurately determining plume speeds is critical for determining volcanic gas emission rates, and the instrument and methods comparison shown here are helpful for addressing this important issue.

The technical emphasis of the manuscript is appropriate for Atmospheric Measurement Techniques and the operational 'lessons learned' will be valuable and of interest to the volcanic gas community. The manuscript is generally well-written and structured but there are some items that need to be addressed prior to publication. Broadly, my main concerns (documented below) are that the manuscript is too vague in places, and that supporting data are incomplete, contain mistakes, or are not available in an open repository. The scientific value of the collected gas measurements are hardly discussed (perhaps a little more effort could be made here, or would it overlap too much with Liu et al.?), therefore I feel that the technical contribution must be significant and substantive to warrant publication. These issues compromise the study's impact and value in its present form but should not be too difficult to remedy. The article will be appropriate for publication in AMT after these issues and the comments below are resolved. I hope that these comments are helpful.

Data availability

At present the manuscript does not adhere to the data standards for AMT. Line 901: 'The datasets generated for this study can be provided upon request sent to the "Corresponding Author".' The data from this study needs be made open and accessible, in accord with current community and journal standards.

AMT/EGU Data policy:
https://www.atmospheric-measurement-techniques.net/policies/data_policy.html

Unfortunately, the data that are available appear to be incomplete and contain mistakes. For example, I tried to further examine the multiGAS data presented in the study but encountered significant difficulties in attempting to do so, as described below.

MultiGAS data from the experiment are said to be available from Liu et al., 2020 (l. 580-581):

https://advances.sciencemag.org/content/suppl/2020/10/26/6.44.eabb9103.DC1

Data that I was able to find in the supplement to Liu et al. (2020) includes data from three flights on May 22 and May 23, 2019 but not from other dates (for example, data from 2016 are plotted in Figure 4 and data from May 26 are plotted in Figure 11 and are not in the Liu et al. supplement). The data available in the supplement apparently include data from two multigas instruments: one from U. Palermo and the Chalmers instrument described herein (although the supplement does not readily indicate which data came from which instrument, or I missed the explanation somewhere – my apologies if I simply missed it!). The supplementary data also do not include absolute timestamps (only sequential integer seconds) so it's not possible to precisely connect these data to the results listed in the Liu et al. study by date/time, and while they do include lat/long, the altitudes are missing which makes plotting and understanding the flight paths hard.

Since Liu et al. (2020) emphasize the data from the U. Palermo instrument, my best guess is that the first two tabs include data from that instrument and my hunch is that the third dataset (Raw data 23-05-19 B) came from the Chalmers instrument, but of this I am not positive. My hunch that the presented data come from different instruments is supported by the observation that the data formats are different (e.g. the first two data tabs have lat/long listed as decimal degrees and the third has lat/long as UTM), but I really don't know for certain.

If my hunch is correct, then what I take to be Chalmers multiGAS data from May 23 appear to be very poor, and only show two SO_2 peaks near the end of the file (one of which is partially truncated; shown below) and CO_2 is poorly correlated with SO_2. The poor correlation between the CO_2 and SO_2 would appear to violate assumption 2 of the analysis routine (l. 383). Elsewhere the CO_2 data show large apparent jumps of ~15 ppm; is this some kind of interference? Finally I note that no units are given for any of the measurements, and the air temperature (Tair) appears to be listed in A/D counts(?) rather than sensible units (the 'Tair' values range from 5369 to 6805). I could not find data from the Sunkist unit anywhere.

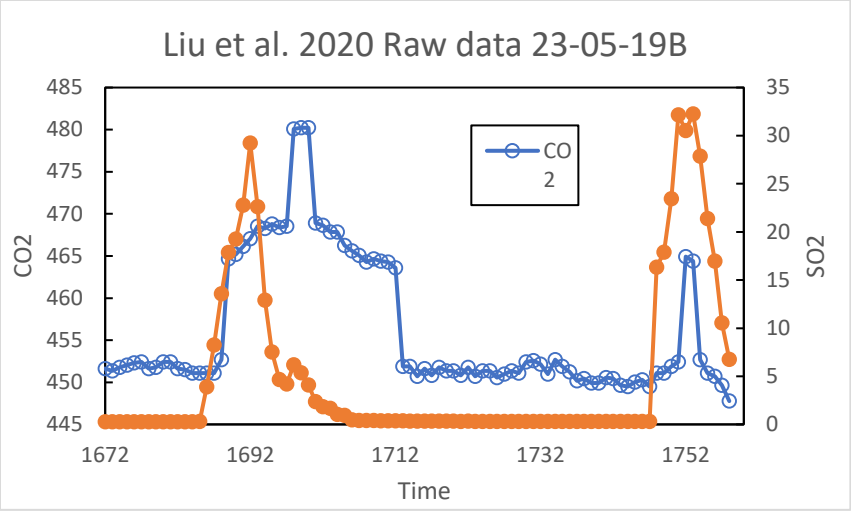

Figure showing CO_2 and SO_2 data from Liu et al., 2020 supplement 'abb9103_Table_S2', perhaps showing data from the multiGAS unit described in the present work(?). Note that the second SO_2 peak is truncated and that CO_2 and SO_2 are poorly correlated.

It's possible that these are the wrong data but based on the available information it is very difficult to be sure. This example makes it clear that the data from this experiment should be:

- better organized
- documented
- quality-assured
- checked for completeness
- made available in an open repository

At present, it is not possible to easily or unambiguously find or identify data from the described systems, which makes independent evaluation of their performances impossible. When preparing these data for release, please make sure that all necessary parameters (e.g. altitude) are included.

Specific Comments

1. The range achieved in the present work (~ 5 km) is good for a multirotor system but is not especially noteworthy and has been achieved previously with commercial multi-rotor drone systems, as summarized in James et al., 2020, Table 2 (copied below) and the references therein (quote from James et al.): "UAS equipped with miniaturised gas sensing instrumentation (see Section 2.2.3; Figures 2A, 15) are now bridging the gap between direct measurements and remote sensing observations, enabling repeatable, proximal measurements from ranges >5 km [Pieri et al., 2013a; Shinohara, 2013; Diaz et al., 2015; Mori et al., 2016; Xi et al., 2016; Di Stefano et al., 2018; Rudiger et al., 2018; Stix et al., 2018b; Kazahaya et al., 2019; Liu et al., 2019; Schellenberg et al., 2019; Syahbana et al., 2019]."

Table 2: Example vehicle characteristics.

System	Payload weight (total weight)	Flight endurance (min.)	Approx. range (km)	Reference
Single rotor				
Thunder Tiger Raptor 90	3 kg (8.2 kg)	12	16	McGonigle et al. [2008]
RMAX-G1	~10 kg (94 kg)	~90	~5	Kaneko et al. [2011]; Koyama et al. [2013]; Ohminato et al. [2017]; Kazahaya et al. [2019]
Multi-rotor				
LAB645	4 kg (12 kg)	~40	~4	Terada et al. [2018]
Vulcan UAV X8	800 g (~10.5 kg)	12–18	2–4	Liu et al. [2019]; Pering et al. [2019]
αUAV	2.5 kg	~35	~4	Mori et al. [2016]
Phantom 4 Pro	(1.4 kg)	<25		Manufacturer specifications www.dji.com/phantom-4-pro
Fixed-wing				
VectorWing 100	1 kg (3.6 kg)	30–45	10–15	Pieri et al. [2013a]
Skywalker Titan	1.0 kg (8.5 kg)	30–45	5–10	
Skywalker X8	0.2 kg (4.2 kg)	30–45	5–10	Schellenberg et al. [2019]

Some of the UAS listed in James et al. are gas-powered, but, for example, Syahbana et al., 2019 used a small electric-powered fixed-wing drone that launched from 11 km distance and gained > 3000 m altitude to monitor Agung Volcano during unrest and eruption. Endurance to launch from >10 km is significant because the vast majority of deaths from PDCs at explosive arc volcanoes occur within ≤10 km distance from the vent (Brown et al., 2017). The introduction should reflect the fact that small, electric, multi-rotor drones cannot match the range possible with equivalent fixed-wing units, but they do have significant advantages in terms of maneuverability, ease of take-offs and landings, ease of integrating payloads, etc., as discussed in the manuscript.

The notion of what constitutes 'long range' and 'high-altitude' is subjective, but given that the drone's performance more or less matches the capabilities of other similar systems it may be appropriate to consider changing the title to the present work to "A multi-purpose, multi-rotor drone system for volcanic gas plume measurements".

2. Figure 1 of the manuscript is copied verbatim from Liu et al. (2020) supplement Figure S3b (shown below) and without correct attribution. Although the team for the two papers are much the same, 'recycling' figures in this way is poor practice and - at minimum - any previously published images and/or figures need to be cited properly.

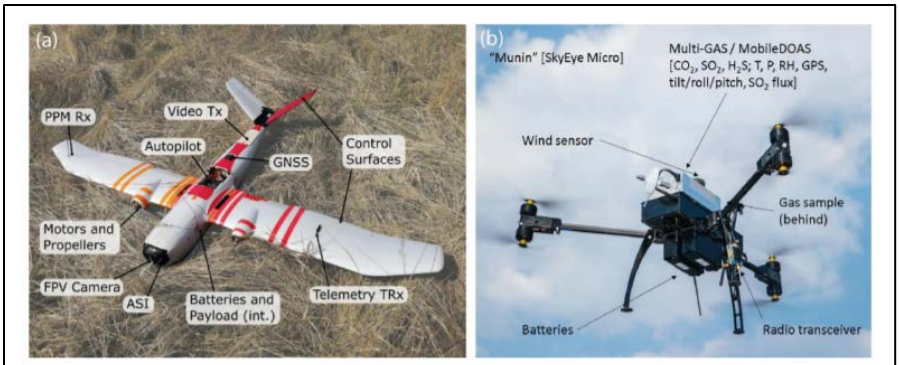

Figure S3: Annotated diagrams of the Unoccupied Aerial Systems (UAS). (a) Fixed-wing 'Titan' aircraft. Image credit: K. Wood; (b) Multi-rotor 'Munin' aircraft in Y6 configuration. Image Credit: M. Wordell. See Material and Methods for more details.

Figure S3 from Liu et al. (2020) supplement. Liu et al's Figure S3b is copied and presented as Figure 1 in the present work without correct attribution.

l.149-156: These seem like very important operational observations. However, critical details are left out or are too vague. "The drone's angle…also proved to be of great important for energy consumption." Can this be made more specific and actionable? What is an optimal flight vector? Roughly how much endurance might the flight optimization gain? Why did power consumption go up so much when encountering clouds?

l. 166-168: "It was found that the time needed for switching between different payloads could be considerably reduced by changes in the drone frame and payload designs (balance, power connection, data access, telemetry)." The present description is too vague. What 'changes' were made? How were instruments mounted? How was balance checked?

l. 169-171: "Access to the drone flight logs were found to be useful…" I agree that they would be very useful for the reasons stated. Please make the logs available as part of the data release.

l.284-296: On my first reading it was unclear if the CO_2, SO_2, and H_2S sensors were self-made or commercially-available units. Only upon reading Appendix A it became clear that the sensors were not self-made. Please amend the section to include specifics of the sensors so that readers don't need to consult the Appendix to find this. Also, the descriptions of the NDIR and electrochemical sensors can be shortened since their measurement principles are well known and described in the manufacturers' documentation.

Figure 3: the box diagram shows the anemometer located inside the instrument enclosure.

3. Measurement of in-plume H_2O mixing ratio

   l.299-301 If I understand correctly, the RH sensor included in the multigas unit measures RH inside the instrument box, not in the sample gas stream. If true, the instrument therefore does not achieve its claimed capability of measuring in-plume H_2O (l. 26). The authors seem to implicitly acknowledge this detail in the conclusions (l. 669) where they list CO_2, SO_2, and H_2S as measured gases instead of H_2O, CO_2, SO_2, H_2S as claimed earlier and in the abstract. It's

confusing to me why the authors go on to discuss how to calculate in-plume H_2O mixing ratios when their system doesn't actually have that capability: l. 299-301 "Our system, however, measures these variables only inside the instrument box, so the mixing ratio is representative of ambient gas passively diffusing in the interior of the unit.." I'm very sorry if I've misunderstood something but I've read through lines 297-303 several times now, and the description and Figure 3 indicate that T, P, and RH measure conditions inside the sampling box, not in the sample stream or in ambient air outside the instrument enclosure. While it's perhaps acceptable to use the P record as 'ambient' or 'near-ambient' P, the T and RH will be useful as diagnostics, but not as plume or ambient measurements. As presented, this is very confusing. I recommend clarifying which in-plume measurements the system supports, and which measurements are for diagnostics. Since the RH appears to be intended for diagnostics, then there is really no need to discuss conversion to in-plume H_2O mixing ratio and that text can be deleted.

Furthermore, the desired resolution of the water measurement is purported to be 1 ppmv (l. 110) - an ambitious goal, to be sure - but the precision of the RH sensor is stated as 3% RH. The point is moot since it appears that the instrument was not designed to measure plume H_2O, but for the sake of argument let's say the total P = 1000 hPa, the P_{H2O} is 20 hPa, and saturated vapor pressure is 25 hPa. In this case the RH would be approximately 80%. Here, ±3% RH precision would translate to about ±0.75 hPa or about 750 ppmv (it's unclear if the given precision is 1σ or a range, etc., please clarify here and throughout). This example suggests that in practical terms it would be impossible to achieve 1 ppmv H_2O resolution with the specified sensors. In addition to random and systematic errors on the RH measurement, I would expect some error in the relationship used to convert RH to mixing ratio, random and systematic errors in the needed P+T measurements, etc.

Please carefully edit the manuscript so that the measurements made are characterized accurately, and that realistic analytical values are given as design goals, and that the methods used to characterize the accuracy and precision of the various sensors are described and/or listed in the measurement specifications. For example, the denuder section (2.6) gives a clear statement on how LOD and LOQ were calculated (l. 501-503).

l.326-327: Please include the time constants of the sensors here. Are the values listed in the appendix t_{90}? Please clarify.

L.327-329: This is an interesting idea, but I do have concerns about its viability. Would a ~1L tedlar bag provide enough gas to get a good 'plateau value' from the sensors? How long do the sensors take to plateau during calibrations? Also, at low concentrations I would expect some sorption of S-containing species that could impact the results.

l.337: what is "the time of variability in gas concentration"? The meaning is not clear in the explanation.

l.360-361: 20 seconds to exchange the volume of the CO_2 optical cell seems like an awfully long time. Can the pump rate be increased to shorten this time? It would be very useful to see what a step function looks like during calibration of the CO_2, SO_2, and H_2S sensors.

4. Multigas data processing technique

l.365-366: "…only one energy storing and one energy dissipating component…" I think what's being referred to here is a resistive-capacitive circuit (RC), which are classically described as first order systems. Perhaps consider recasting this section using more standard terminology.

Equation 1: this looks like an interesting approach; amplifying the signal to better approximate the input versus instead of lowpassing a 'fast' sensor to match a slower sensor. I couldn't get a copy of the reference within the timeframe of this review. Are there other more easily-accessed references that explain this theoretical approach? Is τ the first order time constant here? Most of the electrochemical sensors I use have τ values between about 2 and 6, so would a_1 normally be 2 to 6 and $a_0 = 1$?

l.379: I appreciate that the equations for the frequency-dependent amplitude and phase lag are given, but they could use a little more explanation for readers and a reference. At minimum, the angular velocity (ω) should be defined and it should be made clear that these relationships describe the phase shift in radians (I suppose that $\tan^{-1}(\omega\tau)*180/pi$ could be given if degrees are preferred).

l.382: Please provide evidence that shows the responses of your sensors (e.g. many studies show calibration peaks).

l.383: I am concerned that the assumption that the input signals are highly correlated is dangerous; this may work for measurements with high signal to noise and for homogenous plumes, but heterogenous plumes exist and sometimes ambient background variations can be significant (e.g. Kelly et al., 2013). This is a significant weakness of the outlined approach.

l.384-386: It's not clear to me what these assumptions mean. Is there another way to clarify?

l.397-405: A worked example is needed here, perhaps as part of the data release. It's not enough to say that the evaluation happens in matlab. If the intent is for others to try to use this technique, then an example dataset would be most useful. When iterating the time response factors, is only one parameter varied (a_1) and a_0 set to 1?

Figure 4: I'm surprised the CO_2 shows so much variation with the 20 second time necessary to exchange the gas in the optical cell. Also the response time in the Appendix is listed as 20 seconds for the SO_2 and 30 seconds for the CO_2, yet the CO_2 appears to be much 'faster' than the SO_2 sensor. How is it possible that the CO_2 sensor has such a long exchange time, slower response, and yet shows much sharper measurements than the SO_2? The CO_2 also shows much smaller corrections than the SO_2 sensor. Can this be explained and clarified or corrected? What values of a_0 and a_1 delivered the optimal fit?

The corrected signal in 4b still shows considerable scatter (as do the data in Figure 11), which suggests that perhaps the model isn't working so well. It would be useful to compare the results of this method to the approach described by Roberts et al. (2014).

l.416-420: I disagree that characterizing sensor responses is overly time-consuming. Characterizing such responses offers many advantages for tracking sensor health and provides a basis for simple, reproducible, and automated data processing routines that require no assumptions of plume homogeneity like the presented method. What evidence exists that lab-

derived time responses differ from field performances? This section feels like an overreach and the claims should be substantiated or revised.

l.462: What was the value of the calibration gas? How large were the applied corrections?

l. 915: is "DN_A" equivalent to analog-to-digital 'counts'? If so, I suggest using 'counts' since it's a more common term.

5. Appendix A:

    a. 'Measured Quantities': multigas: correct delta ^{13}C notation
    b. Correct display of Size (L x W x H)
    c. I'm not sure I understand: how is the accuracy of the DOAS (1 ppm*m) smaller than its precision (5 ppm*m)?
    d. What is the model of CO_2 sensor that was used? I tried to check the listed parameters against the manufacturer specifications but couldn't do so with confidence because the model number of the multigas CO_2 sensor isn't given. What I did find suggests that the listed precision and accuracy are not supported by the available documentation. This is another place where open data would help substantiate the authors' claims. Based on what I can find, the *digital resolution* for the lowest-range smartGAS CO_2 sensor (F3-212205-05000) is listed as 1 ppm (the spec listed in the authors' table), but the manufacturer-specified '3σ detection limit' is listed as either ≤ 8 ppm or ≤ 20 ppm, depending on how it is configured ('standard' or 'fast'; I'm not familiar with these sensors to know what this actually means). These specs suggest that the random noise is more like ~3 to 7 ppmv at 1σ. Is this correct? Was the CO_2 sensor modified somehow to improve its precision? How were the values given in the table derived? Furthermore, Figure 3 indicates that the CO_2 signal is recorded using the Arduino's 10 bit ADC, so the best resolution possible will be ~1 ppm based solely on the ADC bit depth and sensor range (listed as 0-1000 ppm), assuming the full analog range is utilized.

        Please revisit these and other specifications in the table and clarify their meaning (1σ, 2 σ, etc) and how they were determined or where they come from.

        https://www.smartgas.eu/fileadmin/11_aktuelle_datenbl%C3%A4tter_flow/DS_F3-212205-05000_CO2_2000_ppm.pdf
    e. The radio link is listed in the Appendix as 400 MHz whereas a 900 MHz radio is specified in line 186. Which is it? Are these bands legal in PNG? What models of radios were used?
    f. Please specify the volume of the tedlar bags (presently indicated as "4 Tedlar bags (X L))"

6. Appendix B: Why are H_2O mixing ratios calculated on a dry basis (Eq B2) and other gases calculated on a wet basis (Eq B1)? The P correction on the sulfur sensors will be very small, but in formal terms it would be better to be consistent. Rarely are such details given in volcano-gas papers, so even with the discrepancy I am happy to see these equations laid out.

Editorial Suggestions and typos

1. Correct all instances of 'in-situ' to 'in situ' (no hyphen, no italics), in line with EGU and other common editorial style guides. See EGU's section on English guidelines and house standards: https://publications.copernicus.org/for_authors/manuscript_preparation.html

l.925 (**E**quation B4)

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