Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-146-RC2, 2017

Atmospheric Measurement Techniques Discussions
Open Access

Interactive comment on "Characterization of the Particle Emission from Ships Operating at Sea Using Unmanned Aerial Vehicles" *by* Tommaso F. Villa et al.

Anonymous Referee #3

Received and published: 4 December 2017

Overall assessment

The research objectives are not clear, is this a proof of concept of the use of UAV systems for emission monitoring and enforcement (MARPOL), or is this climate-related research with the aim to influence maritime policy in that respect, or is this the development of tools to asses emission factors in the field in the optimization of fuel efficiency and emissions in the development of commercial marine engines and emission abating technology (such as scrubbers)?

The authors do not mention some highly relevant projects, studies and operations that have been executed, or are ongoing in Europe whether or not with UAV systems on the subject of airborne and remote ship emission monitoring. Although the study has some interesting and innovative aspects, the use of UAV systems for emission monitoring is not new and should not be presented as such.

As a proof of concept, a valid comparison should have been made with another (remote) measurement technique, the comparison of the results with emission factors from literature is insufficient as validation.

A significantly more detailed description of the performed calibration methods should have been provided, if the sensors were not adequately calibrated, a data analysis based on the absolute values was not scientifically justified.

A profound and overall accuracy assessment should have been included.

Based on the limited added scientific value, the incomplete presentation of the current related airborne monitoring activities, the insufficient scientific validation and calibration techniques, the lack of an error analysis and some premature assumptions, I recommend to reject the manuscript for publication.

Detailed assessment

Line 23: The authors indicate that emissions were assessed during real world conditions. This is not assessed as such as all measurements were performed from and for one ship. Besides the measured RV is a relatively small vessel (94m) while average merchant vessels are in the order of 200-400m. The RV was also running on ultra low sulphur marine diesel fuel while in reality only a fraction of the international merchant vessels use this fuel type. Different factors may influence the successful assessment of ship emissions among others are: ship-type, ship-age, ship-size, ship-shape, ship-activity, fuel-type, funnel height, funnel shape, wind conditions, inversion layers, etc. For a realistic assessment during real world conditions these factors should have been elaborated. Furthermore for this study the flight path was based on the ship position, in real life ship position is not known in detail, AIS only provides basic navigation info, e.g. there is no information on the location and shape of the funnel on the ship. The limited autonomy, range and payload of the UAV make this UAV not suitable for realistic operational measurements at sea during real world conditions, the study can therefore hardly be used as a proof of concept. For actual (cost-)effective operations offshore, much more robust fixed- or rotary-wing UAV systems should be used, these systems have other specifications (speed, manoeuvrability etc.) than the one used in this study.

Line 24: The authors indicate that for the first time ship emissions can be assessed and regulated on a reliable and inexpensive way. This is incorrect, as emissions from ships are already assessed and regulated from both airborne, land based and ship-borne sensors in Belgium, The Netherlands, Denmark, Germany and Finland since 2015 at a large scale and on a reliable and cost efficient manner. The use of the UAV's is not necessarily more cost-effective, especially if operated from a ship, and often more time-consuming with less operational output capacity per flight hour. Clearly more information is required to establish cost-effectiveness (platform cost, number of ship measurements per hour, personnel involved, robustness of platform in offshore conditions, ...) . Furthermore the use of UAV's for emission monitoring operations is not new, in 2016 EMSA ordered a feasibility study, granted to CLS, concerning the use of RPAS for emission monitoring (STEAM project), in addition the Danish company EXPLICIT performed some successful emission measurements with small drones. The only aspect which might be innovative in this study is the measurement of PM emissions from ships using drones, but as this is not yet regulated by international law, this has (currently) only academic use.

Line 64: The authors make the assumption that manned aircraft are not feasible for airborne measurements of ship emissions, although the EU funded CompMon project clearly showed the feasibility of manned aircraft for operational emission regulatory airborne surveillance (e.g. operations in Belgium with >2500 monitored ships in 3 years and operations in Denmark with >1000 monitored ships in 2 years).

Line 69: see comment Line 24

Line 142: Sensitivity range for CO_2 is 50ppm, this is important as this is same order of magnitude as the delta CO_2 for measurements at 100m, this aspect should be discussed further in the article in an overall assessment of the margin of error, which is currently missing.

Line146: Significantly more detailed information should be provided on the calibration method (references samples, calibration-factors, offset, . . .). It is also not clear if a calibration was performed before (and after) every measuring day, this should have been done to ensure the validity of the data.

Line 147 (Figure S1): More information is required for the comparison of the CPC with the DISC, it is not clear what kind of air samples were used for the comparison, it looks like this is just done based on continuous ambient air measurements on board of the RV, for a proper validation a comparison should be made with real emissions. A comparison of the IAQ with the PICARO is completely missing here. If only a comparison (validation) is possible in a lab, this comparison should at least be done during similar conditions as during the field measurement (exposure time, concentration, temperature, . . .), this is clearly not the case as the particle concentrations is very low in this comparison. It looks like the intercept of the linear regression is not put at zero, why is this, was a zero calibration performed? Especially for CO_2 it is important to perform the calibration in the same range as the measurement range as the IR absorption is non-linear, no comments were made on this aspect in the article. Furthermore it should be noted that a linear regression is not an ideal method to compare 2 sensors, the Bland Allman method is more appropriate (Statistical Methods for Assessing Agreement Between Two Methods of Clinical Measurement," by JM Bland and DG Altman, The Lancet, February 8, 1986, 307-310).

Line 158: Flight speed is here expressed as 1.5m/s, it is not clear if this is the airspeed or ground speed. If this is the airspeed, the actual ground speed will depend on the wind conditions, therefore the flight speed through the plume is dependent on the wind conditions too. During the first day, the wind was cross on the ship heading. The plume would be expected at 180° if transect were flown with alternating heading 250° and 70° (perpendicular to the ship heading), the transect with heading 250° would have been flown with a significant different ground speed (ca. 6.5 m/s instead of 1.5 m/s), no mention is made of this in the article.

Line 208: I would suggest adding an indication of the resulting plume location and flight pattern on the graphs. These graphs would also visualise the different airspeed between the transects (see comment line 158).

Line220: Only 9 times the plume was sampled, very few statistical conclusions can be made based on this small sample size, especially the linear regression on line 277 is questionable.

Line 229: The distance (25m) is missing in this sentence.

Line 232: It is mentioned that the CO2 is up to 100 ppm higher in the plume, this is not clear on the graph (only 50-75 ppm), this will be the part for integration to amount to the delta CO2. Furthermore it should be noted that the peaks for CO2 at a distance of 100 m is of the same order of magnitude of the sensor accuracy.

Line 262: Another flight transect could have been used where the UAV would be flown at the same speed and heading as the RV and hovered in the plume, this would require a transmission of measurement info to the control station to adjust flight altitude and pattern to successfully find the plume and measure the plume for longer periods.

Line280: Instead of a comparison between calculated emission factors and the emission factors from previous studies a comparison with the emission factors calculated based on a plume measurement with the other equipment on board of the RV (e.g. Picaro) would have made more sense.

Line 312: Generalization and misconception that the use of UAV systems would consist of a reduced cost. It is definitely not presented in this article that UAV systems could provide a real cost effective alternative to other surveillance methods as no cost benefit comparison was made between different surveillance methods (both fixed stations and airborne sensors; operational output capacity; personnel and supporting platform etc.) and all missions were carried out from a vessel, which has a higher operational cost per hour as an aircraft and a lower speed and therefore a much lower cost efficiency.

Note that a higher cost efficiency could maybe be acquired with this setup where these operations would be combined with other task carried out by patrol vessels, pilot ships or research vessels assuming that these vessels would operate within 2 km of shipping lanes. This was not mentioned in the article.

Line 326: SO2 is completely missing here, SO2 is the only emission regulation which is effectively monitored using airborne platforms at this moment and should therefore at least be included in the discussion.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-146, 2017.