Review of Prata et al., Measuring SO2 ship emissions with an ultra-violet imaging camera

General

This paper demonstrates a very nice and important application of UV imaging cameras, for the detection of sulfur emissions in ship plumes. This technique has been used prior for volcanic measurements of sulfur emissions from volcanoes.

The author shows illustrative graphs of detected SO2 and discusses many problems, clearly pointing out that the signal obtained may be other things than SO2, such as particles.

However, one main objection to the paper is that it gives a double messages regarding the possibility of deriving fluxes (emission rates) from the measurements and the uncertainties involved and I suggest to make this more clear. Forinstance In the summary and conclusion it is clearly said that quantification of emissions using the camera is doubtful, due to the fact that particles, water vapor etc can interfere. However, in the results chapter and error analysis the uncertainty is rather low 20-40%, inconsistent with the overall conclusion.

Details

Abstract:

Row 18; I suggest removing the accuracy number from the abstract. I don't think the paper shows that the uncertainty is well understood. The sentence coming afterwards is sufficient.

P 9470, row 10, SERENAS should be SIRENAS

P 9470, row 20. Some of the conclusions from the Balzani paper should be given here. From the Balzani paper, figure 6, it is shown that the UV CAM measures at least double high values than other techniques, i.e. DOAS (airborne and ground based) and LIDAR when comparing median values , with a much higher spread (order of magnitude higher). A direct comparison on a ship to ship base shows even higher values

P 9478, row 17, 100 ppmm seems high as if this is the detection limit, do I misunderstand ?.

P 9491, row 18, eq 7. It should be indicated here that the wind speed that should be used is the apparent speed, the vector between wind and ship speed; as forinstance stated in the Berg paper (Atmos. Meas. Tech., 5, 1085-1098, 2012).

P 9482, row, row 6, Was the wind measurement really as low as at 1.5 m?

P 9482, row 15, In my mind the wind speed provides the highest uncertainty in the measurement. To my experience the wind is disturbed downwind of ships, especially when measuring as close as done in this

paper. This is not reflected at all in this paper, instead the author simply assumes that multiplying a mast measurement at 1.5 m height by an empirical logarithmic curve a 1 m/s uncertainty is obtained. In my mind this uncertainty is far too low. Mast measurements at 10 m would be more reliable.

P 9483, row 5 and Table 3: I believe the uncertainty analysis is very optimistic, especially in the wind uncertainty which I would believe is at least 30% . The analysis is also not consistent when comparing to other measurements, as done in the Balzani paper, showing 100% differences and a large spread. There should also be a term for the uncertainty in wind direction which can be rather important. In addition the impact of particles or water vapor droplets in the plume is missing.

P 9483, row 13. In my mind the author gives the impression that the UV measurements in most cases agree within 10-20 %. This is not clear at all when looking at the results in the Balzani paper. From the Balzani paper, figure 6, it is shown that the UV CAM measures at least double high values than other techniques, i.e. DOAS (airborne and ground based) and LIDAR when comparing median values, with a much higher spread (order of magnitude higher). A direct comparison on a ship to ship base shows even higher discrepancies, up to an order of magnitude. To claim that the accuracy is 10-20% in most cases is therefore misleading. For instance the measurements of NYK Cool of 174 g/s in Table 4 seem unrealistically high. If correcting for the apparent wind an emission rate of 360 kg/h is obtained corresponding to 36 tons per hour of fuel consumption, assuming a sulfur fuel content of 1%, This is an order of magnitude too high, in my mind.

All in all I believe that this paper does not give a convincing picture that quantification is possible down to a level of 20-40%, since there are too many outliers. I think this is said by the author in the conclusions and summary, but then it should be clear in this section as well.