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Atmospheric Measurement Techniques Discussions

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 #1

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Line 22: There is a typo with the first emission factor given.

L24: The authors indicate that they have demonstrated a "reliable, inexpensive and accessible" way of measuring ship emissions. The measurements here required the UAV being deployed from on board the ship. This seems potentially quite limiting. It would be useful if the authors were to rethink the concept of "accessible." I do not dispute that the method here has potential. But, it has not been demonstrated that it is an "accessible" method, especially given the need to optimize the flight path before performing measurements.

L83: I suggest that both "sophisticated" and "world class" be removed. There is no

need for these superlatives, nor are they justified by the description.

L120-129: While an interesting discussion, at the end it was a little unclear what was specifically done here.

Substantailly more information regarding calibration and testing of the DISCmini and the IAQ-calc are needed. The supplemental has no information on the CO2 comparison. This should be added. For the particle comparison, the authors should indicate the measurement conditions. As they note, the calibration depends on the assumed particle size distribution. What was chosen for calibration? Was this just ambient air? Particles produced from an atomizer? Are the calibration particles relevant to the particles in the plume in terms of the size distribution? Were the DISCmini concentrations corrected to account for the difference in slopes in Fig. S1? Is that what is meant by calibration, or are the instruments just being compared? How is uncertainty estimated? A lot more information is needed for both CO2 and particles.

Eqn. 1: The authors use the integrated peak concentrations to calculate the ratio between delta values and the EF. From Fig. 4 it is evident that the CO2 plume is broader than the PN plume. The authors should consider discussing this issue in the context of how it impacts their EF estimates.

L240: Here, the authors focus on differences in absolute values. Such differences can result for a variety of reasons. What really matters, though, is how different the derived EF values are. I suggest that the authors bring the EFs for these plumes into the discussion.

Table 2 and Fig. 5: The units given for CO2 are not correct. This must be kg/mˆ3. It is not possible to simply have kg as the units, since the volume is not known. Also, if the units are not kg/mˆ3, then the units on the derived EFs will not make sense.

Table 2 vs. Fig. 5: There seems to be an inconsistency. The slope from Fig. 5 can be converted to an EF by multiplying by 3.2 kg CO2/kg-fuel. This yields 6.4e15 particles/kg-fuel. But, the average from Table 2 is only 2.6e15 particles/kg-fuel. These should be closer. The difference may be because the authors have not fixed their intercept to zero. This should be looked at by the authors. Also, in Fig. 5 the x-axis should start from zero.

L283: The authors assert that the 20 m intercepts will give more reliable results than the 100 m intercepts. However, at 100 m the plumes are wider, which offsets somewhat the benefit of greater amplitude of the 20 m intercepts. The authors do not provide an uncertainty analysis currently. The statement here should be justified by demonstrating that the EFs from the 20 m intercepts truly do have lower uncertainties than the 100 m intercepts. The contrast with the background is part of the story, but not the only factor that impacts the uncertainty. For a methods paper, I expect to see more rigorous consideration of measurement uncertainty than is currently provided.

L286: While yes, the observations are "comparable" with other measurements, the authors should certainly note that their measurements are very much on the low end of the literature range.

L290: It is unclear why the authors make their most detailed comparison with Beecken et al., compared to all the other studies cited.

Table 3: The authors need to include Lack et al. (2009, JGR) in their comparison table and in discussion in the text. Lack et al. (2009) report measurements from a variety of different ship types based on plpume intercepts. Their work also clearly shows that the exact EF that one obtains for particles depends on the lower size threshold of the measurement. Here, the authors indicate that it is 10 nm. But, at the same time, the calibration is dependent on the particle size distribution. These issues should certainly be discussed in the context of discussing the measurement accuracy. Perhaps the measured EFs here are on the low side because they really are. But, it may be that some aspect of this is a result of the particular calibration method and the measurement uncertainty. Uncertainties must be discussed more fully, in general.

L299: it is unclear what "in-land transportation" means. Only in looking at the reference is it clear that this means buses operating on "compressed natural gas and ultralow sulfur diesel." It seems that the authors are arguing here that their low PN EF values are a result of the fuel sulfur difference from some of the literature studies. However, I do not find this argument compelling for the simple reason that bus engines are not comparable to marine engines. If the authors want to make this argument, they should compare more directly with ship measurements. For example, the Lack et al. (2011) paper compares PN EF values from before and after a ship in operation switches to low sulfur fuel. They see a negligible difference on the particle number, although the particle mass concentration decreases. This conflicts with the argument that the authors seem to be advancing here through their comparison with a bus study. The same goes for the comparison to the aircraft study. While it is perhaps interesting to compare between engine types, this does not provide any indication that the fuel is what drove this difference.

L311: The authors talk about their method being "validated" because they fall in the range previously observed for ships. To me, this is marginal. A true validation would have used a separate method to measure the EF for this particular ship. This was not done. No discussion of measurement uncertainty has been provided. Thus, we have no way of knowing whether the fact that the measurements here are on the low end of the literature range is because the ship simply had a lower EF or was a result of the measurement itself. For a methods paper, this lacks sufficient details regarding measurement calibration and testing. This is certainly an interesting proof of concept. But, I have substantial concerns regarding the use of terms such as "validation" given the lack of uncertainty analysis or full discussion of specific issues associated with PN measurement using the DISCmini. I think that this paper will only be publishable with a substantially more robust discussion of uncertainties.

Grammar note: The authors consistently say that the "Data was." It should usually be "data are."

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