We thank the reviewer for his/her comments, which have helped us to improve this manuscript. We address each comment below, with our responses given in blue text.

Reviewer #2

As a general comment, I suggest the authors include reference to the MARPOL convention. The fuel sulphur content, as determined by the authors, is ~0.1% m/m, which will be the requirement for all vessels within the ECA zones as of 2015 (http://www.imo.org/OurWork/Environment/PollutionPrevention/AirPollution/Pages/Sulphuroxides-%

28SOx%29-%E2%80%93-Regulation-14.aspx). This makes the work extremely relevant. I suggest having 'MARPOL' or 'MARPOL compliant fuel' (or similar) in the title or abstract to increase the interest in the paper.

We thank the reviewer for pointing this out and helping us to make our study of broader interest. We have modified the abstract to read: "Emissions factors (EFs) for gas and sub-micron particle-phase species were measured in intercepted plumes as a function of vessel speed from an underway research vessel, the NOAA Ship Miller Freeman, operating a medium-speed diesel engine on low-sulfur marine gas oil (fuel sulphur content ~0.1% by weight). The low sulphur fuel in use conforms to the MARPOL fuel sulphur limit within emission control areas set to take effect in 2015 and to California-specific limits set to take effect in 2014."

The authors need to check the document for consistency in terminology. BC needs to be rBC or eBC where appropriate, SO42- or SO4 and kg-fuel-1 or kg-1 fuel (a few examples are given below).

We have checked the document for consistency. We utilize the specific terminology rBC and eBC when it is necessary to distinguish between the measurement methodologies, and have made a point to further emphasize the potential differences in the "Experimental" section.

Page 24641, line 27 onwards. There is a lot of information in the description of the instruments. The authors might consider tabulating it for clarity and consistency i.e. sometimes the manufacture of the instrument is given, sometimes not. Same with model numbers.

We now provide a table with details in the supplemental material.

Page 24642 Line 2 I think this is the first use of OM, please expand.

Done.

Line 6 (CN>3nm) I believe is the first use of CN, please expand. I would also suggest the instrument is described as an Ultrafine Condensation Particle Counter (UCPC), TSI model 3025a or similar. Not all readers will be aerosol scientists. Same for 3010. 3010 also has a D50 of 10nm, unless it has been modified (Needs updating elsewhere e.g. figure 1; page 24649)

We have updated. We also inadvertently listed the wrong CPC model. The two CPCs that were used were a TSI model 3025 (ultrafine) and a Brechtel Manufacturing mixing CPC.

Line 15. I suggest a slight modification to this sentence. 'Corrections to the HR-AMS due to an instrumental collection efficiency (mostly caused by bounce from the internal heated surface, Huffman; Matthew; Middlebrook), need to be applied.' Or similar. Not all readers will know of the AMS CE issue.

We now state: "Corrections to the HR-AMS and SP-AMS to account for the instrumental CE are required. For the HR-AMS, a CE < 1 is mostly caused by bounce of particles from the internal vaporizer surface (Huffman et al., 2005;Matthew et al., 2008)."

Line 22, measured not measusred

Done.

Page 24643, line 3 I think should be (SP-AMS, rBC)

Corrected.

Table 1, replace BC with rBC and eBC where appropriate. Typo in Extinction EF units.

These have been corrected, in the latter case to m^2/kg-fuel.

Page 24646 - line 18. Section 3.1.4 should be 3.1.3

Corrected.

Page 24647 Line 3. I am probably missing something here, but if you have 53% POM and 47% BC, then POM/BC is 1.13 isn't it?

The reviewer raises a good point. Yes, it would seem that this should be the case. However, if one calculates the POM/BC ratio for each intercept and then takes the average (which is what we had done), the result is 1.33. At the same time, if one calculates the POM fraction and then calculates the average one obtains 53%. This can be understood as follows. For a single measurement:

$$BC\ fraction = \frac{[BC]}{[POM] + [BC]}$$

$$\frac{1}{BC\ Fraction} = \frac{[POM] + [BC]}{[BC]} = \frac{[POM]}{[BC]} + 1$$

Because of this inverse relationship, when one uses *average* values then the simple relationship does not hold and one cannot calculate the *average* [POM]/[BC] from the *average* BC or POM fraction. We have confirmed that this is indeed the reason for this (apparent) discrepancy.

Line 18, first use of POC, please expand

Updated.

Figure 3. There are 4 speeds, 5 intercepts and yet 6 solid-line traces. Can the authors clarify what is being shown?

At 2.9 knots, the intercept was of sufficient length that the SMPS was able to complete two full scans, which are individually shown. This has been clarified in the figure caption.

Section 3.1.3/Fig 1. The authors state that the EFCN is not a function of the ship speed, but that the ratio of the 2 EFCN's could be a function of plume age. I suggest removing the ratio of EFCN from fig 1 as it is potentially mis-leading. It would be good to see what they mean by 'some correspondence' for the effect of plume age and coagulation. What I can't quite reconcile is whether the EFCN as a function of speed shows no trend because it is masked by the plume age. For example, if one could fit or assume a coagulation rate to the data and the EFCN were normalised to the same plume age, would a dependence on speed be observed? Is figure 1 d appropriate if it is affected by another process?

We have removed the ratio from Fig. 1. We have replaced the ratio with the approximate plume age to illustrate better what is meant by "some correspondence."

Fig 5/6, why does the speed ratio go above 1?

The speed ratio can go above 1 when vessels operate their engines above their maximum rated power.

I think it would be very useful to include the EF CO2, especially as a lot of the modelling work surrounding the implementation of the new MARPOL limits has focused on the potential CO2 effects.

We are unable to directly determine EF_{CO2}. This would require accounting quantitatively for the effects of dilution on the plumes. We rely on CO_2 as a conserved tracer within the plumes that inherently accounts for the effects of dilution and allows for quantitative determination of co-emitted species emission factors. We agree that this would be interesting, however any estimate of an EF for CO_2 would be highly uncertain and would require detailed modeling of the plume evolution and dilution and so we do not report it here.