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Interactive comment

Interactive comment on "Ship emissions measurement in the Arctic from plume intercepts of the Canadian Coast Guard Amundsen icebreaker from the Polar 6 aircraft platform" *by A. A. Aliabadi et al.*

Anonymous Referee #1

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General comments

This manuscript presents airborne observations of emission plumes from an icebreaking ship operating in Arctic waters, and allows the authors an opportunity to contribute to the existing body of literature on ship emissions. Specifically, the authors present calculations of plume expansion parameters in the Arctic boundary layer, and emission factors for an icebreaker under different operation conditions (open water vs. icebreaking operations). Both the emission factors and the plume parameters are important for the modeling of ship exhaust impacts in the Arctic. I found the scientific

significance and scientific quality of the manuscript to be good, and appropriate for publication. However, I currently find the presentation quality of the manuscript to be lacking, and a significant revision will be required before this manuscript should be accepted. Many of the figures will need to be improved for publication. Figures 2, 5, 6 use traces and axis labels that are too thin or light in color to be seen at the print size of the figure. Figure 8 has multiple vertical axis covered by scatter points and therefore cannot be read. More generally, I found that viewing the manuscript pdf at 300% magnification was necessary to begin to interpret the figures.

Specific Comments

Section 2.1

Can the authors categorize the Amundson by engine category (HSD/MSD/LSD) and fuel type (HFO/MGO)? This would be useful for the comparison to literature later in the text.

Section 2.2.2

In light of the small changes in mixing ratios of CO2 and CO reported, perhaps a discussion of precision of these measurements is important to assess the data quality used for the linear fits. It seems to me that stating only total uncertainty may undersell the quality of the data presented here.

Section 2.2.5

The discussion of the estimation of plume age is difficult to follow, in part because Figure 6 is very hard to interpret. The legend indicates large black circles for the ship track, which I don't see. Also the flight track and fitted plume center line are indistinguishable to me (I'm color-blind). I don't see where the intercept height is used anywhere in the analysis, and so can be removed from the figure. A modest proposal would be to simply color the flight track by NOx mixing ratio so the horizontal extent of the plume intercepts can be seen.

Parsing the text, I don't understand the description of equation 3. The plume center line is described as a high-order polynomial fit of the observed plume intercepts. The plume center line origin (l = 0) is not defined. Is it the ship track? Please add an additional sentence or two here to make this a bit more clear.

Section 3.1

Line 274, "flight tracks shown in Figure 2." Figure 2 shows time traces of wind speed and wind direction, not flight tracks. The measurements of wind speed and direction seem to have a bias with aircraft flight direction – at least this is how I'm interpreting the oscillations shown, especially in panels a) and b). Was this corrected for the plume age calculations? Can vertical lines or another marker be added to show when the plumes were intercepted?

Section 3.3

FLEXPART uses a 100m vertical and horizontal emission source, and the modeled plume structures are presented in Figure 7. For panels b) and c), the plume height shows very little increase across 15 km but considerable growth of plume width. The measured and modeled boundary layer heights for those days are higher than the plume heights, implying very slow vertical mixing. How does this compare with von Glasow [2003], who estimate $\beta \approx 0.6$ for the temperate MBL? FLEXPART seems to show $\alpha \gg \beta$ for panels b) and c), but not a). Can the authors discuss this, especially within the context of their observations? If $\alpha \gg \beta$ is true for b) and c) but not a), would this have an impact upon the calculation of γ in Section 3.5?

In light of the large uncertainty of the fitted line for the first plume intercept, can the authors discuss the uncertainty of the plume age calculation relative to the uncertainty of CO_2 enhancement?

Section 3.5

The interpretation of plume expansion rates in the boundary layer (BL) hinges upon

whether the plume has mixed vertically to the top of the BL. I may have missed it, but I can find no observations of the extent of plume vertical mixing, but rather a reliance upon FLEXPART. It strikes me as circular logic to use modeled vertical mixing to interpret the observed plumes and thereby calculate mixing parameters that will be used by models. Could the authors use a mass-balance approach (Ryerson et al., 1998; White et al., 1976) to the observed mixing ratios during plume intercepts to evaluate the extent of vertical mixing? Can observed plume widths be compared to the modeled widths, to at least confirm that horizontal mixing is correct in FLEXPART? Can the authors make any calculation of α rather than just γ?

Section 3.6

Figure 8 shows changes in CO2 mixing ratio of approximately 3 – 7 ppm CO2 for the three plume intercepts shown, but the x-axis of the scatterplots for plume 3 [Figure 10, panel c) and Figure 11, panel c)] have a dynamic range of 1.2 ppm. The data shown in Figure 8 also includes many more data point than is shown in Figures 10 and 11, panel c). Am I confusing things here? Are the scatterplots (Fig 10 and 11) showing something other than the 1s data shown in Figure 8?

The fits of O3 vs CO2 have no relevance to either calculations of emission factors or plume dispersion, and should be removed.

The authors should explain how the uncertainties were calculated for the slopes of the fitted lines presented in Table 1. It appears that the fits are linear regressions, while bivariate least squares fits would be appropriate – both x and y variables have observational errors. Also, the individual uncertainties of the data points is known and can be used to weight the data. Finally, the stated uncertainty for CO2 is 0.3 ppm, while the range of enhanced CO2 mixing ratios for plume 1 is only 0.2 ppm, i.e. not significant. The data appears to show meaningful correlations, so I suspect the instrumental uncertainties are overly conservative.

Section 3.7

It appears that Tables 3-7 are redundant with Figures 12-16. The authors should use only one or the other to present this data. Figures 12-16 would also be well-served to include the uncertainty range for the authors' data points, if not for all data.

The NOX EF reported for plume 1 (open water) appears anomalously low compared to the existing literature. Can the authors discuss whether this is due to their ability to interpret the measured data, or if this is reasonable for the ship under the operating conditions at the time?

Technical Corrections

Figure 15 caption has a typo ("for for").

Ryerson, T. B., Buhr, M. P., Frost, G. J., Goldan, P. D., Holloway, J. S., Hubler, G., Jobson, B. T., Kuster, W. C., McKeen, S. A., Parrish, D. D., Roberts, J. M., Sueper, D. T., Trainer, M., Williams, J., and Fehsenfeld, F. C.: Emissions lifetimes and ozone formation in power plant plumes, Journal of Geophysical Research-Atmospheres, 103, 22569-22583, 1998.

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