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Interactive comment on "Studies of the horizontal inhomogeneities in NO_2 concentrations above a shipping lane using ground-based MAX-DOAS and airborne imaging DOAS measurements" *by* André Seyler et al.

Anonymous Referee #1

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GENERAL COMMENTS

The manuscript "Studies of the horizontal inhomogeneities in NO2 concentrations above a shipping lane using ground-based MAX-DOAS and airborne imaging DOAS measurements" presents nicely and picturesque the onion peeling approach applied to measurements in the German bight, demonstrated on individual measurements. In the second part of the manuscript, the authors compare 2 specific measurement instances to airborne imaging DOAS measurements taken during the NOSE campaign. These two instances show well the validity of the onion peeling approach, qualitatively and

quantitatively.

While this second part, the comparison with imaging doas, makes also quantitative estimates, the first part, showing two times example measurements within approximately 12 minutes in 5 different azimuth directions, stays very qualitative. It neither includes an estimation of errors by e.g. the negligence of the correction factor in the O4 scaling approach for the effective light path estimation, nor does it include an attempt of making use of some ancillary information about the plume using e.g. the STEM model (Jalkanen et al. 2009 acp 9209-9223 , 2012 acp 2641-2659) and some diffusion model to estimate plume width/ height.

The authors argue that the presented method is suited to measure concentrations when wind conditions are unsuitable for surface measurements which do not measure any enhanced concentration if the wind blows the plume away from the measurement station. However, they themselves mention that the measured concentration with the onion peeling approach is not representative of the in-plume concentration. The authors lack to investigate possibilities how to extract useful information using other available information (using more info from the AIS data in combination with the STEM model and better modelling of the plumes). No attempt was made to connect the measured concentration to the in-plume concentration in the first part of the paper. Even if this is not carried out, I strongly recommend the authors to think about ways how this could be done and at least describe what could be done. Without it, this method seems rather incomplete and its usefulness quite limited.

However, the paper is very well presented and shows that if complementary measurements are available, useful estimates about plume concentrations of individual plumes can be calculated and hence should be published! It remains nevertheless unclear what the main purpose of the measurements is if no connection can be made to the in-plume concentration. This could certainly be clarified better both in the introduction and in the conclusions. I recommend to either include attempts to extract more information about the actual plume concentrations by using more ancillary information in the first part of the paper (basically describing Fig. 6, 7, 8 and 9) or skipping that part and only concentrating on the second part. In the current state, it is a long paper that, over big parts, is rather qualitative and does not give much quantitative information about plume concentrations and hence is not quite suited for measurements of ship emissions on its own.

SPECIFIC COMMENTS

(a) The authors only mention the restrictions on sulphur emission by the MARPOL convention. However, this manuscript is about NO2, so it should probably also give the info on this. Something like... EU adaption of this in form of directive 2012/33/EU NOx emissions depends on the rated rotational speed of the engine crankshaft, implementation in 3 tiers, last one not yet implemented, shifted to $\sim$2021

(b) In the last paragraph of Sect. 3.2, the authors mention the importance of NO to NO2 conversion. Maybe the authors can give some estimates on time- (and spatial) scales for the increase (probably depending on some "standard"(?) background O3 concentration). Also, the authors mention in Sect. 3.3 that plume broadening and dilution over time is neglected. Maybe an order of magnitude estimation should be included and it should be outlined how this information can be used to extract more useful information from the measurements. Which of the two effects dominates for which time-scales? Maybe a reference to some dispersion models that include chemistry?

(c) In Sect. 3.3 it is stated that the initialization period is 90 minutes before the measurements. The big plume (roughly N-S direction) present in all panels in Figure 8 (and 9), originates, according to the authors (Sect. 4.3 3rd paragraph) from two coal-fired power plants in Wilhelmshaven, 50 km away. The authors estimate the plume age to be 110 minutes. However, this suggests that the initialization period needed to be larger than 90 minutes, otherwise the plume would not have had the time to travel that far. I think this should be clarified.

(d) Regarding the plume trajectories, maybe the "apparent wind" approach as illus-

trated e.g. in Berg et al (2012, amt 1085-1098) should be referenced.

(e) It is mentioned that there are two stations measuring wind conditions, one on Neuwerk, one on Scharhörn. However, it is not clear whether the wind used for the calculation of the plume trajectories in e.g. Fig. 6 or Fig.8 is a simple average of the two, or if it depends on the position of the plume at every given moment which wind (some sort of spatial interpolation) is applied, or if only one is used. Please clarify.

(f) Regarding the author's comment to Fig. 6 panel 1 why the plume of the small ship is not seen: Maybe the authors could do a quick calculation which heights are seen at the expected distance of the plume (about from about 20–40 m ?). What do the authors find more likely? Maybe using the STEAM model for that particular ship, together with estimates for the dilution due to diffusion and NO to NO2 conversion the authors could approximate in plume concentration and exclude or not exclude their first alternative.

(g) Can the authors comment on the effect on the MAXDOAS results when the plume is over the instrument, as also indicated by high in-situ measurements? Does this lead to cancelling effects or is the vertical extend of the plume negligible comparable to the horizontal?

(h) Fig6, panel 10: The plume from the big ship cannot have yet reached the VIS-only region (Delta L). However, compared to the measurement 4 minutes before, the VMR seems to have increased by around 1.5 ppb. Any suggestion why?

(i) Similarly, panel 15 seem to indicate a larger intersect of the plume with the viewing direction for the UV region than for the VIS-only region. Still it looks like (as mentioned below, maybe not the best choice of colour map) the VIS-only region has a much higher average VMR. Probably this is due to the effect of overestimated length (due to negligence of correction factor as mentioned by the authors) and hence more of the intersection is in the UV-only path?

(j) The first two (not numbered) equations seem to indicate that the air density in fact cancels out in the authors approach to estimate the VMR since only surface values for concentrations?

(k) The title suggests a more "equal weight" between the two methods in terms of "being presented". However, the imaging approach seems to be merely used for validation and is not presented as such, since this is done in a different publication. Maybe the title should reflect this.

(l) The authors conclude in their last sentence of the manuscript that this approach can be successfully applied to ship emission measurements. Nowhere in the paper is an estimation of the ship emission presented. I advice the authors to delete or reformulate this sentence.

TECHNICAL CORRECTIONS AND SUGGESTIONS

(a) page 2, line 31: ... is of (not on) the order of....

(b) page 5/6: 2 equations on these sides are not labelled. I think all equations should be labelled.

(c) page 8, last sentence of penultimate paragraph: This is a really confusing sentence. I would probably reformulate to something like: "The movement of the ship together with the measured wind results in an apparent wind direction very different from the measured wind direction. Therefore, a measurement along the measured wind direction does not in genereal correspond to measurements along the plume".

(d) Fig. 3,4 and 5: For easier reference, it might be a good idea to bundle those into one figure with 3 vertically aligned panels.

(e) Fig 6 and 8: maybe a length scale would be nice to include. Also, I suggest to label the viewing directions on the right-hand side on each row. I am not sure if a jet-like colour scale is the best choice. The gnuplot type one used in Fig. 1 or viridis or any other colour scale that is monotone in lightness (The first and the last comment also hold for Fig. 7 and 9) would be better. However, maybe that is just something the

authors can keep in mind for the next publication.

(f) page 13, line 2: "lightboth"??

(g) page 14, Sect. 4.3, second paragraph: Figure 8... (not Figure 6)

(h) Figures 1,6,7,8, and 9: Can the authors quickly state why Nigelhörn and Scharhörn got merged into one island in their map?

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