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Interactive comment

Interactive comment on “Monitoring shipping emissions in the German Bight using MAX-DOAS measurements” *by* André Seyler et al.

Anonymous Referee #2

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General comment: This paper describes a 3 year series of multi axis DOAS measurements carried out from the German island Neuwerk, just south of the entry to the river Elbe. This is in the main ship channel of the port of Hamburg and the main aim of the measurements was to study these by observing UV and visible light horizontally towards the ship channel. The paper is well written, with good language and instructive graphs. The paper is a nice combination of measurements methodology and results paper. It shows the methodology to measure mixing ratios in a coastal places, together with ship plume measurements and some results about the effect of new IMO legislation. However, the OBJECTIVE and AIM should be declared more clearly in the text. The paper is also rather long, and I would recommend to shorten it, by removing sections which are outside the main scope of the paper. Forinstance merging and shortening sect 4.5 and 4.6 corresponding to mixing ratio measurements

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Discussion paper

and comparisons. .All in all, I believe the paper should be published, with some minor improvements, based on answering my specific comments below:

Specific comments: Row 71, p 2: It is claimed that 25% of the NOx emerges as NO2 from the stack, but usually 10% is assumed from fluegas stacks; please give more details: I assume you also assume some titration?

Row 278, p 9: IN the equation do you fit differential absorption cross sections or the absolute ones? Since you are using prime I assume you mean the differential ones; IN

row 336 I however get the impression that you use the absolute ones.

Row 311, p 10: It is claimed that the vertical paths cancels out between path 1 and 2 in Fig 5; I agree with the stratospheric portion but for the tropospheric part there should be a cos (SZA) difference, even if NO2 is homogenously distributed in the troposphere?

Row 387, p 13: Is it assumed that the wavelength difference in O4 signal is linear; if so what are the uncertainties involved?

Row 406, p 14: It is claimed that the conditions at the Neuwerk radar tower is similar to measurements from high mountains; please motivate better. Eq 4, p 13: It is difficult to follow how you get the expression in eq 4.

Row 464 p 16: On this place, and some others, its is claimed that the differential slant columns are higher for SSE and ESE and (more elevated). But part of this should be wind speed effect since I would imagine that the wind speed will be higher from the sea and this will dilute the slant columns more. Has this been investigated ?

Row 470 p 16: Graph 7 is not totally clear. If I understand right the plot correspond to overlayed windroses for different elevation angles rather than that the area of each color represents the wind rose information. I interpreted the latter and I think this should be clarified forinstance in the figure text.

Row 500 p 17: You here discuss the results in Fig 8. The differences in the UV and visible are explained from the penetration length, but should the Visible not in general be higher since it gives the chance of penetrating plumes further away, rather than the opposite which appears to be the case for all plumes here? You explain that the UV should be stronger for close by plumes since a higher fraction of the photons are then affected by absorption. Is is not so that the O4 can only simulate slow variations? Please elaborate..

Row 614 p 25: You claim that fig 12/fig 13 shows good agreement between MAX DOAS and in situ, but in my mind this is the case for Fig 13 but not for fig 12, where there appears to be rather big difference in he averages of the two sensors with factor 2-3?

Row 665 p 27: You suddenly refer to fig 20, without having mentioned fig 17-19 yet in the text. You should consider reordering.

Row 891 p 35: As concluded here and discussed in section 4.9, the ratio of SO2/NO2 gives an indication of sulfur fuel content in ship plumes. Are you aware that SO2/NO2 ratio measurements from airborne DOAS is used operationally since 2015 by Beecken and Mellqvist (Chalmers University) in the CompMon project and surveillance around Denmark and that this has been presented on several official workshops last year? You mention that the +2015 measurements are biased by noise since you don't really observe any SO2 then. I don't think it then makes sense to show the green data (+2015) in figure 5 since these histograms then only represent noise? Secondly you don't mention when comparing to other measurements that the amount of NO to NO2 titration is very important for the ratio, and this will depend on the distance to the plume, whether you are over land or sea etc. Please add some discussion on this.

Row 903 p 35: It is mentioned that there are still SO2 coming from land. This is surprising since there are very few SO2 emission sources anymore and power plants generally have abatement equipment. It would be interesting to understand this better ?

Technical Corrections: Well written in most places. Row 812 p 31: Change limis to limits Row 873 p 33: Change This to These

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