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Interactive Comment

Interactive comment on "Characterization of OMI tropospheric NO₂ over the Baltic Sea region" *by* I. Ialongo et al.

Anonymous Referee #1

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The paper "Characterization of OMI tropospheric NO2 over the Baltic Sea region" by I. Ialongo et al. presents some exemplary case studies of NO2 patterns in the Baltic sea. Though these studies are potentially interesting and generally match the scope of ACP, the current manuscript is rather sketchy. "Emissions" are presented as "burden parameter" in units of molecules instead of e.g. molec/sec, and comparisons to emission inventories are not provided. Discussion of uncertainties is only rudimentary and rather qualitative, but still the uncertainty of the estimated "emissions" and lifetime is stated as low as 10% in the abstract, which I consider as completely unrealistic. Major revisions are required before possible acceptance on ACP.

1. Focus and structure

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The manuscript contains a mixture of different aspects (the city of Helsinki, a ship track, year-to-year variations), but each of these is only touched superficially.

According to the title, the focus of the paper is the characterization of NO2 over the Baltic Sea. To strenghten this focus, a more complete approach is needed. I.e. the discussion of cities can not be limitied to Helsinki, but has to include other cities like Stockholm and in particular Saint Petersburg. Does the lifetime/emission estimate work there as well? If not, why?

According to Figs. 1 and 5, there are significant NOx sources South-West of the considered region. The comparison of calm and windy conditions in Fig. 5 clearly reveals that the Baltic Sea is affected by NOx outflow from these sources. These sources have thus to be identified and their impact has to be discussed and compared to local sources.

The different studies (ship tracks, cities etc.) should be separated by subsections.

2. Methodology

The applied methods are only sparsely described and there are some inconsistencies and mistakes:

a) OMI data

It is stated several times in the manuscript that the OMI pixel size is 13x24 kmˆ2. But this is only true for nadir geometry, and pixel size increases significantly towards the swath edges. This has to be clearly stated. How are pixels at the swath edge are treated? (E.g., Beirle et al. removed the outermost 10 pixels on each side of the swath.)

b) Lifetime and emission estimate

- The authors refer to the method proposed by Beirle et al. and indicate that they apply the same method to Helsinki. However, there are several differences with respect to

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the details (e.g., only 4 wind directions are considered instead of 8; a "calm threshold" of 5 m/s was chosen instead of 2 m/s etc.). Thus, a more detailed summary of the Beirle et al. approach has to be given, and different implementations have to be clearly indicated.

- In Beirle et al., emission rates are derived (mol/s), while in Ialongo et al., just a "emission factor" is given in units of molecules. What is the physical meaning of this "emission factor"? How can this E result in an NO2 line density if multiplied by an exponential decay and smoothed with a Gaussian (both unitless)? Check the units and provide emission rates instead of an "emission factor". The resulting emissions should also be compared to emission inventories.

- One implicit assumption of Beirle et al. is that the source is "point like" or at least symmetric (i.e. the spatial distribution can be accounted for by the convolution with a Gaussian). However, if the distribution of sources is asymmetric, this alone would cause a virtual "outflow" pattern, even without any wind, and would thus bias the fitted lifetime. This potential bias is reduced if different (in particular opposite) wind directions are fitted (as in Beirle et al.), but this is not the case here. Please discuss; does the mean line density for calm conditions look symmetric?

- The discussion of errors is very short. A simple reference to Beirle et al. is not sufficient here. If the authors claim that emissions and lifetimes can be estimated for Helsinki, they also have to provide a dedicated (and realistic) discussion of uncertainties for Helsinki, beyond the errors derived from the fit.

3. Ship tracks

The study of an exemplary ship track is not convincing:

a) The ship track seems to be interrupted at about 20.5°E/58.3°N. Please comment.

b) The integrated NO2 amount obviously depends on the choice of the considered box. In the paper, it is close to (and downwind from!) Gotland, an Island with several

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oil production facilities

(http://mapx.map.vgd.gov.lv/geo3/VGD_OIL_PAGE/images/Baltic_province_new_2009.jpg)

and a lot of tourists during summer. Fig. 5 (a) looks like the "shiptrack" is just crossing the southern dip of Gotland. Please comment.

c) Most alarmingly, the shiptrack pattern is far more distinct for windy conditions (Fig. 5)! My concern is that this may be just caused by the a-priori: The AMFs are lower over the shiptrack, as the model predicts a different (lower) profile shape, resulting in artificially enhanced tropospheric columns, as long as there is some tropospheric residue to be increased (i.e. under windy conditions, transporting NO2 from SW). This possible artefact might be ruled out by analyzing the mean (tropospheric) slant columns.

Further comments:

P2023 L7: At this point, Beirle et al. is not an appropriate reference, as it neither deals with ship emissions nor the global NOx production, but focusses on Megacities.

P2024 L10: The "strong need" for monitoring NOx emissions from ships is only given if there is significant ship traffic in the Baltic sea. Please quantify.

P2025 L1: I do not see the argument. As there are still high uncertainties in NOx emissions as well as chemistry, I would rather focus on strong sources at moderate latitudes, where the retrieval uncertainties are relatively low.

P2029 L7-8: Why is this a "logarithmic distribution"? What I read about logarithmic distributions, they are only defined for integers and look quite different than Fig. 2.

P2032 L17-19: The sorting of data according to wind direction has actually been proposed and described in Beirle et al., 2011.

P2032 L20: What "good agreement with NOx emission data" is referred here?

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Figure 2: The 30° intervals are not consistent with the grouping of OMI observations (N, S, E, W). The definition of 0 as wind from West to East does not match the ECMWF definition: see http://www.ecmwf.int/products/data/archive/data_faq.html#wavedirection

Figure 3: What does white mean - a gap or a value below 1.8e15? Please modify the plot such that gaps can be discriminated from low values.

Figure 4: How can the mean wind be 4.9 m/s, if only wind speeds above 5 m/s have been selected? If this is the consequence of adding wind vectors, this would be inconsistent with the definition of w as projected component (P2026 L21).

Figure 6: The black boxes in the left and the center panel do not match.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2021, 2014.