**Atmospheric
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Discussions

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

Anonymous Referee #2

Received and published: 22 February 2014

This paper presents an analysis of tropospheric NO2 as measured by the OMI instrument over the Baltic sea region, covering an area of about 800 km by 700 km. Their analysis focused on an estimating emissions but also provided maps and trends, based on annual seasonal and multi-annual means. The analysis methods used in this work are not new, but this paper does a reasonable job of applying them to provide a comprehensive analysis of one particular region. While the topic is of scientific interest and there is real potential in this work, in my opinion there are two overarching issues that need to address before it can be accepted.

1. The quality of English needs to be improved. There are numerous grammatical errors, e.g,

page 2024, line 25 "...being the Baltic Sea area relatively small..." —> "... being that the Baltic Sea area is relatively small..."

Furthermore, beyond these there are many examples which sounds odd, e.g.,

page 2022, line 20 "...as far as they are..." —> should be "... as long as they are..."

I have pointed out a few additional examples below but there are likely several that have been missed. I suggest that once the scientific issues have been addressed that it be critically reviewed for grammar and flow by one or two English colleagues.

2. Analysis Details and of uncertainties

As far as I can determine there is no analysis of uncertainty. The statistical uncertainty coming from the non-linear fitting is provided, but beyond that the only real mention is "for a complete analysis of the uncertainties see Beirle et al. (2011)". I assume that this means all other sources of uncertainty were ignored. The statistical uncertainty will be small compared to the other random and systematic sources of error, and these other sources are not even mentioned (let alone quantified). The 10% error assigned to the NO2 emission rate (E=1.0 +/- 0.1) is totally misleading. In contrast, locations analysed in Beirle et al had errors more like 50%, and these locations had larger emissions where presumably the relative errors would be smaller. Furthermore, NOx will be emitted primarily in the form on NO and not NO2.

The emissions values are only as good as their uncertainties. A detailed and convincing analysis needs to be performed for several reason, not the least of which being that there are many sceptics in this field that would not put much stock in satellite-derived emissions. Quoting uncertainties of 10% would only provide them ammunition. Beirle et al. would be a good guide for this as they have examined several sources of random error. Beyond that addition sources of systematic should be considered:

a. clear-sky bias: Only OMI measurements over clear skies are considered. How might this bias the results? There are a couple of papers that have looked at this: Geddes et

al. (Remote Sensing of Environment, 2013), McLinden et al. (ACPD, 2014).

b. GMI model used in SP retrieval: The emissions used by the GMI model are from 1997 or 1998, and these impact the profile shapes and thus air mass factors and VCDs. How have emissions in the Baltic area changed since then and discuss how this could bias your emissions numbers.

c. Winds: What ECMWF reanalysis was used, what is its resolution. Why use winds at 950 hPa? This corresponds to what, 250 or 300 m? I am guessing that at the locations considered here the wind speed increases rapidly with altitude and so an average wind speed over the boundary layer could be twice what are used here. This would have large implications on the derived lifetime and emission rate. Discuss this.

Other comments:

page 2022, line 7: Measurements over snow will help with signals. Rework this sentence to as to not mix up the two issues (snow –> high signals, other complicating issues / high latitude –> lower signals)

page 2022, line 16: Provide the emission rate in terms of mass as this is more useful, either in addition to molecules or instead of. Is this an annual amount or for the summer only. It should be converted into a rate.

page 2024, line 28: one exception is the oil sands work of McLinden et al (GRL 2012), you should add this as a counter example

page 2023, line 6: "as they represent a relevant part" ... I assume you mean the portion of ship emissions is large enough that they need to be considered. "Relevant" means connected with or pertinent. I would suggest you rephrase this using "as they represent a sizeable fraction" or something similar

page 2024, line 7: "could increase" - this sounds odd since 2012 is in the past. use "may have increased" or something analogous.

page 2025, line 1: "remain still large" —> "remain large"; likewise "lifetime estimations" is probably better phrased as "estimates of lifetime"

page 2026, line 1: Why consider only June-August? Why not May and September? These should also be snow free and will increase you signal to noise.

page 2026, line 5: The local time of the Baltic is UTC+2? State this here so that readers know that UTC 12:00 is a good match with the OMI overpass time. It is not obvious otherwise.

Figure 1: It is difficult to make out the letters in the panels on the left.

Figure 2: Figure 2 shows the distribution of winds. Have these been sampled in the same way as OMI (considering only clear skies)? If not, they should be as this can dramatically change the patterns. Redo these, or add additional panels, showing the clear-sky wind distributions.