

Interactive comment on “Temporal and Spatial variation of Contribution from Ship Emissions to the concentration and deposition of air pollutants in the Baltic Sea” by Karin Haglund et al.

Anonymous Referee #2

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Temporal and Spatial variation of Contribution from Ship Emissions to the concentration and deposition of air pollutants in the Baltic Sea

Anonymous Review

General

The paper presents and discusses results of a numerical modelling study of the influence of international shipping on concentrations and deposition of pollutants (i.e. sulphur) in the Baltic and North Sea regions. The model used was the EMEP chemistry transport model with a grid resolution of about 50 km x 50 km, target years are

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2009 to 2013. In addition, scenarios based on estimates of future conditions (different fuel sulphur content due to upcoming regulations) were also created and evaluated. Additionally the effect of open loop scrubber water release on the acidification of the Baltic Sea has been assessed. Unfortunately, the influence of a nitrogen emission control area (NECA), which has now been decided to come into effect in 2021, has not been considered. In general this is an interesting study addressing a relevant and current topic discussed by pollution assessments and in connection with international governance. But the paper seems not be ready for publication in the present form for several reasons. It could be published after some revisions.

Major remarks

i) It looks like that the text in parts has more the status of a draft paper (e.g. some untidy parts in the reference list and missing words), and it seems not to have been carefully read by someone, who is familiar with the English language (best a native speaker) before submission. There are many language issues which need to be clarified/corrected by someone experienced (the reviewer does not intervene here, since it is assumed that many text passages read differently after the paper has been revised). Often the line of thought is not easy to follow, especially in the sections describing methodological aspects.

ii) The paper cites the study of Jonson et al. (2015), which uses the same CTM (EMEP model, but with a higher spatial resolution) to evaluate the influence of shipping emissions on the air quality in the same region (Baltic and North Seas), discussing also future settings. In addition the current day shipping emissions in that paper are based on AIS data in combination with data from a technical ship data base, this is more or less state of the art. Here the present paper should tell the reader “what are the major differences between the two modelling exercises”, i.e. to underline the added value of the present modelling study. It would helpful to learn why the relative coarse grid resolution has been used, although present day computing power should allow for better. It would also be interesting to learn why the authors decided for a non-AIS based ship

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emission approach. It would very interesting to compare results of the two modelling studies (Jonson et al. and present), since they have overlapping modelled years and consider the same region. It should at least be tried to summarize differences and similarities in the results in more thorough manner.

iii) In the introduction to section 3 it is mentioned that the considered time period (2009 – 2013) was covered by two consecutive runs with different model versions (rv4.4 for 2009 to 2011 and rv4.8 for 2011 to 2013). Why this? It might be problematic. It is necessary to inform the reader on the differences between those model versions. Do they come to similar results, if the otherwise same settings are used? 2011 seems to have been modelled using both versions, this would an ideal case to compare the results. Please comment. Why has the period not been cover by a run using the newest version of the EMEP model?

Some minor remarks

Title: The title of the paper should include a hint that also future scenarios have been generated and evaluated. Why is the North Sea not in the title, although it has been modeled and is partly discussed by the paper? Is it modelled as source for advected pollutants with south westerly winds? At least in the beginning of the Abstract both seas are mentioned. If you have a special reason, why your evaluation concentrates on the Baltic Sea (although the NS is also modelled), please provide it in the introduction to the paper.

Abstract: In the Abstract a lot of numerical numbers are reported, which in general is good. Many of them are given as “maxium %”, which is not so easy to conceive. To better grasp the pollution situation it is important for the reader to know: “Was this maximum (ore close values) reached several times in the period?” or was it a unique value (mean over all years?) in the distribution of concentrations and deposition values? Was it the result for a tiny spatial sector (it is anticipated that the smallest possible is 50 x 50 km²) or was a larger area affected? In short: Are the reported

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maximum values relevant or are the extreme outliers? It would be better to provide also another statistical value (mean, std . . . , and max.) for high concentration areas and background. (To concentrate on reporting maximum values is also unfortunate, since on page 6, line 8, it is stated that the model has difficulties to represent maximum values. This was for Vavihill and Utö, but might be the case everywhere).

Nomenclature: The model used is the EMEP Chemistry Transport Model, it should be consequently named “EMEP model” throughout the text (and e.g. not just “EMEP”, which is a programme).

Wrong section: On page 4, line 5 to 9; the information given here does not belong into a section called “Regulation of Shipping”. Shift it.

Scope of paper: On page 2, line 17, it is written: “Here emissions of sulphurdioxide, nitrogen oxides and particulate matter are examined.” No, this is not the case. The resulting concentrations and deposition values are examined. May be the effect of emissions are examined. This is not a work which concentrates on gaining better emissions.

Deaths: “Corbett et al. (2007) showed that shipping-related emissions of particulate matter contribute to approximately 60,000 deaths annually on a global scale, . . . “. Corbett et al. did not show this! It is at best a crude estimate.

New regulations: At the end of 2016 it has been internationally decided, that in 2021 a NECA will be implemented in the Baltic Sea and North Sea. In 2020 the Sulphur content in shipping fuels will be globally limited to 0.5%. This needs to be mentioned now in a future section 1.2. The latter value is relevant for your scrubber scenario reported at the end of section 3.2, where you still work with 2.7% S. Since the ships need to hold 0.5% globally, the owners will most prob. use fuel with this value (which is discussed for a longer time and now decided). Please comment.

Methodology: Please provide in section 3 a reason, why you choose the time period

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2009 to 2013. Are there meteorological (e.g. variability) or computing limitation or emission data reasons?

2013: Please provide a rational, why a huge part of your discussion is addressing 2013. There might be a good reason (e.g. representative). Please discuss this selection more expanded.

Consistent: Last sentence of the paper in the summary: "The validation of the model showed that the model underestimated most of the pollutants but the model was overall consistent with the measured data in 2013 at Vavihill and Utö." How can this be (the model often underestimates)? Are these locations totally different compared to the domain. What does "consistent" mean here? Looking into figure 3 there may be some consistencies but as many inconsistencies. Please explain better your understanding of "consistency".

Figures: All figures should have units at their axis (only some have).

References:

page 2, line 13: the third reference should read "Matthias et al. 2010", also in reference list page 16, line 6 (Volker is the given name).

page 3, line 2: the references Arya (1999) and Raven and Berg (2006) are text books, an interested reader on the nitrogen impacts has to dig a lot in those books to find the relevant pages. Please provide more dedicated references here; there are many (and better ones).

page 14, line 4 to 9: here a few references (3) seem to be lumped together into one paragraph.

page 14, line 20: the paper by Jonson et al. 2015 is available as full ACP paper for 2015 (Vol. 15 /83-798), not ACPD.

page 16, line 10: it should be H. (Hulda) Winnes.

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