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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Environmental impacts of shipping in 2030 with a particular focus on the Arctic region" *by* S. B. Dalsøren et al.

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The paper seems to be a straightforward application of previously documented models covering an important and 'hot' topic, the impact of increased shipping emissions in the Arctic region. I think the manuscript is fine in general, but quite a few clarifications and improvements (including better Figures) are needed before acceptance for ACP.

>We thank the referee for the comments and suggestions leading to an improvement of the paper.

* There have been previous studies looking at the impact of these increased shipping emissions. The authors need to be very explicit in explaining why another paper is needed, and what is new.

>The reason for another paper was stated in the introduction: "In this study we use high resolution ship emission inventories for the Arctic more suitable for regional scale evaluation than those used in former studies. Some model studies have been done on impacts of future ship emissions in the Arctic but these are mainly made for parts of the region or based on simplified or old projections." To be more explicit we have now included the first sentence also in the abstract.

* On the same theme, this paper presents a number of results for changes in ozone, NO2, etc., but only one coarse-resolution model is used. The authors need to discuss how this model's results sit in the context of other studies done with other CTMs (Granier, Eyring, etc). For example, on P26649 we read that Granier et al. found factors of 2-3 changes in ozone, much larger than those found in this study. Why?

>Comparison with other studies would have been relevant but the different emission inventories used makes it less useful. For the Eyring et al. the reason is already stated in the Discussion section: "Eyring et al. (2007) performed multi-model calculations for ship emissions in 2030. One of the scenarios assumed a 2.2 % annual growth from 2000 which is quite similar to the MFR scenario. However, the results for the MFR found here are not very well suited for comparison as the assumed emission distributions are very different. The Eyring et al. (2007) study used a dataset only accounting for a few of the major trade routes." Regarding the Granier et al. study we have now added the following text to the same paragraph in the Discussion section: "Granier et al. (2006) finds larger increases in ozone and NOx along diversion routes than this study. The reasons for the differences are probably related to higher emissions in 2050 of 0.65 and 1.3 Tg (N) compared to 0.12 Tg (N) in the HIGH case in 2030 in this study. Granier et al. (2006) also introduces the traffic one month earlier (July) and Arctic ship emissions are absent in their basis simulation whereas this study uses year 2004 emissions as basis. These factors would make ozone production more efficient in the Granier et al. (2006) study."

* The issue of non-linearity in modelling emissions from ship-plumes has been known

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and studied for a long time (e.g. Song et al., 2003, Kasibhatla et al., 2000). This is an important issue, but here is only brought up in 2 sentences in this study. I suspect this issue is a significant source of uncertainty in the calculations presented here, and it deserves a proper discussion.

>The following text addresses this in the Discussion section:"It should however be noted that this study might overestimate the concentration change and RF of ozone due to the coarse resolution in the simulations with the OsloCTM2 model. Not resolving the scales of the chemical and physical processes in the exhaust plumes might lead to prediction of too high ozone production per emitted NOx molecule (Paoli et al., 2011). The effect of an Arctic ECA would be less if plume chemistry reduces ozone production efficiency in the Arctic similar to what studies indicate for low latitudes."

As written we agree that this might be a significant source of uncertainty. We limit the references to the study by Paoli et al. since this is a review paper containing numerous references to most studies on this. However, no/few studies or measurement campaigns exist on possible plume effects in the Arctic and it is therefore difficult to quantify this uncertainty.

* I missed a Tabulation of the non-Arctic emissions used in this study. This could well be an extension of Table 1. In any case, Table 1 should make it clear that the numbers come from Corbett et al.

>We agree that a tabulation of the non-Arctic emissions is useful. It is now added. It is also added that the numbers for the Arctic is from Corbett et al. (2010).

* OC is discussed without mention of secondary compounds. I assume the authors are modelling only a primary inert OC compound? If so, what are the implications of this assumption for the RF calculations?

>The organic matter (OM) emission factor used is an average from about 100 ship plume measurements at mid- latitudes, where the plume was in equilibrium as the mea-

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surements were taken. The emission factor therefore reports the OM after the semi-volatiles have either evaporated or condensed, depending on their structure/volatility. It must be recognized that the OM emission factor is an average with a wide standard deviation. A cooler Arctic may encourage a higher condensation of semi-volatiles, but this would probably be swamped by the variability already reported in the emission factor. Any formation of secondary species, and associated uncertainty is therefore linked to the VOC inventory and secondary organic aerosol (SOA) formation processes. The SOA scheme implemented in OsloCTM2 also shows small SOA values over the Arctic regions (Hoyle et al., 2007). Therefore only primary OC have been included in these simulations and we have added the following sentences to the first paragraph of the discussion section:"The organic matter (OM) emission factor used is an average from about 100 ship plume measurements and reports the OM after the semi-volatiles have either evaporated or condensed. Any formation of secondary species, and associated uncertainty is therefore linked to the VOC inventory and secondary organic aerosol (SOA) formation processes. The SOA scheme implemented in OsloCTM2 also shows small SOA values over the Arctic region (Hoyle et al., 2007). For these reasons only primary OC was included in the simulations."

* Why are the HIGH and MFR SO4 direct so similar for summertime in Fig. 11? Table 1 suggest different emissions.

>Large reduction in sulfur emissions in both the HIGH and MFR scenarios results in rather similar emissions of sulfur (see table 1a) for the 2030 regional Arctic fleet. The large difference in sulfur emissions between the scenarios is mainly found for the diversion fleet (table 1a). This fleet only operates in the period August-October (table 1a caption text). For this period there is quite large difference in SO4 direct (and indirect effect) in Figure 11.

* In many places (e.g. starting on L16 of the Introduction, or the second paragraph of the conclusions section), the authors resort to qualitative terms, with changes being large, significant, etc.,but with the reader being given no clue as to this means). If

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a change is mentioned, quantify it. This vagueness gives a careless impression and makes it hard for the reader.

>We agree that qualitative terms should be avoided. We are now more quantitative throughout the text.

* Figures: In general, the quality of the Figures is quite poor - the maps are hard to read as they are so small, captions are not self-explanatory, and fonts in some Figures also need improvement. In more detail: Type: The focus is on the Arctic, but the maps make this nearly impossible to see in any detail. Polar stereographic maps would be much better, as done in Odemark et al. 2012. For the few occasions where the S. Hemisphere is mentioned, it would be enough to just use text or Supplementary material.

>We agree that polar stereographic maps would better highlight the Arctic. However, there are whole paragraphs and sections discussing non-Arctic concentration changes and global radiative forcing. We therefore feel that showing global maps is slightly more informative for the overall overview.

Quality: The fonts and colourbars used need improving, e.g the numbers run into each other in Fig. 1a, and the text in Fig. 6 is tiny, being far inferior to that used for Fig. 7.

>The fonts and colourbar in Fig. 1a and Fig. 6 is now improved to make it more readable.

Captions: - Many of the captions say the same thing, but for different pollutants. It is then clearer to say, "as Fig. XXX, for YYY".

> We agree and have changed captions in accordance with the suggestion.

- Use (a), (b), ... etc, instead of the wordy and less-clear upper-left, upper-right.

> The suggestion gives a better solution. We have changed to (a), (b), ... etc

- Figs 1-4. What does at the surface mean? True surface concentrations are zero for depositing pollutants.

>True, instead of surface we now say “lowest model layer close to the surface”

- Figs 6-11. The terminology is very unclear. What is RF 2004-2030? I assume the authors mean changes in radiative forcing between 2004 to 2030. What is "BC in air RF 2004-2030"? This phrasing makes little sense.

> By RF 2004-2030 we mean the radiative forcing from 2004 to 2030. The radiative forcing is defined as a change. Definition IPCC TAR: “the change in net (down minus up) irradiance (solar plus longwave; in W m–2) at the tropopause after allowing for stratospheric temperatures to readjust to radiative equilibrium, but with surface and tropospheric temperatures and state held fixed at the unperturbed values’.” Adding the word “change” in the text would be superfluous. Since we separate the Radiative Forcing (RF) from BC into the contribution from BC in the atmosphere and BC in snow we use the term “BC in air” for BC in the atmosphere.

* Where is the 4th sub-figure in Fig. 5? Be consistent with the other Figures

>The reason it was not there was that it is not discussed in the text. We have now included it to make the Figures consistent.

* Throughout, please check tenses and English usage in general.

> We have gone through all text to improve tenses and language.

Other comments P 26648, Abstract L3. "A set of models" sounds as though an ensemble of CTMs is being used. Rephrase.

>As the referee points out the phrase could be misleading. We now write “A chemical transport model and a radiative forcing model”

L16-17. Do you mean changes in RF? Also, be explicit if you mean global or regional values.

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L18. contracts "with"

>We have now added the word “with”.

L21-22. This sentence is difficult to understand - clarify.

>We have reformulated this sentence. The text should now be more to the point and easier to understand.

P26649, L2-3. These references are getting old, and recent years have seen more and more interesting trends - find a recent reference.

>We have now added the references : Stroeve, J. C., Kattsov ,V., Barrett, A. P., Serreze, M. C., Pavlova, T., Holland, M. M., and Meier,W. N: Trends in Arctic sea ice extent from CMIP5, CMIP3 and observations, Geophys. Res. Lett., 39, doi:10.1029/2012GL052676. 2012.

Stroeve, J. C., M. C. Serreze, M. M. Holland, J. E. Kay, J. Maslanik, and A. P. Barrett. 2012. The Arctic’s rapidly shrinking sea ice cover: a research synthesis. Climatic Change 110(3-4): 1,005-1,027, doi:10.1007/s10584-011-0101-1.

P26651, L24-25. Tenses: Large emission increase*s* *are* found

>Thanks, correction made.

P26653, L7-8. Explain why no changes were made in land-based emissions between 2004 and 2030, this is not obvious to the reader.

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>This is explained later in the paper in the Discussion section where we gathered all the discussion on uncertainties. Though we see the point of explaining this earlier we have kept the text on this in that section to avoid having the same/similar text two places.

L18-21. The issue of comparison to measurements is swept over here. OsloCTM2 is said to be as good as other models, but how good/bad is that for ozone and NO2?

>In this section we refer to former studies where comparisons to measurements were made (Endresen et al., 2003;Dalsoren et al., 2007;Dalsoren et al., 2010) and (Eyring et al., 2007). These studies include comparisons, discussions on plume effects and limited measurements data in ship impacted air outside plumes. It is difficult to summarize the findings for several oceanic areas in a few sentences in this study that focus more on scenarios and less on comparison to measurements. For the interested reader we therefore refer to the listed former studies.

P26655, L27-29. The phrase about critical levels seems odd unrelated to the comment about 3-6% increase. Clarify. Also, I am not sure that Hjellbrekke is the best reference for such a discussion. (Torseth et al., ACP, 2012 is a better reference for the EMEP data anyway, but that too has little to say about critical levels.)

>We agree that this phrase on critical levels was a bit unrelated to the comment about 3-6% increase. We removed the critical levels part of the sentence and the reference.

P26661, L10-11. Forming *the* basis. Also, it is better to say the Dalsoren et al and Corbett et al datasets than just the "two" datasets.

>Corrections made in accordance with the suggestions.

P26662 L11. Changes (or lack of) in sea-ice are mentioned, but many other meteorological factors will change as a result of global warming. This needs to be discussed.

>A short discussion on this is now included in this section.

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L18-24. This section needs some references.

>We have added a reference to the synthesis article by Serreze and Barry on Arctic amplification: Serreze, M. C., and R. G. Barry. 2011. Processes and impacts of Arctic amplification: A research synthesis. Global and Planetary Change 77: 85-96, doi:10.1016/j.gloplacha.2011.03.004.

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