

Authors Reply to referee comments by J. Moldanova.

MS No.: acp-2012-386, Title: Reducing CO₂ from shipping – Do non-CO₂ effects matter?

Comment 1

The BC and OC emissions in relation to the total PM emissions are 2.4 and 8%, respectively in your emission inventory. Especially 8% of OC in PM sounds low. There are only few data published and the variability is quite large so it would be good to spent few words on discussion of assumptions behind using the actual emission factors. What reference have been used and which were the assumptions for emission factors and PM compositions used (type of fuel and engine)?

Reply

With regard to the emission factors used in establishing a baseline inventory for the fleet, we rely on the inventory developed by Dalsøren et al. (2009). The main source for emission factors used by Dalsøren et al. (2009) is Cooper (2002). While it is recognized that there is significant uncertainty in the reported emissions factors, and also that new studies have improved our knowledge about emissions, it has not been the aim of this study to provide updated inventories for the fleet as such, but to rely on an established global ship emission inventory, of which Dalsøren is among the most recent.

The emission factor used by Dalsøren for PM is taken from Whall et al. (2002). For the composition of PM, Dalsøren et al. states that the black carbon (BC) emission is taken from Sinha et al., 2003, while organic carbon (OC) emissions is from Petzold et al., 2004. For the PM mass is emitted as sulfate Petzold et al. (2008) is referenced.

The sparse data for PM composition from the referenced sources are mainly from data recorded for marine diesel engines running on heavy fuel oils. The present study is concerned with the cargo fleet which is dominated by heavy fuel oil, omitting many of the segments using lighter oil. Thus the data from the references are thought to be appropriate for this study.

It is recognized that more recent literature on emission factors is now available, as reviewed e.g. by Lack and Corbett (2012) who assessing the impact of engine load, fuel quality and exhaust after treatment systems for engines used by navigation. Although the work made by Lack and Corbett (2012) contain a lot of detailed information of BC emissions from ship engines, data on total PM emissions are excluded from the literature review. While it is recognized that there are substantial uncertainties in these emission factors and that updated inventories are due, this is beyond the scope of this work.

However, the authors agree that a brief discussion would improve the paper, and will amend the manuscript to address this, including references to primary sources, and highlighting the uncertainty in the emission factors. In the interest of keeping the methodology description for baseline emissions brief, part of the discussion will be placed in section 5 (Discussion and Uncertainty).

New references:

Cooper, D. Representative emission factors for use in “Quantification of emissions from ships associated with ship movements between ports in the European Community” (ENV.C.1/ETU/2001/0090). IVL Swedish Environmental Research Institute Ltd. for ENTEC UK. 2002.

Petzold, A., Feldpausch, P., Fritzsche, L., Minikin, A., Lauer, A., and Bauer, H.: Particle Emissions from Ship Engines, *J. Aerosol Sci.*, 35, 1095–1096, 2004.

Sinha, P., Hobbs, P. V., Yokelson, R. J., Christian, T. J., Kirchstetter, T. W., and Bruintjes, R.: Emissions of trace gases and particles from two ships in the southern Atlantic Ocean, *Atmos. Environ.*, 37(15), 2139–2148, doi:10.1016/S1352-2310(03)00080-3, 2003.

Whall, C., Cooper, D., Archer, K., et al.: Quantification of emissions from ships associated with ship movements between ports in the European Community, Rep. 06177.02121, Entec, Northwich, UK, 2002.

Comment 2

For sulphate part in PM 40% have been used, citing Petzold et al, 2008. What are their assumptions considering type of fuel and engine (HFO and 4-stroke engine, engine load) and what are the assumptions in your emission inventory (eg. are there some assumptions of use of MGO or other fuels in the 2010 fleet?). Primary sulphate (emitted from the chimney) is often relatively small part of the total sulphate generated by shipping, this could be good to stress in the text as there is often confusion about role of primary and secondary sulphate in different communities (air pollution modellers, global modellers).

Reply

Petzold et al. (2008) performed test rig studies, in which detailed aerosol microphysical and chemical properties were measured in the exhaust gas of a serial MAN B&W seven-cylinder four-stroke marine diesel engine under various load conditions between 10% and 100%. The emission studies were complemented by airborne aerosol transformation studies in the plume of a large container ship in the English Channel using the DLR aircraft Falcon 20 E-5. The engine of this container ship was a MAN B&W two stroke marine diesel engine. Both engines were running on Heavy fuel oil.

Dalsøren assumed that all non-cargo vessels and all ships in the lowest size categories (<1000 GT) are using fuel with sulfur content of 0.5%. Dalsøren modeled sulfur oxide emissions to vary with assumed fuel oil sulfur content, while PM composition (incl. primary sulfate) did not vary (Table 2, Dalsøren et al. 2009). For these segments the PM emission are thus particularly uncertain because of the simplified approach used. However, the present study is concerned only with the cargo fleet which is dominated by heavy fuel oil, omitting many of the ships using MDO. Thus the data from Petzold et al. (2008) are thought to be appropriate for this study.

It is recognized that there are substantial uncertainties in the emission factors, also in relation to primary sulfate, and that while updated inventories are due; this is beyond the scope of this work. The authors agree that a brief discussion would improve the paper, including references to primary sources. In the interest of keeping the methodology description for baseline emissions brief, part of the discussion will be placed in section 5 (Discussion and Uncertainty).

The authors agree that the distinction between primary and secondary sulfate is of particular relevance, and will include a comment to address this in the revised manuscript.

Comment 3

p.8, l. 25-27: (for SECAs) SOX, PM and OC emissions are not distributed uniformly : : : on p. 12, l. 34-37: It is noted that the change in sulfur emissions will likely influence on emissions of OC and BC (e.g. Lack et al. 2011; Lack & Corbett, 2012). While this may substantially impact on BC

and OC emissions, their relative importance to sulfur is minor (see Figure 6a) and is disregarded in the following. These 2 statements are in disagreement to me – SOX is obviously reduced in SECAs but how do you treat PM and its components SO₄, BC and OC? Please, harmonize these 2 paragraphs and explain how do you treat PM and its components.

Reply

Regarding an explanation of the treatment of SOX and PM in ECAs; As stated in section 2.1 we “adjust for the effect of Emission Control Areas (ECAs)” in the baseline inventory. The details of this adjustment have been omitted for brevity, considering also that the impact of this adjustment is minor on a global level. The adjustment in ECA for SOx emissions builds on the assessment by Buhaug et al. (2009), who find that the existing ECAs have contributed to a 3.4% reduction in global SOx emissions, which corresponds to a 42% reduction in ECA emissions (Buhaug et al., 2009, their table 4-8). Moreover, we have applied a reduction factor for PM and its components scaled to ¼ of the SOx reduction factor, based on the findings of ENTEC (2005, 2007). The exception is BC emissions which are reported by Corbett et al. (2010) to be not well correlated with fuel sulfur content.

A statement reflecting this will be added to the revised manuscript to increase transparency.

Regarding the disagreement between the two statements, the authors believe that the two statements are consistent in that they both recognize the impact of sulfur regulations on other emission components. However, the second statement points to the fact that these secondary effects are disregarded, reflecting that “A simplified approach is employed” to assess the low-sulfur inventory for 2020 in terms of climate impacts (section 4.2.3). The use of such a simplified approach is found to be justified considering the results shown in Fig 6a.

New Reference:

Corbett, J. J., Winebrake, J.J., Green E.H., An assessment of technologies for reducing regional short-lived climate forcers emitted by ships with implications for Arctic shipping, Carbon Management 1(2), 207–225, 2010b.

ENTEC. Entec UK Limited, Service Contract on Ship Emissions: Assignment, Abatement and Market-based Instruments, Task 2c - SO₂ Abatement. 2005, European Commission Directorate General Environment, 2005.

Comment 4

p. 13, l. 27-31: Replicating the analysis for a scenario in which no low sulfur mandate is enforced, we find that the warming in 2050 is twice that in the low-sulfur case, and that cooling is delayed until 2090 – I might be confused here but you show that on the short term the RF from the reduction scenario is dominated by strong warming from sulphur reduction (less sulphur = warming, Figure 6). But here my understanding is that no sulphur regulation causes warming in 2050 doubled (more sulphur = warming). Am I missing something here or is there 1 ‘no’ too much?

Reply

The authors recognize that this paragraph can be misunderstood.

The key point is that the warming effect under discussion is the result of removing sulfur as a consequence of CO₂ abatement, and that the magnitude of sulfur removal relates directly to the baseline emissions. Thus the volume of sulfur removed (and thus the warming) in a scenario without sulfur regulation is larger than in a scenario with sulfur regulation.

A brief statement will be added to the revised manuscript to clarify.

Suggestion 1

Abstract – l. 7-8 'In the reduction inventories CO₂ emissions are reduced : : : Is there a possibility to describe the studied mitigation strategies in few words? This would make the abstract more concrete. The same applies for the rest of the manuscript, one need to go to Eide et al. (2011) to find out what mitigations are considered.

Reply

The authors agree that more specific mention of the mitigation options considered will benefit the paper, and the necessary modifications to the manuscript will be performed, including adding a summary table of all the measures modeled by Eide et al. (2011).

Suggestion 2

p. 3 l. 13: This harms ecosystem growths – maybe biodiversity or just ecosystems?

Reply

Agreed.

Suggestion 3

p.3 l. 15: asymmetric growth in nitrogen poor regions – maybe asymmetric plant competition?

Reply

Agreed.

Suggestion 4

p.4 l.2: IMO regulations are on SO_x and NO_x. The PM is regulated only indirectly through secondary PM formation. This statement is often used and quite confusing.

Reply

Agreed. Will remove reference to PM.

Suggestion 5

p. 4 l. 15-17: What year are the reductions at USD 50 related to, 2030?

Reply

The reductions relate to 2010. This will be clarified.

Suggestion 6

p.4, l.34 – 39: some references? e.g. Lack et al., 2012.

Reply

A reference to Lack et al. 2012 will be added.