Atmos. Chem. Phys. Discuss., 13, C7941–C7945, 2013
www.atmos-chem-phys-discuss.net/13/C7941/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate and air quality trade-offs in altering ship fuel sulfur content" *by* A.-I. Partanen et al.

Anonymous Referee #2

Received and published: 12 October 2013

Decision and general comments:

I recommend to accept the paper upon minor revisions. The paper shows that we can save human lives due to lower PM2.5 and achieve climate cooling when applying strict limits for ship sulfur content close to coastal areas, at the same time as we allow for higher sulfur content over open oceans. Hence, the paper is a very important input to policy making and geoengineering. Even with the limits planned for 2020, the authors show that there is still a small cooling potential of the 0.5 % ship sulfur content, at the same time as the 0.1 % sulfur content close to coastal areas, would eradicate almost completely the human mortality. The paper is concise, to the point and very well written.

Specific comments:

Abstract

Not all people know what the present day situation is. Although the present day situation is described in the introduction, I would suggest to include an explanation of the present day situation of sulfur control also in the abstract, so that the reader is able to compare with the scenario (1) and (2).

Introduction

Is it possible to explain more clearly your aim in the last paragraph? As it is now, I feel it is a bit too complicated for me.

Methods

"The aerosol model resolves nucleation of new particles (Kazil and Lovejoy, 2007)"

Could you add to the text whether the nucleation rate in the planetary boundary layer of this model is dependent on sulfuric acid multiplied with organic carbon concentrations, or if it is only dependent on sulfuric acid? This makes a huge difference to nucleation rates over the oceans, since ships emit mostly SO2 giving sulfuric acid, but not much organics. This means that with a nucleation rate dependent on both sulfuric acid and organics, we will get very low formation rates over the open oceans even at a heavily trafficked ship lane. On the other hand, if only sulfuric acid is included in the expression for the nucleation rate, then we will have also nucleation along ship routes over the open ocean (Merikanto et al., 2009). Would it be possible to comment on this fact in your paper – in other words whether you have a positive bias of the magnitude of nucleation due to ship traffic (with sulfuric acid only - scheme), or if you have a negative bias of the magnitude of nucleation (sulfuric acid + organics)?

"The combination of the model version and the cloud activation parameterization is unpublished and may differ from the official model version to be released with respect to e.g. tuning parameters."

I am not sure what you mean here. Would it be possible to clarify?

“We implemented the model modifications done by Peters et al. (2012) to set all shipping emissions consistently in the first model layer assigning primary sulfate, organic carbon and black carbon emissions from shipping to the soluble Aitken mode with geometric mean radius of 30 nm.”

Two points from my side: 1. What do you think about motivating your choice of emissions centered at 30 nm diameter with the papers by Jonsson et al. (2011) and Petzold et al. (2008)? 2. By examining the references Lieke et al. (2013), Popovicheva et al. (2012), and Xie et al. (2007) the size segregation emissions for the different group of compounds might be different than what you have suggested here. I recognize the difficulty of knowing exactly in what size ranges different chemical species groups are emitted, and that number concentration emissions are dominated at around 30 nm diameter. Hence, I accept the choice of ship emissions in your paper. Please consider though to discuss how the choice of size distribution emission can affect the results. Peters et al. (2012) have discussed some of the problems with the size distribution.

“Lacking a precise formulation, we used the original black carbon emissions for all simulations.”

Which is the original study that you are referring to here?

“Due to the model version used, our analysis includes only sulfur, organic carbon, and black carbon aerosol emissions from shipping. Other main aerosol and aerosol precursor compounds in shipping emissions include nitrogen oxides and volatile organic compounds (Eyring et al., 2010), but we expect them to have only minor effects on aerosol-induced premature mortality and radiative forcing."

I understand that it is hard to know the chemical composition of the ship emissions, since these are poorly characterized, and that the other compounds influence to a minor extent to the total mass emissions. However, wouldn’t it be little safer not to make a conclusion that the chemical composition of the other minor compounds has a minor effect on health and radiative forcing? For example, Lieke et al. (2013) show

that the particles emitted below 100 nm diameter at a test ship engine contain for example CaSO4, or other crystalline salts. Even if these compounds do not comprise a significant fraction of the total mass (which is dominated by particles larger than 100 nm diameter), it might still comprise a significant mass fraction of the particles below 100 nm diameter. These particles are possibly influencing cloud activation to a large extent, and hereby the radiative forcing. Please, would you mind considering to change the conclusive remark?

Results

Section 3.1. I’m a little surprised by the very large difference between ships-2020 and geo-wide for PM2.5 concentrations over continents, which means that the emissions far away from the coast-line (the difference between geo-wide and ship-2020) can still affect PM2.5 over continents to a significant extent. Ok, I realize that SO2 can be oxidized to sulfuric acid long time after the first ship emissions, and that’s why ship emissions far away from the coast can influence continental levels. But, I didn’t realize that this could give as high as 0.5 ug/m3 PM2.5 over continents. Maybe you could add one sentence stating that although coast-line emissions are not allowed in geo-wide, the emissions further away from the coast-line still are able to contribute significantly to the continental PM2.5?

Section 3.4.1. “We estimated the resulting error by assuming that the ratio between the modeled and bias-corrected PM2.5 values follows a linear fit between modeled and measured PM2.5 concentrations (Fig. 5, red lines).”

Gets complicated. Seems difficult, but is there a way to explain it more clearly?

Discussion

Section 4.1. Maybe the words “prefer” and “pareto” could be changed to something else?

“If the sensitivity runs are excluded, the other simulations cannot be put into a preferred

order without deciding some conversion method between RFP and mortality rate. For example, geo-narrow offered a stronger cooling ($-0.53 Wm^{-2}$ vs. $-0.43 Wm^{-2}$) than geo-wide but had also a greater annual mortality rate (34 900 yr^{-1} vs. 15 400 yr^{-1})."

I don't fully understand. Would it be possible to clarify?

References mentioned by referee Jonsson, Å., et al., 2011. Geoph. Res. Lett., 38, L13809, doi:10.1029/2011GL047672. Lieke, K. I., et al., 2013. Atmos. Science. Technol., 47:9, 1038-1046. Peters et al., 2012. In your paper. Petzold, A., et al., 2008. Atmos. Chem. Phys., 8, 2387-2403. Popovicheva, O., et al., 2013. J. Environ. Monit., 14, 3101. Xie, Z., et al., 2007. J. Geoph. Res., 112, doi:10.1029/2006JD007247.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21989, 2013.