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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol indirect effects from shipping emissions: sensitivity studies with the global aerosol-climate model ECHAM-HAM" *by* K. Peters et al.

J. Pierce (Referee)

jeffrey.robert.pierce@gmail.com

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Review of "Aerosol indirect effects from shipping emissions: sensitivity studies with the global aerosol-climate model ECHAM-HAM" by Peters et al.

This paper explores the effect of ship emissions on aerosol microphysics and the climate effects of these aerosols. Experiments were performed to test the sensitivity of the simulation results to uncertain inputs such as the total mass emission rates, particle size, hygroscopicity and composition. The simulated radiative forcings depend greatly on the assumed inputs and highlight the need for continued research on ship emissions.

The paper is generally well written and well within the scope of ACP. I recommend this paper be published once the following comments have been addressed.

General comments

- There may be considerable uncertainty in the AIE forcings due to the prediction of clouds in ECHAM-HAM and the predicted susceptibility of cloud changes to aerosol changes. Two models with identical aerosol emissions may have very different AIE predictions due to ship emissions. This should be discussed.

- Observable ship tracks generally have widths that are smaller than the grid resolution used here. Given that the shift between open-celled and closed-celled straticumulus clouds may be non-linear with aerosol concentration, there may be uncertainties in the predicted AIE due to these plume-cloud effects. This should be discussed.

- The subgrid nucleation and growth in sulfur-rich plumes (relevant to the number and size of the sulfuric acid particles from ship emissions) depends greatly on the atmospheric conditions. Obviously day/night, sunny/cloudy conditions will affect the nucleation and growth. Additionally, if the background air already contains a lot of aerosols, nucleation/growth will be strongly suppressed as nucleation will be slower, growth will be slower and coagulation will be faster. Thus, more particles should be formed in cleaner environments and fewer in polluted. This is explored in detail for coal-fired power plants in "Stevens, R. G., Pierce, J. R., Brock, C. A., Reed, M. K., Crawford, J. H., Holloway, J. S., Ryerson, T. B., Huey, L. G., and Nowak, J. B.: Nucleation and growth of sulfate aerosol in coal-fired power plant plumes: sensitivity to background aerosol and meteorology, Atmos. Chem. Phys., 12, 189-206, doi:10.5194/acp-12-189-2012, 2012", but the results should be general for ship emissions too. This dependence of sub-grid nucleation/growth on background aerosol could affect the size distribution and forcing predictions, and this should be discussed when presenting the assumed emission size distributions of sulfate.

- What is the definition of statistical significance here throughout? Is this that there is

a <10% of a chance that the difference between the two simulations (averaged over 5 years) could have happened randomly? Perhaps this was specifically stated and I missed it, but if not, please add.

- How does nudging affect cloud susceptibility? I could imagine that nudging could prevent clouds from fully responding to aerosols. Has this been tested before? Please comment on this.

Specific comments

P7077 L27-28: Please give a quick description or estimation of why reduced AIE's occurred despite increasing fuel consumption? Is this because of improved emissions controls? P7078 L11-12: You suggest combustion of cleaner ship fuel leads to reduced emissions of BC, reducing potential cooling effect – but it would also reducing BC warming as stated on previous page. Correct?

P7079 L17: In Kazil et al. (2011), they used activation nucleation in the continental boundary layer (not in the marine boundary layer), and the ion-induced nucleation every else, correct? This is an important detail and should be mentioned.

P7081 L7: COADS should be Comprehensive Ocean-Atmosphere Data Set.

P7081 L23: Do emissions inventories include days at port? Are these emissions substantial? If so, how does the model handle this emission input and uncertainty?

Section 2.4.: Please discuss the NS experiment. I deduced from the Table that this experiment is the same as the other experiments in every way except that all ship emissions were turned off; however, this never mentioned in the text. The first use of "NS" in the text comes later, but not in reference to the simulation, but to the soluble nucleation mode. The NS simulation is eventually mentioned later, but not defined. The two uses of "NS" is a bit confusing.

Section 2.4: I think that the experiments testing the reduction of BC/OC are very interesting as they are relevant to the recent discussion of climate controls through soot

reduction. However, many regions of the world are beginning to require ships to use fuels with lowered sulfur content. It would be interesting to test this effect.

Section 2.4 and throughout: I find the use of "emissions parameterization" a bit uninformative. It would be more informative to say emissions size and composition (a bit tricky since it also includes the fraction of SO2 oxidized on the sub-grid scale).

Section 2.4: Related to the previous comment, it would be useful to know how much of the differences between the A and B experiments is due to the difference in size of sub-grid sulfate, the fraction of SO2 oxidized in the sub-grid scale or the hygroscopicity of the emissions. If short simulations could be ran to give global-average BL estimates of what each of these factors contribute, this would be very helpful in determining the most important parameters to measure/improve.

P7083 L6: I never understood how new coarse-mode aerosol could EVER be formed through sub-grid nucleation and growth. There really is no justification for this. The use of this aspect of the AERCOM inventory really needs to stop. I'm glad that the authors are using something different for the base-case scenario. This should be extended for all sub-grid sulfur sources too in the future.

P7083 L11-16: The fraction of SO2 oxidized to H2SO4 should depend greatly on the time from the source where the measurement was taken (not to mention the amount of sunlight and NOx and VOC concentrations that determine the OH concentration that determine how quickly SO2 is oxidized). How does the time since emission (and other factors) differ between estimates, and how does it differ from these factors in the model? Also, its not obvious to me why high-sulfur vs. low-sulfur fuels should have a different fraction oxidized as SO2 concentration does not greatly affect OH concentration. Perhaps NOx or VOC concentrations also change between the fuel types? Or perhaps there were not enough measurements to be confident in the differences between low and high SO2 fuels. (This final issue with high-sulfur vs. low-sulfur fuel is somewhat tangential to your paper, but it might be worth mentioning if you have any

insight.).

P7085 L24: Aqueous oxidation is generally estimated to be somewhat more important for taking SO2 to sulfate globally, and this would depend on the cloud cover amount as well as H2O2 and O3 concentrations (though these concentrations probably correlate well with OH).

P7087 L3: The second explanation in this sentence is incorrect. The additional particles emitted to KS would increase the condensation sink and would lower [H2SO4] for a constant H2SO4 production rate. Lower [H2SO4] would LOWER the growth rate of the Aitken-mode particles to accumulation-mode sizes, not make them faster. The likely reason for the faster growth of Aitken-mode particles to accumulation-mode sizes is the additional SO2 emitted by the ships that would increase [H2SO4].

P7088 L2: Increasing the size of particles from the Aitken-mode to the accumulation mode would decrease dry-deposition rates, not increase them (dry deposition rates have a minima in the accumulation mode). However, the wet-deposition would increase due to more particles being large enough to act as CCN, and this effect likely dominates over the lowering of the dry-deposition rates.

P7088 L7 and the caption of figure 5: AOD does not have a fine mode. You could say "total AOD and the fine-mode AOD" or "AOD and its fine-mode component", but "the AOD and its fine mode" seems awkward to me.

P7089 L10: Why is there so much noise in the cloud property changes even when averaged over 5 years? The magnitude of the noise is very large.

P7091 L8: I would have guessed that a cloud thickening would cause a reduction of outgoing longwave radiation. Higher cloud-top heights have colder temperatures and would emit less longwave radiation.

P7092 L1-6 and Fig 9: species column burden differences between experiment A & B in the specified hemispheres are difficult (and at times not able) to discern.

Section 4.1: Please explore/discuss the contribution to the differences between A and B from sulfate aerosol size, solubility and the fraction of SO2 oxidized on the sub-grid scale.

P7094 L3: inrease = increase

P7097 L28-: SO2 mass emissions from ships are nearly 2 orders of magnitude larger than OM and BC emissions. This means that the ship OM and BC emissions are located in regions where the potential for nucleation is elevated (at least compared to if the SO2 emissions were missing). This makes the OM and BC from ships somewhat unique compared to many other sources of OM and BC where the SO2 emissions are smaller.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7073, 2012.