Reply to the review of "Aerosol indirect effects from shipping emissions: Sensitivity studies with the global aerosol-climate model ECHAM-HAM" (acp-2012-139) by Reviewer #2

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GENERAL COMMENTS

This paper presents a global model study of the ship-induced direct and indirect aerosol effect and an estimate of the resulting radiative forcing. Several uncertainties concerning the emissions of aerosol from shipping are investigated using sensitivity simulations, although an important source of uncertainty, namely the geographical distribution of emissions, is not considered.

Given the still very limited amount of model studies on this topic available in the literature, this work represents an important contribution to improve the quantification of aerosol effects from ship traffic. The scientific method and the underlying assumptions are presented in sufficient detail and the relevant literature is cited and discussed.

Nevertheless, a more precise comparison with previous modeling studies is necessary. The manuscript is well structured and clearly written. The quality of the figures can be improved. This concerns their size (panels are often too small), the labeling of the color-bar and the highlighting of the regions of statistical significance (see detailed comments below).

I recommend this paper for publication in ACP, after a major revision addressing the following issues. We thank the reviewer for his/her supportive opinion regarding our work and we shall assess the remarks/concerns in the following.

MAJOR REMARKS

Model vs. observational estimates of RF: in a previous paper, the same author team performed an observational analysis of the large-scale impacts of ship emissions on clouds, as mentioned in the Introduction (Peters et al. 2011b), and found no significant effect. The current manuscript is now focusing on the same problem using a modeling technique and finds a significant effect, in reasonable agreement with previous modeling studies. Such discrepancy between model and observations is very interesting and the authors should elaborate more on the possible reasons for it. I would discuss this in a separate section.

Thank you for this indeed very intriguing remark. In fact, a more detailed comparison of observations vs. modelling results is planned in the future. Nevertheless, we follow the reviewer's advice and add some discussion on this discrepancy in the present manuscript as well as by adding the following: in our satellite study (Peters et al., 2011b), we focused solely on tropical regions due to limitations of the sampling strategy in mid-latitudes (please see that paper for a more detailed argumentation). In this study, statistically significant changes of cloud properties resulting from shipping emissions are mostly constrained to mid-latitudes, *i.e.* the results for the tropics are very noisy and lack statistical significance. Therefore, the results shown in this study are in line with the findings of our previous work. This will be further investigated in the future and preliminary results can be found in Peters (2011).

We have modified a corresponding passage in the manuscript:

"The changes over tropical oceans are rather noisy because here, the relatively large variations in macrophysical cloud properties, such as cloud liquid water path/-geometrical thickness, dominate the signal even for the five year averages considered here. This is in-line with the findings of Peters et al. (2011b) and a more thorough comparison to observations will be performed in the future. "

Model evaluation: model evaluation is not mentioned at all. Please provide at least one reference for that and summarize the main strengths/weaknesses of the model. A key issue is the representation of (low) clouds in the global model, as already noted by the other reviewer. This is a major source of uncertainty for this kind of studies. I suggest to include a comparison with observational data for clouds (e.g. ISCCP).

Thank you very much for this important remark. We choose to present the evaluation of the ECHAM5-HAM model in two main areas:

- 1. Performance of ECHAM5 with respect to clouds
- 2. Performance of the aerosol sub-model HAM

Cloud cover

For evaluation of the models' simulated cloud fields, it appears useful to compare them to observations, e.g. satellite data. This cannot be performed straight forward, but requires the application of so-called "satellitesimulators" to the model output to ensure direct comparability of modelled and observed cloud fields (e.g. Bodas-Salcedo et al., 2011). For the case of the ECHAM5 model, this has just recently been performed in the studies of Nam and Quaas (2012, in press) and Gehlot and Quaas (2012, in press), where Nam and Quaas (2012, in press) compared the ECHAM5-simulated cloud fields to CALIPSO and CloudSat observations whereas Gehlot and Quaas (2012, in press) used ISCCP observations for the same purpose. In both studies, the convection parameterisation is based on Tiedtke (1989) with modifications for penetrative deep-convection according to Nordeng (1994), which is the same setup as for our simulations. However, in the simulations of Nam and Quaas (2012, in press) and Gehlot and Quaas (2012, in press), cloud cover is calculated by employing the scheme of Tompkins (2002) whereas we employed the cloud cover scheme of Sundqvist et al. (1989). Our experience shows that the two cloud schemes produce quite similar cloud distributions in a present-day climate. We are thus confident that the results presented in Nam and Quaas (2012, in press) and Gehlot and Quaas (2012, in press) are also applicable to our simulations.

Both of those studies found that ECHAM5 overestimates high-cloud cover and underestimates mid- and low-level cloud cover. The overestimation of high cloud cover is attributed to the convective scheme transporting too much water into the upper troposphere by not allowing for enough detrainment at lower levels. This then leads to too many, too thick and too high clouds, especially in the tropics where convection is intense, at the expense of midand low-level clouds. If simulated, often a too low coverage by low-level clouds is simulated (Nam and Quaas, 2012, in press). From common sense, one expects low-level liquid water clouds to be most susceptible to shipping emissions. The underestimation of such clouds in ECHAM5 may in fact explain the noisiness of the AIE signal in the tropics. For the mid-latitudes, i.e. where the bulk of shipping emissions occurs, low-level clouds seem to be represented reasonably well.

Overall, this deficiency of the ECHAM5 model in correctly representing lowlevel liquid water clouds may lead to an underestimation of calculated AIEs. We have added some discussion of this into the manuscript (the "model description" section.):

"Gehlot and Quaas (2012, in press) and Nam and Quaas (2012, in press), using satellite observations, evaluated the ECHAM-simulated cloud cover using the same convection parameterisation but a different cloud cover scheme, *i.e.* that of Tompkins (2002). Both studies revealed that this model configuration overestimates high-cloud cover at the expense of mid- and lowlevel cloud cover, especially in the tropics and subtropics. As the ECHAMsimulated cloud fields are similar for both cloud-cover schemes (Quaas, 2012), these findings also hold for the model configuration we use in this study. As shipping emissions are most probably bound to impact the properties of lowlevel clouds, the AIEs obtained with this model may represent a low estimate, especially for tropical and subtropical regions."

Aerosol submodel HAM

The version of HAM used for the model simulations presented in this study is in fact an updated version of the original HAM aerosol submodel (Stier et al., 2005). The version used in this study is presented and evaluated against observations in Zhang et al. (2012), a study currently under review for ACP. The authors show, compared to the previous version of HAM, that the model performs better in terms of the simulated aerosol size distribution as well as spatial-temporal variance of aerosol properties and that biases in AOD and the Angstrom parameter are reduced. Model deficiencies to be investigated in the future include a positive bias of AOD over the storm tracks, a negative bias of AOD and aerosol mass concentration in high-latitude regions, and a negative bias of particle number concentration, especially that of the Aitken mode, in the lower troposphere over the heavily polluted regions. We have added additional information to the revised manuscript:

"The version of HAM used in this study is based on the original model presented in Stier et al. (2005) with several new developments. In their recent work, Zhang et al. (2012) present the changes applied to the original model version and evaluate these changes with respect to observations. Here, we will not delve into the details of this model evaluation, but it should be stated that the model-simulated aerosol size distribution and spatio-temporal variance of the aerosol population have improved compared to observations. Remaining deficiencies include positive and negative AOD biases over storm-tracks and high-latitudes, respectively, as well as a negative bias in particle number concentrations in the lower troposphere over polluted areas (Zhang et al., 2012). "

We have also inserted the following note to the very end of the revised manuscript:

"As there exists a considerable inter-model spread regarding total AIEs (e.g. Penner et al., 2006; Quaas et al., 2009), it must be noted that the estimated range of AIEs stemming from shipping emissions crucially depends on the employed GCM model setup, encompassing the used convection, cloud cover and aerosol- and cloud-microphysical parameterisations. Future effort should therefore focus on performing model intercomparison studies of not just total aerosol indirect effects, but also of aerosol indirect effects attributable to a certain economical sector, e.g. shipping emissions."

Comparison to previous work: throughout the manuscript, the authors often refer to the assumptions and results of Lauer et al. (2007), who performed a similar work. The two studies actually use a similar model and basically the same methodology. Therefore similarities and differences should be analyzed more systematically and related to resulting estimate of the AIE. In particular: 1) What could be the impact (if any) of the different horizontal resolution (T42 vs. T63) adopted in the two studies? 2) What are the main differences in the emission inventories, in terms of total emissions, size distribution and geographical distribution? 3) What are the impacts of the different cloud schemes and aerosol models? For example: according to the model description, aerosol nitrate is not considered in HAM, while it seems to be included in Lauer et al. 4) Which chemical mechanism is used in the model? What are the most important reactions and reaction cycles included? How do this compare with Lauer et al.?

Thank you for this important comment. Here, we can by no means perform a full systematic evaluation of the differences between the two studies because Lauer et al. (2007) do not present any thorough analysis of the underlying processes which lead to the large AIEs in their study. In particular, they do not present analysis of particle number concentrations per aerosol mode and CCN diagnostics. This also holds for the study of Righi et al. (2011), wo used the exact same model setup as Lauer et al. (2007). In our study, we present these model diagnostics related to the effect of shipping emissions for the first time and assessing differences to earlier studies would indeed be more feasible if these parameters were also available for earlier studies (in the comparable detail). Righi et al. (2011) however do present some analysis of changes in aerosol number concentrations in the supplementary material. Those results are in line with our findings. It should also be noted that our results compare much better with the results of Righi et al. (2011) compared to those of Lauer et al. (2007).

In the following, we go into detail on the reviewer's enumerated remarks.

- 1. This would indeed be interesting to investigate. However, we choose here not to perform any sensitivity experiments using a coarser horizontal resolution because we do not have a carefully tuned model version at hand other than the one we used in our study. Setting up well tuned but "identical" simulations is thus beyond the scope of this study.
- 2. Lauer et al. (2007) present simulations employing three different emission datasets (inventories A, B and C). In the following, we present some detail on the total emissions (as the reviewer already mentioned, we do not use emissions of nitrate and CO) and geographical distributions and tentatively compare them with our setup:
 - A This inventory is described in Eyring et al. (2005). Compared to our inventory, total emissions are higher (SO₂: 47%, BC: 67%, POM: 21%) and geographical distribution is performed using AMVER data, thus representing a realistic distribution which is in principle comparable to that of the inventory used in this study.
 - B This inventory is the standard AeroCom setup as presented in Dentener et al. (2006) and Olivier et al. (2005). Total emissions of SO₂ are approximately the same as in our study (2.4% difference), but the fractionation of BC vs. OC emissions is reversed, *i.e.* total BC emissions are higher than total OC emissions: BC: 430% higher, OC: 60% lower. The emissions are distributed just along the main global shipping routes, thus being an unrealistic representation of actual ship movements (see our manuscript for further discussion). This is also a reason why Lauer et al. (2007) conclude that the geographical distribution of shipping emissions has a substantial effect on the retrieved AIEs.
 - C This inventory is described in Wang et al. (2007). It provides monthly mean emission sums and also monthly geographical distribution of emissions. Global total annual emission sums are

higher than for the inventory used in our study: SO₂: 16%, BC: 230% and OC: 470%. For this scenario, the monthly mean geographical distribution of emissions is derived from COADS data.

Here, we will only discuss the characteristics of emission inventories A and C in Lauer et al. (2007) as these most closely represent actual shipping movements. Both of these inventories yield higher total emissions compared to the inventory we use and the spatial distribution is quite similar. As noted in our manuscript, the uncertainty related to the spatial representation of emissions on an annual compared to a monthly basis is most probably substantially smaller than the uncertainty associated with the emissions themselves (A. Lauer, pers. comm. 2011).

Furthermore, the work presented in Lauer et al. (2007) does not allow for a thorough investigation of the effect of emission parameterisation (including sub-grid scale instantaneous formation of sulfate), emission amount and geographical distribution. As all these factors change from inventory to inventory, the model's response is a convolution of all these factors. Singular effects cannot be isolated in the way that we have performed in our study. A more detailed comparison is possible when considering the results of Righi et al. (2011). Indeed, their results on the impact of the chose emitted particle size distribution are very well in line with our findings. We have added the reference to Righi et al. (2011) at various points of the manuscript.

For all three emission inventories, Lauer et al. (2007) assign particulate emissions to the soluble Aitken mode of the aerosol submodel. This is what we do for the experiments using the modified emission parameterisation, thereby yielding a comparable emission parameterisation. This is already mentioned in the submitted manscript.

In the experiments in which we scaled the emissions by the factor 1.63 (Asc and Bsc), total emissions are slightly higher than the ones used from inventory A in Lauer et al. (2007) (about 11% for SO_2). We have added the following short note to the manuscript:

"In the study of Lauer et al. (2007), the highest annual total emission of SO_2 was 11.7 Tg, and Righi et al. (2011) used maximum annual SO_2 emissions of 14 Tg. This has to be kept in mind when interpreting the results."

3. In the scope of this study, we cannot investigate the effect a different cloud microphysical scheme would have on the simulated results. In Lauer et al. (2007), aerosol activation is calculated following Abdul-

Razzak and Ghan (2000), whereas we employ the method following Lin and Leaitch (1997).

According to the information given in their paper, Lauer et al. (2007) used the same cloud microphysical parameterisation setup as we did in our study (Lohmann et al., 2007; Khairoutdinov and Kogan, 2000).

The main difference between the two studies (Lauer et al. (2007) and ours) is the use of a different aerosol-submodel: MADE (Ackermann et al., 1998) vs. HAM (Stier et al., 2005; Zhang et al., 2012). It is not the purpose of this paper to perform a detailed model intercomparison study. Please see previous AEROCOM phase 1 studies to which results from both models were submitted for a detailed comparison (Textor et al., 2006; Kinne et al., 2006). Upcoming AEROCOM phase 2 studies will also provide an overview. In short, several differences between the two models are apparent:

- In MADE, the aerosol population is represented by three overlapping log-normal modes whereas HAM uses seven overlapping log-normal modes (including a separate mode for newly formed particles, the nucleation mode).

MADE's modal structure does not allow for explicit treatment of soluble and insoluble aerosol and assumes a perfect internal mixture of all compounds. HAM considers insoluble and soluble modes with mixing allowed between the modes.

- regarding aerosol species, MADE considers sulfate, nitrate, ammonium, dust, sea salt, black carbon and particulate organic matter. Of those, HAM covers all but nitrate and ammonium. As nitrate aerosol may be important in terms of anthropogenic climate forcing (Andreae and Rosenfeld, 2008, and references therein), neglecting nitrate aerosol in HAM may be one of the factors contributing to the differences between the two studies.
- 4. In our simulations, we do not run ECHAM-HAM with full chemistry. In HAM, chemistry is based on the sulfur cycle as presented in Feichter et al. (1996). Three-dimensional monthly mean oxidant fields (OH, H₂O₂, NO₂ and O₃) are prescribed from runs with the chemical transport model (CTM) MOZART (Horowitz et al., 2003). In their study, Lauer et al. (2007) used online-calculated tropospheric chemistry as calculated by the chemistry module MECCA.

Regarding the last two points, we have added the following information to the discussion section of the revised manuscript:

"It is beyond the scope of this study to delve into the exact differences between the two model versions, but it should be noted that the two employed aerosol sub-models differ in terms of their representation of the aerosol size distribution, treatment of aerosol species and representation of tropospheric chemistry (see AEROCOM phase 1 publications for more detail, *e.g.* Textor et al., 2006). In addition Lauer et al. (2007) calculated aerosol activation following Abdul-Razzak and Ghan (2000) whereas this was treated following Lin and Leaitch (1997) in our simulations."

Geographical distribution of the emissions: the uncertainty in the geographical distribution of emissions is not assessed, although this could have a potentially large impact on the resulting estimate of the AIE. I suggest to consider an additional sensitivity study, where the QUANTIFY inventory (based on ICOADSAMVER) should be replaced by, for example, the inventory of Lamarque et al. (ACP, 2010).

Thank you very much for this comment. However, we refrain here from performing an additional sensitivity study using a different geographical distribution of shipping emissions (*i.e.* Lamarque et al., 2010). In the following, we go into detail on our scientific reasoning which is mainly based on the findings of Lauer et al. (2007) (L07) and Righi et al. (2011) (R11):

- According to their paper, R11 use the exact same model environment as L07. However, they used a different set of emission inventories to investigate the effect of utilising biofuels in shipping. For those inventories, the geographical distribution of the emissions was very different from that used in L07 and total fuel consumption was also higher because R11 considered annual emission totals for the year 2006 (year 2000 in L07).

For their REF simulation, *i.e.* a simulation using an inventory representative of current fuel sulphur content regulations, R11 obtained a substantially lower ($\approx 30\%$ less) AIE estimate than L07 did for their inventory "A" (the inventory which's emission match those of REF in R11 most closely). This is surprising, as the emissions of SO₂ are $\approx 17\%$ higher in R11's REF compared to L07's "A" scenario. Investigating this relatively large difference of AIEs, R11 argue that it is most probably attributable to the different geographical distribution of the emissions compared to L07. Indeed, compared to scenario "A" of L07, the emission inventory used in R11 shows less emissions in highly frequented shipping corridors and more as well as wider distributed emissions in lesser frequented shipping corridors. R11 then argue that these differences may in fact yield the 30% difference in AIE compared to L07, despite a substantial increase of SO₂ emissions.

So for the case of comparing the results of R11 and L07, we acknowledge that the geographical distribution of shipping emissions may very well play a role in determining shipping induced AIEs. However, when comparing the annual mean emission distributions used in our study (see Fig. 1 of the revised manuscript) to that given for inventory "C" in L07, *i.e.* that of Wang et al. (2007) and thus being the same as used for Lamarque et al. (2010), differences between the two spatial distributions are by no means as striking as those obtained when comparing the spatial emission distribution of R11 and L07("A").

Therefore, we conclude that the difference in derived AIEs obtained from using the geographical distribution of Lamarque et al. (2010), when using the same annual total emissions as for our inventory (Behrens, 2006), would not be substantial compared to our current results.

- In their work, L07 argue that the geographical distribution of shipping emissions plays a major role in determining the AIE. However, this conclusion only holds for comparing the results of their "A" and "C" (geographical distribution according to AMVER and ICAODS, respectively) experiments to those obtained from "B" (AeroCom distribution). As the AeroCom inventory only considers emissions of SO₂ along main shipping routes, the conclusion drawn in L07 is by no means surprising.

A slightly more quantitive analysis (Tab. 1) reveals that the AIEs obtained from L07's experiment "C" almost exactly scale with the total emissions of SO₂ compared to "A"; the small difference in AIE-ratio may be most plausibly explained by the difference in emission of primary sulfate (higher ratio in "A" compared to "C"). The AIEs of L07's experiment "B" by no means scale with total SO₂-emissions compared to "A".

These findings very well justify the conclusion that the obtained AIEs depend on the geographical distribution of shipping emissions when comparing "A" and "C" to "B", but NOT when comparing "A" to "C".

Inventory in L07	AIE	$SO_2 [Tg yr^{-1}]$	SO_2 -ratio	AIE-ratio
А	-0.60	11.7	1	1
В	-0.19	7.6	0.65	0.32
\mathbf{C}	-0.44	9.2	0.79	0.73

Table 1: Results of Lauer et al. (2007). The AIE is given as annual means in Wm^{-2} , the SO₂- and AIE-ratios are computed with respect to the first row (experiment A).

Therefore, the use of the monthly resolved emission inventory used in "C" (Wang et al., 2007) proves to have no substantial effect of the annually averaged AIEs – a result anticipated from personal communication of the first author of this study with A. Lauer (2011).

Therefore, using an emission inventory which utilises the geographical distribution presented in Wang et al. (2007) would simply reproduce the findings of L07.

To shed light on these issue in our paper, we have added some more information to the revised manuscript:

Introduction:

Concerning the results of Lauer et al. (2007): "Their results highlight the importance of using an adequate geographical distribution of the shipping emissions, *i.e.* spread-out about shipping corridors rather than concentrated along main shipping routes."

Discussion:

"We did not investigate the effect of employing a different geographical distribution of the shipping emissions, such as that presented in Wang et al. (2007) and used in Lamarque et al. (2010), because the effect on the obtained AIE-estimate would most probably be negligible compared to that obtained when using the given geographical emission-distribution of the QUANTIFY inventory (Behrens, 2006) (according to the results shown in Lauer et al., 2007)."

Statistical significance: compared to Lauer el al. 2007, a relatively low confidence level is adopted (90% vs. 99%) and nevertheless the regions of significance are much less and quite limited. How can this be explained, given that a similar nudged dynamics is used, with a comparable number of simulated years? How reliable are the conclusions, for example regarding aerosol

number (Fig. 4 and Section 3.2) or RF effects (Fig. 8 and Section 3.5) where most of the features which are discussed occur outside the marked regions of significance?

This is indeed a very important remark and we must say that we do not fully understand the low variability of the results shown in Lauer et al. (2007) compared to our results. As noted above, there are some differences in the representation of the aerosol system and the coupling to the cloud microphysics. The inclusion of nitrate aerosol may also lead to more robust changes regarding aerosol-cloud interactions.

However, we are confident in our test for statistical significance because the level of significance reduces as we follow the applied model changes down the process chain. That is, statistical significance is highest for changes in species burdens and then reduces for number burdens, AOD, CCN concentrations and ultimately AIEs.

Furthermore, the geographically resolved results of Righi et al. (2011) (their supplementary material), using the same model setup as Lauer et al. (2007), also show substantially lower significance compared to the results of Lauer et al. (2007). Righi et al. (2011) also used a confidence level of 90%, *i.e.* the same as we adopt for this study.

We could try to increase the statistical significance of our results by running even longer simulations. As running the model is however quite expensive, this is beyond the scope of this study.

We choose to keep the discussion of the results as is in the paper but we added the following notes to the manuscript:

Section 3.2:

"Although most of the discussed changes in particle number concentrations occur outside the specified confidence intervals, we are confident that the shown plots and explanations represent the actual processing of shipping emissions to a high degree of accuracy. Running the model for an even longer time period would most probably increase the areas of significant changes. As this is however computationally quite expensive, performing such extra simulations is beyond the scope of this study."

Discussion:

"It must also be investigated what processes on the sub-grid scale lead to the substantial reduction of statistically significant changes down the track from changes in emissions to changes in cloud radiative properties. The question then arises whether an ever more realistic treatment of aerosol processes in GCMs eventually leads to them becoming almost irrelevant on a global scale." Effect of reducing carbonaceous emissions: the effect of carbonaceous emissions as simulated in the experiments BnoBC and BnoC is basically negligible. This is not surprising, given the relatively low emissions of these species by shipping. The authors claim that this sensitivity study is important in view of future ship-fuel regulations. However, such regulations will deal mostly with sulfur, which is currently very high in ship fuels (Buhaug et al., 2nd IMO GHG study 2009). Therefore a sensitivity study with reduced SO2 emissions (like in Lauer et al., ES&T 2009 or Righi et al., ES&T 2011) will be much more valuable.

Thank you very much for this well-founded comment and we are very much aware of the fact that future control strategies for shipping emissions will focus on the reduction of the fuel sulfur content (as this is presently indeed very high). However, lesser fuel sulfur content is found to generally lead to cleaner fuel combustion and thus less emissions of BC/OC (see Lack and Corbett (2012) for a recent review). Compiling a representative emission inventory for assessing the effect of fuel sulfur content reductions near coast-lines (see *e.g.* Lauer et al., 2009) or even on a global scale is thus beyond the scope of this study and should be left to future modelling efforts.

As the title of our manuscript suggests, we perform here sensitivity experiments with the ECHAM-HAM aerosol climate model. It is in fact broadly accepted that carbonaceous emissions, especially BC, may have important implications for anthropogenic forcing of climate by either i) absorption of incident solar radiation which is even enhanced for internally mixed carbonaceous particles, ii) providing an increase in CCN numbers, iii) semi-direct effects or even iv) enhanced cloud droplet absorption of incident radiation if BC is mixed within (Ramanathan and Carmichael, 2008, and references therein).

Due to the fact that BC and OC emissions from shipping will most probably decrease in the future (along with sulphuric emissions of course) and that these emissions may have substantial climatic impact, we are confident that the results shown here are of scientific relevance. The results of our study very nicely highlight the subtle interplay between aerosol microphysical processes which merit attention in the scientific community.

We have added the following note to the revised manuscript:

"Although very intriguing, we explicitly do not attempt to assess the climate impact of emission controls resulting from a reduction of fuel sulfur content on local and global scales (IMO, 1998; Lauer et al., 2009; Righi et al., 2011) and this should thus be an important topic of future climate model

intercomparison studies."

Figures: the use of black contours to mark significant changes is confusing (especially in Figure 3, Figure 7 and Figure 8). I would rather use a hatch pattern (e.g. diagonal lines) to mark them or simply mask out the nonsignificant regions (in white or gray). The color-bar labeling is ambiguous: it is not clear, for example in Figure 3, what should be the value of the tick between 4 and 7 (5.5?) or between 7 and 15 (11?). In the caption, please specify that these are ship-induced (relative) changes and which kind of time average is shown (multi-year or specific year?).

We thank the reviewer for these recommendations for increasing the readability of our figures. For the revised manuscript, we have modified the figures and their caption accordingly:

- for the global plots, we have now changed the marking of the statistically significant to a diagonal hash pattern, following the reviewer's advice.
- The contour spacing was deliberately chosen non-linear in order to adequately show the whole range of results. We agree that the labelling of the colour bar is indeed ambiguous and we have now included an index for every change in contour colour.
- We have also adapted all the figure captions to highlight that we are showing ship-induced relative changes.
- for the zonal mean plots, we have increase the font size in the legend to increase readability.

MINOR REMARKS

Table 1 and Figure 1: since the AeroCom emissions for the shipping sector are not used in this study, I find these two panels quite misleading. I would remove Figure 1 and put the values for total emission of different species as additional columns in Table 2. This will also help to highlight the differences among the experiments and to compare with previous studies.

Thank you for suggesting this modification to the manuscript. We followed the reviewer's suggestion and removed Figure 1 and Table 1 from the manuscript and modified Table 2 (now Table 1) accordingly.

Figures 10 and 11: these could be merged in a single figure.

We have thought about this while producing the original manuscript. The readability of the panels would then suffer significantly. We therefore do not follow the reviewer's suggestion here.

P7074-L14: the smallest value should be given first: -0.32 to -0.07. The same applies to other parts of the manuscript.

Thank you, we modified the manuscript accordingly.

P7074-L15-17: "The magnitude of the AIEs depends much more on the assumed size distribution". Please mention the dependence on the geographical distribution.

As we do not perform an assessment of the dependence on the geographical distribution of the emissions, we do not follow the reviewers suggestion here. We have however added information on why we do not investigate the effect of the geographical distribution to other parts of the manuscript (see above).

P7074-L17-20: as mentioned above, the different geographical distributions used in previous studies could explain some of these differences.

We now mention in the manuscript that previous studies have found the AIE to depend on geographical distribution of the emissions (Lauer et al., 2007; Righi et al., 2011). Following our reasoning in reply to a previous comment, we shortly explain (in the Discussion section) why we think that in our case, the geographical distribution of emissions most likely does not explain the difference to Lauer et al. (2007). The differences are most likely due to a different model environment. We elaborated on this in reply to the reviewer's previous comments.

P7076-L1-3: "largest contribution to positive RF". Please add "anthropogenic". Anyway, according to IPCC the third contribution should be tropospheric ozone.

Thank you for this important comment. We have modified the manuscript accordingly:

"Thus, BC is perhaps the fourth largest contributor to positive RF, following CO_2 , methane and tropospheric ozone and reducing..." P7077-L26: replace "0.6" with "0.60" for consistency.

We applied this to the revised manuscript.

P7078-L3: give a reference for the total GHG RF value (e.g. Forster at al., IPCC 2007).

Thank you for pointing this out. We added the reference at this point.

P7082-L25: please specify the value of the height.

As the dynamics core of ECHAM is formulated on sigma-pressure levels, this height varies with meteorological condition and latitude. Over oceans, the model level above the surface level covers the volume from about 60 m - 150 m for mid-latitudes and 65 m -170 m for the tropics. We have added this information to the revised manuscript:

"Over oceans, the model level above the surface level covers the air-volume from about 60 m - 150 m for mid-latitudes and 65 m -170 m for the tropics."

P7083-L17-25: in this context, it would be useful to refer to the previous modeling study by Righi et al. (2011), who considered various size distributions corresponding to different ageing of the ship plume.

Thank you for hinting at this. We were in fact not aware of this study at the time of preparation of this manuscript and we thank the reviewer very much for pointing at it.

We have added this reference to the work of Righi et al. (2011) in various other parts of the revised manuscript as it contains important results with respect to our work.

P7084-L22: I would put a comma after vice-versa.

Thank you for pointing at this. We changed the manuscript accordingly.

P7085-L9: since a 90% confidence level is adopted in the manuscript, I would write this value here.

We have now added additional information on the performance of the t-test. That part of the manuscript now reads:

"The level of statistical significance applied to all plots is 90%, *i.e.* the null hypothesis (that the sample means are from the same population) is true with <10% probability and is thereby rejected in the regions indicated by the contours in the plots."

P7086-L3-8: this is not surprising, given the relatively low emissions of these species by ships. The last sentence should refer to Lauer et al. 2009, who reached a similar conclusion.

To avoid any ambiguities in the revised manuscript, we removed the phrase referring to the "surprising result" from this phrase. The reference to Lauer et al. (2009) was added.

P7089-L22: replace "atmospheric radiation" with "radiative budget".

We have changed this in the revised manuscript.

P7090-L15: name these regions. How do they compare to previous results, considering the differences in the geographical distribution of the emissions?

This effect is very often found off the western coast of southern Africa, especially during the biomass burning season (July - October). This has been quantified in a number of studies (see Peters et al., 2011a, for references). Previous GCM studies on the effect of shipping emissions do not show spatial plots of DREs as these are found negligible for shipping emissions. Thus, a comparison to previous studies is not possible here.

In our simulations, monthly resolved emissions from biomass burning are included according to AeroCom recommendations (Dentener et al., 2006) and we hint at this effect as it illustrates the subtle interplay of aerosol microphysical processes and the radiation budget on a regional scale.

We have added some additional information on this to the revised manuscript and the passage now reads:

"Interestingly, the DRE is distinctly positive in some parts of the semipermanent stratocumulus fields off the southern Africa west coast. The reasons for this effect in \mathbf{B} can be twofold."

P7091-L10: replace "models' " with "model"

We have changed this in the revised manuscript.

P7091-L15: I would replace "Figs. 9, 10, 11, 12 and 13" with "Figs. 9-13"

We have changed this in the revised manuscript.

P7094-L1: replace "0.6" with "0.60" for consistency.

We have changed this in the revised manuscript.

P7094-L3: replace "inrease" with "increase"

Thank you for hinting at this misspelling. We have corrected it in the revised manuscript.

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