**Letter to the Editor:**
We thank the reviewers for their generally positive feedback and their valuable comments for improvement of individual aspects of the paper. Before addressing the two reviews, we would like to point out that we found an error in the radiation scheme, with SOME MINOR EFFECTS on the cloud optical properties. We therefore repeated the simulations discussed in this article. The general conclusions and all main results of the article are not affected by this error. The macrophysical effects within the ship tracks as discussed in section 3.4 are, however, slightly reduced in magnitude and occurrence in the new simulations. For consistency, all numbers and figures have been updated in the revised version of the paper.

**Reply (blue) to comments (black) by referee #1:**

This paper presents the results of real-case simulations with the COSMO regional model of a prominent ship-track scene observed by MODIS over the Bay of Biscay.
Aerosol-clouds interactions and feedbacks are analysed, considering also various sensitivities. The results help understanding the features and limitations of cloud parameterization, and interpreting the current estimates of global aerosol-cloud forcing.
The suggested approach is very interesting and the topic is of relevance, given the relatively low level of scientific understanding of the involved processes and their importance for climate. This paper therefore fits well to the scope of ACP and deserves publication.
The manuscript is generally well written, although more clear and precise formulations are desirable in some parts (please find detailed suggestions below).

I have only a few questions regarding the emission setup of the model (Sec. 2.1.1), namely:

1.
The number-to-mass conversion is done using a measured radius of 0.04m from Hobbs et al. Does this assume that all the particles have the same radius or is a size distribution also assumed?

The number-to-mass conversion is done assuming the same median radius for all particles. This will be clarified in the text (P26728, L21). This admittedly introduces uncertainties in the emission mass estimate, as not all particles will be emitted at the same radius. However, the thereby introduced uncertainty to the emission mass flux can be expected to lie within our performed mass flux sensitivity experiments.

2.
It is not immediately clear how the size distribution parameters of Righi et al. are used here. From the caption of Table 1, I understand that they are used for partitioning the mass emissions between the Aitken and accumulation modes, but this is not clear in the text.

This information will be inserted in the text (P26729L3).

3.
It is also not clear why the authors use the measured radius by Hobbs et al. to convert number to mass, and then the radii of Righi et al. to assign the mass to the size modes. Although it is stated in Sec. 3.1 that this does not affect the results, why not consistently using the same values for both operations?

The emission size specifications based on Righi et al. (2011) were chosen, as they were especially and carefully constructed for parameterised aerosol size distributions based on a range of test bed and field measurements of ship exhaust. Based on expert judgment emission size specifications for fresh and aged size plumes were constructed. This not only provides us with reliable emission size estimates, but also allows us to investigate the sensitivity towards fresh and aged emission size specifications, as was done in previous global studies.

4.
Using a measured number emission flux introduces further uncertainties, as this quantity varies quite rapidly with the plume age. Was there a specific reason for this choice? A better possibility would be to start from mass emissions (e.g., considering typical emission factors for the given ship class from the literature) and convert them to number using the same parameters used for mass partitioning. This conversion depends on plume age too, but the uncertainty can be quantified using two sets of parameters (as the authors actually did when considering a fresh and an aged distribution for the partitioning).

We fully agree with the issue raised about specifying the emission flux using field measurements of the exhaust plume. These measurements are indeed very sensitive to the temporal and spatial variability of the plume and location of the measurement.
The suggested alternative approach relies on emission factors given the mass of fuel burned. Although these emissions factors are known for various ship types, estimating the burned tons of fuel as a function of time includes a range of uncertainties, too. The speed and loading estimate of the vessel significantly determine the amount of fuel burned per hour. Both of these entities are not known and poorly constrained, as no information regarding the vessels operating in January 2003 in this region is available. As this alternative approach requires more assumptions to determine the emission mass flux, we chose to work with the direct field measurements obtained in a similar boundary layer to the one considered in this paper.
In order to address the above-mentioned uncertainty of these estimates, sensitivity studies with regard to the emission mass flux and size distribution were performed.

Minor remarks:
P26722, L14-15: if possible, please provide the corresponding relative changes in CDNC and radius. Will be done.

P26725, L26: please also specify the vertical resolution. Will be done.

P26726, L11: was the entrainment rate parameter tuned? If yes, how?
The entrainment rate was not tuned, but simply set following Tiedtke,~M.: A comprehensive mass flux scheme for cumulus parameterization in  large-scale models, Q.~J. Roy. Meteor. Soc., 117, 1779--1800, 1989.

P26727, L17: is the global ECHAM-HAM simulations using the same cloud scheme, parameterizations and emissions? Please specify.  Specifications will be added in revised paper.

P26728, L9: what about the hygroscopicity of ash? Is it also relevant in this context?
Ash particles, like black carbon and organic carbon, are characterised by a very low hygroscopicity. Therefore they will have to be coated with soluble material to act as efficient CCN (Popovicheva et al. 2009).
Ash emissions were not considered in this study as they were:
a) not measured in the two field campaigns (Hobbs et al. 2000, Lack et al. 2009) used for the

specification of the ship emissions and
b) the implemented M7 aerosol module cannot describe particles of this size in its current form. However, we believe the error in neglecting this category of emissions to be small. In test bed measurements 4.2% (mass percentage) of the emissions were characterised as ash (Petzold et al. 2008). Given the large radius of these particles, the contribution to the particle number, which is proportional to mass/radius$^3$, will be small. Furthermore, due to their large size they can be assumed to sediment rapidly or to be scavenged quickly.
A comment about this will be added to the revised paper.

P26728, L21: I guess this is the median radius, not the mean. This will be corrected.

P24729, L16: I wonder whether the ship emission update time step (3 minutes) is dependent on the ship speed.
The emission update time step was shortened to 1.5 min for the v_ship=20 m/s simulation. For the 5m/s simulation the time step was kept the same. This can be done as the emissions are additive over time. This will be added to the manuscript.

P24731, Eq. 1: I guess SFC means surface. I would write it explicitly, or better, just write 0 there. Will be done.

P24731, L13: why is the cloud cover predominantly 0 or 1? Is it due to the high horizontal resolution?
The cloud cover is set to 1 if the grid box is saturated and grid-scale clouds form. The majority of stratiform clouds in these simulation is grid-scale, which is why the cloud cover is predominantly 0 or 1. As you interpreted correctly, this is primarily a consequence of the high resolution of the simulations. In addition to the grid-scale cloud water, sub-grid-scale water is produced by the shallow convection scheme. The cloud fraction in these boxes, however, is already set to 1 due to the grid-scale water content.

P26737, L19: up to which altitude/pressure is the column integrated? The column is integrated up to the PBL top defined by the determined inversion top. Comment will be added in revised paper.

P26738, L7-13: this seems to contradict the results of the cited global model studies (Righi et. 2011, Peters et al. 2012), which found a quite high sensitivity to particle aging. I think this is worth a comment. The following comment will be added:
This result is contradictory to global studies (Righi et. 2011, Peters et al. 2012), where a high sensitivity of the aerosol-cloud interactions to the aging of the prescribed emissions was found. The cause for these different sensitivities remains to be addressed. It could be due to different treatments of the cloud or aerosol microphysics within the different models, or it may be attributable to the different microphysical aging of the plume allowed by the higher resolution.

P26739, L10: here radiative effects are discussed, but Fig. 6c shows $\tau$. Will be corrected.

Text corrections:
P26722, L3: please replace "parameterisations" with "model parameterisations". Will be done.
P26722, L10-13: this sentence is long, rephrasing as "The simulations, which include moving ship emissions, show that..." may improve readability. Will be done.
P26723: L15: remove comma. Will be done.

L26726, L18: I would break the sentence after “dust” and start a new one. Will be done.

L26726, L23-24: replace the .larger or equal. symbol with .less-or-equal. Will be done.
P26727, L24: put a comma after “monoxide”.Will be done.
P26732, L25: for clarity, I would write “mass flux (ship10) or emissions size (ship10A)”.Will be done.
P26736, L17: replace “as is consistent” with “consistently”. Will be replaced with “as expected”.
P26738, L29: replace “Within the ship” with “In the ship”. Will be done.
P26740, L10: replace “this” with “the one analysed in this study”.Will be done.
P26740, L18: replace “on the order of” with “on a typical time-scale of the order of”. Will be done.
P26741, L20: delete “within”. Will be done.

**Reply (blue) to comments (black) by referee #2:**

Recommendation: Minor Revisions

Summary:
A real case involving ship tracks near Europe is simulated in a regional model at kilometer scale using a sophisticated bulk treatment of aerosols and cloud microphysics. Since no observations of the ship emissions from that case were available, ship emissions from another case study were used and scaled up by a factor of ten to produce cloud effects similar to those observed. The microphysical and macrophysical impacts of the ship emissions on the cloud are described in detail.

The experiments are well-designed, and the paper is well-written and presents its evidence clearly. I believe that it should be acceptable for publication if the authors address the concerns raised below.

Major comment:
1. This concern may reflect my particular biases and interests, but I was curious if the authors could quantify more clearly the macrophysical impacts described in section 3.4. My thought is that perhaps a table could compare the values of various quantities averaged over the plume points and points with significant cloud response in the ship10 simulation (as defined on p. 26730) and the same regions in the clean simulation. The numbers from the clean simulations would provide a baseline, and the differences between the two regions in the clean simulation might give some indication of the natural variability in the background state. My suggestion for possible quantities of interest are:
- surface precipitation rate
- liquid water path →
- total water path (\int qv+qc dz up to some height which lies above the inversion in both simulations) → dominated by qv (O(7g/kg)), O(0.1g/kg))
- average bl temperature (computed similarly with either T or liquid water temperature or the similar versions of potential temperature)
- BL-integrated radiative cooling, if available.
- surface sensible and latent heat fluxes
- BL-integrated Na and Nc
- average inversion height and cloud base height
- optical depth and/or SWCF

This list is probably excessive, but I thought that some quantification of these changes might give insight into the mechanism underlying the macrophysical cloud changes. If the authors choose to add such a table, only a subset of these would be necessary, I think.

The analysis in section 3.4 suggests that most of the LWP changes are due to the decrease in precipitation, but there's also some discussion of increased mixing. Since the background BL is not well-mixed, would it be possible to increase LWP simply by making the profile better mixed with identical BL-integrated total water and liquid-water temperature? The explanation in the paper about mixing liquid water down below cloud base is referring to this mechanism in part, though it also mentions increased cloud liquid due to weakened precipitation.
Many of the things suggested by the reviewer have been previously investigated by the authors. However, no clear signal other than precipitation suppression could be determined as a driver for the simulated LWP increase. Surface precipitation is small throughout the simulation (less than 0.1 mm/h). Surface energy fluxes fields are relatively homogeneous throughout the region and thus cannot be used as an argument for changes within the track regions. Significant changes in the PBL profiles of potential temperature and water vapour (qv ~ qt in these simulations) were also not found. The increase in optical depth was enhanced in these regions, due to the enhanced LWP increase. Radiative cooling, could also be excluded as the driver of additional condensation, as it is reduced when integrated over the PBL in regions of increased LWP, due to the increased solar absorption.
The only driver for this change is the particularly strong increase in precipitation in regions where the Nc increase was found to be 55% higher than the track mean. In order to demonstrate that these changes exceed the background variability, an LWP histogram for clean and ship10 will be added to Fig. 9. Furthermore, more quantitative statements will be given in the discussion of this phenomenon on p. 26741. Finally it will be clarified in the text, that the stability and the mixing coefficients within the PBL did not change, but that more qc is mixed downward in the PBL due to the higher vertical gradients of qc itself. The following table with LWP, total water path, rain water path, cloud droplet number and activated aerosol number burden, optical thickness will be added to the revised paper:

Table 3. Overview of changes in microphysical (cloud droplet and activated aerosol number burdens), radiative ($\tau$) and macrophysical entities (LWP, total water path (TWP) and rain water path (RWP)) averaged over the following and regions and simulations: the enhanced (50% increase) LWP region in *ship10*, the track regions in *ship10* and *clean*, and the background region for *clean*.

| entity | enhanced LWP *ship10* | track *ship10* | track *clean* | background *clean* |
|---|---|---|---|---|
| cloud droplet number burden [$cm^{-2}$] | 12622 | 8677 | 1644 | 1434 |
| activated aerosol number burden [$cm^{-2}$] | 90194 | 87448 | 28808 | 26493 |
| $\tau$ | 8.4 | 6.8 | 4.34 | 3.8 |
| LWP [$kg\,m^{-2}$] | 0.067 | 0.055 | 0.038 | 0.033 |
| RWP [$kg\,m^{-2}$] | 0.007 | 0.01 | 0.017 | 0.016 |
| TWP [$kg\,m^{-2}$] | 8.5 | 8.6 | 8.6 | 8.1 |

Minor comments (28/14 means line 14 on p. 26728):
28/14: Does "global emissions" refer to "global shipping emissions"? Please clarify if necessary. Will be corrected to "global shipping emissions".

28/25: For clarity, I would suggest starting this paragraph with "In the present simulations, ..." The similar phrase on 29/8 could be deleted, 31/eqn 1: Please specify the dependent variable for tau, e.g. \tau(x,y,\lambda_i) = \int_{SFC}^{TOA} … Will be done.

31/14-17: Could this approximation be responsible for some of the model-MODIS disagreement in figure 11?
We have tested for this. We implemented the current version of the COSP simulator used in COSMO (which only considers grid-scale cloud water) into our code and compared our analysis of optical thickness (for grid-scale clouds only) with the COSP product. In this comparison, we see no differences that would indicate a bias due to our diagnostic alone. However, as a considerable part of the cloud water content is due to SGS cloud water, we decided not to use the COSP simulator for the comparison with the MODIS satellite.

31/23: Please use consistent units for the subsidence rate here. Will be done.

32/19-20: What does "near the upper troposphere" mean? I would suggest using "lower free troposphere", above the boundary layer but below about 650 hPa, or "mid-troposphere", roughly 400-600 hPa. Will be done.

33/18: Duplicate cm-3 seems to be a typo. Will be done.

33/19: I would suggest changing "merely" to "only" if that doesn't change the meaning. Will be done.

34: Can the authors speculate as to why the ship emissions need to be scaled up by a factor of ten as compared to Hobbs et al (2000) to roughly match the microphysical and optical properties of the observed shiptracks. Is it the emissions, the homogeneous distribution of the emissions over four kilometer-scale grid cells, or some microphysical process that takes over while the plume is processed? My thought for future work is that a Lagrangian treatment of the plume might provide better estimates of subgrid variability (which might be able to be plugged into a PDF-based cloud scheme) and less numerical diffusion. However, this thought is less than half-baked at present, as I'm not sure how to handle the turbulent mixing of the plume in that framework. Further, treating the aerosol microphysics (coagulation, sedimentation, etc. ) along many Lagrangian trajectories might get expensive.
A comment will be added. It remains to be seen in future work, if this really is a dilution issue. If it were a dilution issue, a different approach, such as the one suggested here, would certainly be needed and its feasibility explored.

36/3: Why does theta increase above the inversion? Is it related to the increased subsidence with time? Due to the large-scale subsidence the lower-troposphere displays a sub-adiabatic temperature gradient, as indicated by the stable theta profile. However, the subsidence rate itself is not found to vary with time. The inversion itself is increased and strengthened due to the continued PBL collapse during this time.

36/29-37/2: This sentence can be removed. There is no need to apologize for making scaling arguments about turbulent flows. Much can be learned in this way. Will be removed.

39/10: Since SW CRE is negative, speaking of "increased" SW CRE is a bit confusing. I would suggest using "stronger" or "strengthened" instead. Sentence will be changed.

39/15: Add "the changes in" before "microphysical properties …" Will be done.

39/18: I think that "were" fits better in this sentence than "where". Will be done.

pp. 26740-41: See major comment 1. above. See reply above.

41/17-18: I have trouble imagining a situation where the top of the sub-cloud layer is not near saturation. Perhaps, beneath a near-surface inversion underlying arctic stratus... Unless the authors have a particular scenario in mind, the paragraph would be fine without this sentence, I think. Will be done.

43/26: This sentence is a bit awkward. If all simulations with ship exhaust (even ship) had a doubling of tau, then I would suggest: "Furthermore, all simulations with ship exhaust displayed at least a doubling of tau with respect to the background." If this wasn't true of ship, then remove "or did not". Will be changed to reviewers suggestion.

45/2-4: In the first paragraph of section 3.4, increases in qc are attributed to reduced precipitation, while there is also a suggestion of sub-cloud moistening by precipitation evaporation. While a better-mixed boundary layer is likely playing a role, my impression is that cloud base changes are driven by some combination of increased mixing, increased BL radiative cooling and decreased precipitation. This might be clarified by the table suggested above.
See comment above.

58/fig 5: Might a panel d showing profiles of total water (qv+qc) be helpful in showing the increased mixing of the boundary layer moisture as the ship tracks develop?
The total water content qt is dominated by qv, as qc contributes less than 1%. Therefore, not much would be seen in such a profile.

**Additional References used in replies only:**

O. Popovicheva, E. Kireeva, N. Shonija, N. Zubareva, N. Persiantseva, V. Tishkova, B. Demirdjian, J. Moldanov and V. Mogilnikov: Ship particulate pollutants: Characterization in terms of environmental implication, J. Environ. Monit. ,11, 2077–208, 2209.