

## Response to Referee 1

We thank the referee very much for the many relevant comments. They helped to improve the paper considerably.

Note that we conducted new simulations because the ship emissions were shifted by two weeks in the old simulations. This especially affected the impact of transit ship emissions in the Arctic in late summer 2050. Furthermore, we increased the number of simulated years from 10 to 20 for better statistics, as suggested by referee 3. In some cases, the results have thus changed; as an example, the SW CRE now increases significantly with additional tenfold Arctic ship emissions in 2050.

Referee's comments in blue, our replies in *grey and italic*.

- In general, a lot of the discussion concerning specific results does not include the numbers in question and not references to where to find these numbers in the text (if present). This makes the discussion only qualitative. This is a shame, when the numbers are clearly available from the model results. Also, the reader ends up flipping back and forth looking for the numbers to back the discussion. Below, I will list some places in the text where this should be addressed.

*Thank you very much for this comment. In the new version, we include now more quantitative information, namely numbers in brackets, additional Supplementary figures (Supplementary Figs. 4-14), and Tables 2 and 3.*

- The reader lacks some of the basic information about the set up and the control simulation to be able to understand the results. There should, for instance, be a plot of the sea ice concentration at annual minimum or averaged over each season available for both periods, at least in the supplementary.

*We include now plots averaged over each season for 2004 and 2050 in the paper (Fig. 2).*

- A lot of the changes that occur between 2004 and 2050 are discussed, but not shown. This goes for example for temperature and precipitation. Make sure to label when the results you are referring to are not shown (see comments below) and please consider to show more of the changes that you use in your explanations, at least in the supplementary.

*We included additional figures in the Supplementary Material (Supplementary Figs. 4-14) and now mention when something is not shown.*

- For some figures, you average from 70-90N, for the tables you use either 60-90N or 75-90N. It would be more consistent if your figures and tables matched and one could follow the impacts of interest from figure to tables etc. Please consider changing this.

*We now average the figures of concern (aerosol size distributions) between 75° and 90°N instead of 70° and 90°N to be consistent with the averages displayed in Tables 5 and 7, and with most averages given in brackets.*

- Parts of the text is very oral and parts are too elaborate. Below I make both comments on things that should be changed and comments about how the text itself can be improved.

o P2, L24: "Nowadays" is very oral. Please rewrite. Also include "(. . .) pristine compared to other regions (. . .)" The use of depleted here make it sound like the aerosols have been removed. Please rewrite.

*Changed to:*

*"Compared to other regions, the present-day Arctic air is exceptionally pristine, and aerosol levels are very low."*

o P2, L32-P3,L8: May be a bit hard to follow because the indirect effects are described before the general radiative effects of clouds. Consider a rewrite to change the order.

We shifted the effects of aerosol-cloud interactions on radiation to the paragraph where the CREs are described.

o P3, L18: “Re-emission of SW”? Please rewrite!

Changed to:

“Aerosol scattering of shortwave (SW) radiation tends to cool the atmosphere, whereas absorption of SW and longwave (LW) radiation tend to warm it (Boucher et al., 2013).”

o P3, L23-27: Suggest to use the same terms for SW and LW description. For LW, the emissivity includes the water path and the temperature is height dependent. I suggest to make the definitions a bit more tidy.

Changed to:

“Similar to aerosol particles, clouds impact the Earth's radiation budget by absorption and emission of LW radiation (warming) and scattering of SW radiation (cooling). To a smaller extent, LW radiation is also scattered and SW radiation absorbed (Chou et al., 1999; Slingo 1989). The absorption and emission of LW radiation is a function of the emissivity of the cloud (which depends on microphysical cloud properties and the water path), the (height-dependent) cloud temperature, and the surface temperature (Corti and Peter 2009; Chen et al., 2006; Alterskjaer et al., 2010; Shupe and Intrieri, 2003). The scattering of SW radiation is a function of the microphysical cloud properties, of the cloud water path, of the solar zenith angle, and of the surface albedo (Corti and Peter, 2009; Liou et al., 2002; Shupe and Intrieri, 2003).”

o P4, L11: a bit confusing to have figure references in a listing of the main goals of the paper. Suggest to move this.

We moved the figure reference after the goals and changed the text to:

“Figure 1 provides a simplified overview of how the increase in Arctic temperature can affect radiation. The most important interactions between atmospheric variables, aerosols, clouds, and surface properties are included. The figure shows that the increase in temperature directly affects sea ice, specific humidity, and aerosols. Changes in these variables can then directly or indirectly impact clouds and radiation.”

o Figure 1: A bit confusing. Why not use red for increase and blue for decrease?

We adapted the figure following your suggestions.

o P5, L12: Does lowering the CDNC threshold affect the global radiative balance?

We used a model version that was retuned, which is now explicitly mentioned in the text.

Changes to:

“Thus, we decided to use  $10 \text{ cm}^{-3}$  as a lower threshold for the CDNC everywhere and retuned this new model version.”

o P8,L10: Define the abbreviations COADS and AMVER.

We did this for COADS. For AMVER, the acronym is better known than what it stands for, and the full name of AMVER has changed several times. Therefore, we rather refer to the homepage with a footnote.

o P9, paragraph starting on line 18: I find this very confusing and suggest a clarification of what you mean in this paragraph. Do not see the link between these two sentences. The reasoning therefore fails.

o P9, L25: Please rewrite to “(. . .) can be considered as a realistic (. . .)”

o P9 General comment to the justification of a tenfold increase of the ship emissions.

I suggest a rewrite of this discussion. You increase emissions so as to see a signal and try to justify it afterwards, while at the very end of the paragraph state that the emissions are now “probably too high”. Increasing your emissions to get a signal is fine. To discuss that emission estimates may be too low is fine. Your emissions may well be an upper estimate. However, the discussion is long, a bit vague and a bit on the defensive side and makes the reader question whether the authors question their own reasoning here.

We rewrote the text to make it clearer (page 8 in the new document). We tried to account for all of your suggestions.

o P9, L30: Please define your abbreviations and give references to the models used. Did you use RCP8.5 for future simulations here? Please specify.

Changed to:

*“Both SIC and SST are prescribed in ECHAM6-HAM2. For future conditions, we used model results from the Earth System Model MPI-ESM as input (simulation for the climate model intercomparison project phase 5 (CMIP5), RCP8.5; Giorgetta et al., 2013).”*

o P10, L9: The results would have appeared more robust if the sic and SSTs used were an average taken over eg a ten year time period centered at 2004 and 2050. Using one year (2003) and one ensemble (2050) to test the robustness of your choice of sic and sst is too week.

*We agree that the interannual variability in SIC and SST is large and only looking at one other ensemble might not be sufficient to confirm that our results are robust. However, we refrained from averaging SST and SIC over ten years since we wanted to conduct simulations with a realistic state of the Arctic Ocean. By averaging over SIC, less regions are either ice-free or totally covered with ice; instead, more regions with intermediate sea ice coverage exist, which can have impacts on aerosol emissions and clouds. Furthermore, we are not sure how much sense it makes to average SST in regions which are sometimes ice-free and sometimes covered by sea ice.*

Changed to:

*“We refrained from averaging SIC and SST over several years (e.g. 2000-2010) to avoid having spurious regions with intermediate SIC and SST. However, the interannual variability in SIC is pronounced, therefore we performed test simulations using SIC and SST from: i) the years 2003 and 2004 from AMIP and ii) the first and the second ensemble members from the MPI-ESM CMIP5 simulation for the year 2050. Overall, the Arctic SIC in 2003 was somewhat smaller than in 2004, and the SIC in the first ensemble member from MPI-ESM was smaller than in the second ensemble member. We found that the basic results and main conclusions do not depend on these differences in SIC but looking at only two years for both present-day and future might not be sufficient to confirm that all our results are robust. In the following, we will always refer to the simulations using SIC and SST from 2004 and future SIC and SST from the first ensemble member of MPI-ESM.”*

o P11,L10: reference needed after statement “Furthermore, most models (. . .) prevalent in the Arctic”.

Changed to:

*“Furthermore, models of different types generally have problems to reproduce the structure of mixed-phase clouds prevalent in the Arctic (Morrison et al., 2009; Klein et al., 2009; Fan et al., 2011; Morrison et al., 2011; Possner et al., 2017), and the future sea ice extent as well as the prescribed aerosol emissions are highly uncertain (Collins et al., 2013).”*

o P11,L17: When you say “significantly” here is there a statistically significant change? Please specify.

Yes. Supplementary Fig. 4 shows the changes. Moreover, we added the following sentence at the end of 2.4:

*“Throughout this paper, the term “significant” is interchangeable with “statistically significant”.”*

o P11,L18: How much does the SIC decrease? Please specify.

We added the figures showing SIC (Fig. 2).

o P12,L3: “(. . .) modified (. . .)” What does modified mean here? Consider removing.

It refers to the fact that the sea salt parameterisation by Long et al. 2011 has been extended with corrections for SST, as mentioned in Section 2.1.2.

Changed to:

*“Present-day emissions are a factor of  $\approx 3$  lower in our simulations compared with Browse et al. (2014), which results from the differences in the two parameterisations (Gong, 2003; Long et al., 2011, with SST corrections) as shown in the study of Long et al. (2011).*

**o P13,L5: Is it only the change in CCN concentration that affects CDNC? Not moisture availability? At what supersaturation do you calculate your CCN concentration? Does the average supersaturation change between the runs?**

*Thank you very much for pointing this out. We checked and found indeed that not only the changes in aerosol particles, but also changes in meteorology are responsible for the increases in CDNC. In our model, the CDNC depends on the calculated CCN concentration and the lower threshold of  $10 \text{ cm}^{-3}$ . The CCN concentration is calculated interactively following Köhler theory (parameterisation of Abdul-Razzah and Ghan, 2000) based on the aerosol size distribution and the maximum supersaturation. The maximum supersaturation depends e.g. on the updrafts, the temperature, and the CCN number concentration. We found that the updrafts available for activation increase between  $75^\circ$  and  $90^\circ\text{N}$  below  $\sim 750 \text{ hPa}$  in early autumn (Supplementary Fig. 7), which contributes to the enhancement in CCN concentration.*

*Changed to:*

*“In general, the number of aerosol particles acting as CCN increases in the future, which leads to enhanced CDNCs (Fig. 4d). The increase in the number of CCN is not only caused by the increases in oceanic aerosol emissions, but also by changes in meteorology: the updrafts available for activation increase in the boundary layer between  $75^\circ$  and  $90^\circ \text{ N}$  in early autumn (Supplementary Fig. 7), which supports the formation of cloud droplets in this region.”*

**o P13, L22: Please explain why the increase in ICNC near the surface is due to the increase in CDNC.**

*This explanation was wrong. We thought that the increase in CDNC (as well as the increase in droplet radius) increases the contact freezing rate, but this is only important in limited areas far north. The simulated increases in ICNC are due to enhanced convection.*

*Changed to:*

*“The increase of ICNC near the surface is mainly caused by enhanced convection, which leads to small but numerous simulated ice crystals following the temperature-dependent empirical parameterisation of Boudala et al. (2002).”*

**o P13,L25: insert (not shown) after (near Svalbard).**

*We show this now in Supplementary Fig. 9. Furthermore, we correct “where precipitation is most enhanced” to “where convective precipitation is most enhanced” (which dries out the atmosphere and thus decreases cloud cover).*

**o P16,L2: ((. . .) except over the Arctic Ocean (. . .))” This is over the sea ice?! This should most definitely be specified.**

*Correct. Changed to: “except over sea ice”*

**o P16,L6: “since the clouds (. . .), more SW radiation can be absorbed (. . .)”. Consider rewriting this for clarity.**

*Changed to:*

*“Part of this warming might be caused by BC and dust aerosols above clouds (Supplementary Fig. 10): the clouds reflect more SW radiation than the snow/ice-free surface and part of the scattered SW radiation can also be absorbed by aerosol particles causing an increase in aerosol absorption as compared to clear-sky conditions (see e.g. Myrhe et al., 1998).”*

**o P16;L13: What does the optical thickness change from and to? If you do not give the numbers you need to specify that it is not shown.**

*We added the numbers in brackets and in Tables 2 and 3.*

**o P17,24: Please insert “(not shown)” after “temperature”.**

We added the figure for surface temperature changes in early autumn to the Supplementary Material. Furthermore, the surface temperature is included in Tables 2 and 3 and now further discussed in the text. The text is changed to:

“This is because i) the SW component dominates in these months due to the higher zonal zenith angle and ii) the surface temperature over the central Arctic Ocean does not show pronounced increases like in early autumn (Table 2), therefore not enhancing the LW CRE. The surface temperature even decreases in some regions because melt ponds on ice can have temperatures higher than 271.38 K (but below 273.16 K) in late summer, while the SST is 271.38 K in gridboxes with  $0 < SIC < 1$  (equilibrium conditions, i.e. heat changes lead to changes in SIC, not SST).”

o Table 2: Please include the values from the control run to get the relative importance.

We now include the values from the control runs. Moreover, we splitted Table 2 and Table 3 (in the original document) for better readability into two tables each, one describing differences due to natural changes (Tables 4 and 5 in the new document) and one describing changes due to ship emissions (Tables 6 and 7 in the new document).

o P22, section 3.2.1 general comment: Your hypothesis is very likely, but can you verify in your model that this is the case? Perhaps you could perform some sensitivity test?

Right now this section is not very strong as it just lists model results without any proper discussion. It would also be good to include a vertical cross section of the aerosol change. This would be beneficial for the next section as well when discussing freezing.

We added Supplementary Fig. 13 to strengthen our argumentation and discuss it in the text. Changed to:

“The additional aerosol particles emitted by ships provide additional surfaces for the condensation of gaseous sulphuric acid. Thus, the vertically integrated condensation rate of sulphate increases where the ship emissions occur (not significant; Supplementary Fig. 13b). The vertically integrated nucleation rate of sulphate shows neither a clear decrease nor a clear increase along the shipping paths (Supplementary Fig. 13d); if the increase in condensation suppressed nucleation, we would expect a decrease in the nucleation rate. However, the vertical cross section of aerosol particles in the nucleation mode shows that the number concentration indeed decreases significantly near the surface (Supplementary Fig. 13f).”

o P22,L29: How much does the Liquid water path /mass increase?

We now include the LWC in Fig. 11 (subfigures e and f).

o P23,L2: Please include the numbers you are describing in the text.

Changed to:

“Using satellite data, Christensen et al. (2014) studied the effect of ship tracks on both mixed-phase and liquid clouds. In the late summer of 2050, the clouds that are impacted by ships in our simulations are mostly liquid. Therefore, we restrict our comparison to the influence of ships on liquid clouds. Consistent with the observations by Christensen et al. (2014), we also found decreases in the effective radius and increases in cloud optical thickness. The relative changes in effective radius are larger in their observations (-20% at cloud top height) than in our simulations (-2% to -4% at altitudes below 500 hPa; averaged between 75° and 90° N, whereas changes in cloud optical thickness compare well (+20% in both studies, averaged between 75° and 90° N). The LWP slightly decreases in their analysis (-1%; in-cloud); in contrast, it increases in our simulations (+17%; all-sky, averaged between 75° and 90° N). While our simulated precipitation shows no clear trend, the results by Christensen et al. (2014) suggest that ship emissions delay precipitation by enhancing cloud lifetime. The different results could be explained by the location of the ship tracks analysed by Christensen et al. (2014): the majority of their samples lie between 45° S and 45° N, and only very few datapoints are from the Arctic. Precipitation formation at high latitudes differs considerably from that at low latitudes since e.g. convection is usually much more important at low latitudes.”

o P25,L32: The significant areas are not large and looking at the figures it looks like the numbers you are giving here are averaged over the whole region. If so, please make it clear that this number is not only including significant changes.

We wanted to highlight with the given numbers that the local significant changes are small in absolute amount. We removed the numbers since they confused more than they helped. In the next sentence, it is mentioned that the changes in radiative forcing of BC deposition are much smaller than the changes in CREs.

o P25,L32: The figure reference should be to figure 13(f)?

Yes, indeed!

o P27,L8: “While the CRE (. . .)” is it not the change in CRE? Same goes for line 7 and the figure text to figure 14.

Yes, thank you.

o P27,L9: The change in optical thickness is significant in very small regions. This should be mentioned. Also when the average numbers are given in the following sentence, make clear that these numbers are not significant.

The changes are now significant and more widespread in the new simulations.

Changed to:

“While the changes in CRE caused by changes in cloud cover and cloud top altitude are not significant (Fig. 15a-d), the increase in cloud optical thickness leads to significant decreases and increases in the SW and LW CRE, respectively (Fig. 15e, f). Averaged between 75° and 90° N, the increased optical thickness changes the SW CRE by  $-4.6 \text{ W m}^{-2}$  and LW CRE by  $0.52 \text{ W m}^{-2}$  in late summer (significant).”

o P27, L14: Are these numbers significant?

Yes; we now mention that.

Technical corrections: - Polarstereographic maps: Please insert a few more latitude lines, perhaps at the borders for averages that you use: 70N and 75N.

We inserted every 5° a latitude line.

- Figures using a blue to red color scale: The lightest colors are impossible to separate in printed figures. You need to improve this color scale. At the same time, consider to use a white color surrounding zero so that values at this separation (zero values) do not come out in color.

We added a white surrounding around zero and adapted the color scales for many figures.

**The following minor changes were adapted following the referees suggestions:**

o P2, L17: Remove “where some (. . .) are labelled”

o P2,L20: Replace “until” with “before”

o P2, L22: Remove “(cruise ships)”

o P2, L28: Remove “and can (. . .) (Vali, 1985)” It is of no relevance here and only distracts the reader.

o P2, L32: Make part of the previous paragraph.

o P3, L1: Perhaps mention why smaller droplets increase the cooling effect of clouds?

o P3, L31: Suggest new paragraph before “How Arctic clouds (. . .)”

o P4, L22: Suggest to remove: “HAM2 (. . .) modes.” and move “To link (. . .) implemented (. . .)” to the end of the next paragraph, after “(. . .) sedimentation”.

o P6, L13: Suggested rewrite: “(.), we used an inventory described in (. . .)”

o P6,L19: Remove “more equations can be found therein”

o P8,L15: Remove “in addition”. It is a bit confusing

o P8, L19: Please replace the word “exploit”

o P8, L21: Consider removing the sentence “We processed (. . .)”. Too detailed information in my opinion.

o P9, L10: Suggest to remove “(non- (. . .) control)”.

o P11, L3: suggest to remove “(eg. SW radiation, temperature)”. It is not necessary.

o P11,L3: Rewrite to “change considerably”

- o P11,L9: Rewrite to “deviate considerably”
- o P11, L17: Rewrite “in the vicinity”. Suggest “Over the arctic ocean”
- o P13,L3: Please insert the “(. . .) increases in the future (. . .)”
- o P13,L8: “averaging over cloudy and non-cloudy conditions”. This is a bit confusing. Are you writing about allsky conditions? Also, please replace increases with increased.
- o P16,L10: cooling effect vs warming effect. Please specify the actual numbers here.
- o P16,L14: Consider replacing disentangle by “distinguish between”
- o P22,L7-8: Please insert “(not shown)” after “late summer” and after “significant for OC”.
- o P22,L12: Please remove “using the hypsometric equation”. Redundant.
- o P22,14: Please move your reference to figure 9b to directly after “800 hpa” to avoid confusion.
- o P25,L21: How much does the optical thickness change. You should strengthen your discussion by describing the actual model results.
- o P25,L27: “(. . .) under clear-sky conditions.” Insert “(not shown)” here.
- o P27,L4: please consider changing the numbers here to (-2 to -20 Wm<sup>-2</sup>)
- o P27,L5: please insert “(not shown)” after “correlated with ship emissions”.
- o P27,L16: Please insert “very”: “(. . .),ship emissions lead to a significant, but very weak (. . .)”
- o P27,L20: Suggest to add “in limited regions” or something similar after “(. . .) and lead to significant net cooling”
- o P32,L16: Insert “a very small” in front of “local warming”.
- All figures: Move (a), (b) etc above figures.
- Figure 7: Suggest to use different color scales for positive and negative values in (a) and (d).