Response to anonymous referee #1

We thank reviewer #1 for valuable comments which helped improve the manuscript. All the reviewer's points have been carefully considered, and our manuscript has been modified accordingly.

Below follow answers to the general and specific comments.

General comments

As a general criticism, it would have been helpful if the aspect of model validation and individual model strengths and weaknesses, including recent model improvements if applicable, would have been described in slightly more detail. As a pure model study without any element of validation against measurements a mere reference is made to a paper where the models were validated previously. Even though the paper is in its present form rather long, a brief synthesis of the findings from this validation study would be helpful to put the results into context and to allow a more critical interpretation of the findings.
Answer: The authors agree and have included a brief synthesis of the validation study of Schnadt et al. (2010) in the beginning of Section 3. Based also on the comments of referee #2, we have included comparisons with multi-year ozonesonde observations from Logan (1999) in the appendix of the revised manuscript.

Many references are made to other papers when discussing the methodology used in this study. More detail should be added in order to make the reader more easily understand the methods used without having to read all the other papers. A complete and full description is not always required but sentences similar to "we have used the same method as in Grewe et al 2010" are not really helpful.
Answer: This has been improved in the revised manuscript.

Specific comments

Page 16804, Line 16: The forcing contribution of contrail cirrus is the climate change mechanisms with the largest uncertainties. If this mechanism is presented as the primary driver for aviation impacts then this uncertainty needs to be made clear.
Answer: This has now been made clear.

Page 16805, Line 21: The Grewe et al (1999) study precedes Sovde et al (2007) by eight years. It can be reasonably expected that significant model improvements have taken place during that time (e.g. chemical reaction rates) and therefore substantial differences in the findings should not come as a surprise. I would recommend to compare Sovde 2007 to a further more recent study if available. Potential model developments could also be mentioned on p. 16816, line 1.

Answer: A more recent study than Grewe et al. (1999) was unfortunately not found for future simulations of aircraft impacts on atmospheric ozone. However, despite the 8 year difference the two studies yield fairly similar result. Assuming ozone concentrations in the UTLS of around 100 ppbv, the ozone impact of aircraft emissions predicted by Grewe et al. (1999) is 7-10 ppbv (depending on the emission scenario used), which is comparable to the 10 ppbv value of Søvde et al. (2007). Potential model developments are now mentioned where indicated in the manuscript.

Line 28: Is "project" the right word here? How can a study from 2007 project composition changes for the past (i.e. the year 2000)?
Answer: The word "project" has been replaced by "estimate".

Page 16806, Lines 2-3: This requires further clarification because at the moment this sentence does not seem to add any relevant information; what has been done in Eyring et al 2007 and how is it relevant to this study? If not relevant consider omitting.
Answer: This sentence has been rephrased for clarification. The authors find the sentence relevant as the two ship emission scenarios for 2030 (3.10 and 5.95 TgN/yr) in Eyring et al. (2007) bracket the ship emissions used for 2025B1 in our study (4.93 TgN/yr). Also, in the results and discussion section we have compared with the two scenarios in Eyring (referred in the manuscript as the low and high scenarios of Eyring), and it therefore seems relevant to include a sentence in the introduction describing some main findings from that study.

Line 25 ff: It would be better to explain first what the objective of the study is, i.e. considering scenarios for possible high or low developments (what developments are meant, technological developments?) and when this has been explained it can be clarified which SRES scenarios were chosen and why those were chosen. Otherwise the reader will at first not understand the reasons for choosing B1 and A1B and A1B HIGH, in particular as road transport emissions are pointed out here in a study on non-land based transport.
Answer: The authors agree and have rewritten the paragraph to first explain the objective of this study and then describe the scenarios used.

Line 22: Cannot find van Aardenne et al, 2005 in reference listing.
Answer: van Aardenne et al. (2005) has been added to the reference list.

Page 16808, Line 2: Figure 2 is virtually illegible when printing the article; this must be much larger in the final ACP paper. Figure 2 caption: the units contain an error, remove the last m. Consider whether a figure title is really necessary as the information can be gained from the figure caption.
Answer: The unit error has been corrected and the figure title has been removed.

Line 23: Remove "the" before 12 new shipping routes.
Answer: This has been corrected.

Line 25: If the Northern Sea Route is mentioned here specifically then this route should be made recognisable (e.g. by an arrow) in Figure 2 (cannot determine if that has been done).
Answer: The Northern Sea Route has now been marked by an arrow in Figure 2.

Page 16809, Line 8: Does this mean A1B HIGH experiments are not part of this study? Why was it mentioned then at all earlier?
Answer: The follow-up study dealing with the A1B HIGH scenario was mentioned to justify why we had left out road traffic from this study. The authors agree that this could be confusing and have now skipped mentioning A1B HIGH in the introduction.

Line 10 ff: It should be mentioned in the text that biogenic & soil emissions were kept constant for all years according to Table 1.
Answer: A sentence mentioning this has been added to the revised manuscript.

Line 16: This needs to be made clearer; which models did not change their CH4 emissions or surface mixing ratios? What were the 2000 surface boundary conditions, was it one global value (how many ppbv?) or was there regional variation considered?
Answer:
There were some differences in the setup of the CH_4 mixing ratios of the different models. Two of the models, p-TOMCAT and MOCAGE, kept the 2000 values also for the future runs, while the other four models (OsloCTM2, UCI CTM, TM4 and LMDz-INCA) updated the mixing ratios based on IPCC (2001). The global CH_4 abundances from IPCC (2001) were 1760 ppbv, 1909 ppbv and 1881 ppbv for 2000, 2025B1 and 2050B1, respectively. The models applied a hemispheric scaling with approximately 5% higher abundances in the northern hemisphere than in the southern hemisphere.

In order to investigate the effect of using different CH_4 surface mixing ratios, sensitivity simulations have been performed with the OsloCTM2 model. Reference (BASE) and perturbation simulations (AIR and SHIP) were carried out for the 2025B1 scenario with the CH_4 abundance fixed to year 2000 (1760 ppbv). The results were then compared to the original OsloCTM2 simulation using 2025B1 CH_4 abundance (1909 ppbv). The 2025B1 scenario was chosen because the difference from the year 2000 CH_4 surface mixing ratio is larger than for the 2050B1 scenario, hence this sensitivity test provides an upper estimate of how the different CH_4 mixing ratios may affect our results.

Table 1 shows that the relative methane lifetime changes due to aircraft and ship emissions are only slightly affected (up to 1-2%) by using CH_4 abundances for 2000 instead of the 2025B1 value from IPCC (2001). Global distributions of aircraft- and ship-induced ozone and OH have also been investigated, and the impacts of changing the CH_4 mixing ratio were found to be insignificant.

The manuscript has been updated clarifying which models kept the 2000 CH4 values in the future simulations, and what the CH_4 abundances were in 2000, 2025B1 and 2050B1. A sentence stating that there is only a small effect on the aircraft- and ship-induced

perturbations when using 2000 surface methane in the 2025B1 simulations has also been added.

Table 1. Relative changes (%) in methane lifetimes (integrated up to 50 hPa) due to a 5% decrease in aircraft emissions and due to a 5% decrease in ship emissions, as calculated by the OsloCTM2 model for two different CH_4 surface mixing ratios. Values are given relative to the BASE case, and are scaled to 100% by multiplying with 20. Note that this table does not include the feedback effect of methane changes on its own lifetime.

	AIRCRAFT		SHIPPING	
CH_4:	2000 (1760 ppb)	2025B1 (1909 ppb)	2000 (1760 ppb)	2025B1 (1909 ppb)
2025B1	1.110	1.113	4.013	3.957

Line 18 ff: Consider rephrasing this paragraph. Why were new reference runs required for B1 ACARE? (Line 24)
Answer: New reference runs were required because we want to study the atmospheric chemistry impact of aircraft emissions under the B1 ACARE scenario versus the impact of aircraft emissions under the B1 scenario. Hence we need one reference run (with 100% aircraft emissions) and one perturbation run (with 95% aircraft emissions) for each of the scenarios. The paragraph has been rephrased to clarify this.

Line 28: Should it not read "scaled" instead of "unscaled"? This is confusing. Scaled to what?
Answer: It should read "unscaled" (i.e. impacts due to 5% emission perturbation), please see the two answers below. The sentence has been modified for clarification.

Line 25: Have the 5% scaling been applied to all emitted species or only to NOx?
Answer: The 5% reduction of emissions in the perturbation simulations has been applied to all emitted species of the transport sector. This has been clarified in the revised manuscript.

Page 16810, Line 12: What is meant by "unscaled"? Is it the result due to a 5% emission perturbation? Would 100% then be equal to a complete removal of emissions from the respective transport sector? This needs to be made clearer.
Answer: This has been made clearer in the revised manuscript. "Unscaled" means the result due to a 5% emission perturbation (i.e. the direct difference between BASE-AIR and BASE-SHIP), while "scaled to 100%" means that the result due to a 5% emission perturbation has been multiplied by 20 (i.e. 20×(BASE-AIR) and 20×(BASE-SHIP)).

Line 24: It would be good if a brief summary of the findings from Schnadt et al 2010 could be included, particularly as this seems to be published in an internal report which may not be publically available. A few sentences should be sufficient to describe the overall level of agreement between the models in their abilities to reproduce the present day atmosphere (very wide spread of results or all in the same ball park) and in which model parameters the largest disagreements can be found (e.g. OH, NOx, etc).
Answer: The authors agree and have included a brief synthesis of the validation study of Schnadt et al. (2010) in the beginning of Section 3. The Schnadt et al. study only

compared the models to CO measurements, but comparisons with O_3 sondes have now been included in Appendix A in the revised manuscript.

Page 16812, Line 24: Here some information should be added how the LMDz GCM meteorology was brought in line with that of the CTMs, was there any nudging implemented? How long was the duration of each integration?
Answer: This information has now been added to the LMDz-INCA model description.

Page 16813, Line 28: A bit more detail on the chemistry treatment in MOCAGE would be helpful to make the information better comparable to the other models. It is apparent that each participating modelling group has composed its own section of text, each in their own style and in their own level of detail. A more systematic comparison would be of advantage. Most model properties (grid spacing, vertical extent, detail of chemistry, duration of integration, forcing, etc) could be synthesized in a table. Alternatively or in order to supplement this, a leading abstract with a description of features that are common to all models could be written and individual model differences ought to be listed afterwards, model by model.
Answer: A couple of sentences on the chemistry treatment in MOCAGE have now been added. The common model properties are described at the beginning of Section 3 and the individual model descriptions have been harmonized. A systematic comparison of model properties is given in Table 2 in the manuscript.

Page 16814, Line 5: "Scaling up the impacts" how much scaling is actually done here?
Answer: The sentence has been rephrased to clarify how the scaling was done (i.e. the impacts were scaled to 100% by multiplying the response of the 5% emission perturbation by 20).

Page 16815, Line 1: Figure 5, change axis labelling from "delta ppbv" and "delta ppbv, scaled" to "delta ppbv 5%" and "delta ppbv, scaled to 100%" in order to make the difference obvious and to avoid misunderstandings. The same should be done for all subsequent figures where applicable. Do the red lines refer to the red axis? If the red axis refers to all lines (which I presume) then it might not be any need to use red colour for this axis after labelling it clearly.
Answer: The authors agree and have changed the axis labelling in Figures 5-10. However, we have decided to keep the red color on the right/top axis because the red lines refer to the red axis only (the other lines refer to both axis). The captions of Figures 5, 6, 9 and 10 have been changed slightly to point this out.

Line 20: Replace "variability" with "difference"
Answer: This has been done.

Page 16820, Line 17: The method used in Hoor et al 2009 needs to be described.
Answer: The method used in Hoor et al. (2009) was described in the subsequent sentence, but this was poorly expressed in the original manuscript. The sentence has now been rephrased to avoid confusion.

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