

Referee Comments to Hodnebrog et al., ACPD, (2011)

General comments:

This paper presents findings from multi-model sensitivity studies comparing present day emission scenarios for shipping and aviation with projected future impacts of emissions from these transport sectors. The study can be seen as a complementary study for non land-based transport sector emissions to a previously published paper which looked specifically at impacts from land-based transport sector emissions.

The paper does not present new science in the sense that here a number of established atmospheric models has been used (even though these models may have undergone improvements and further developments) to carry out sensitivity studies based on perturbation to global transport sector emission inventories. Such types of studies have been carried out in the past and the appropriate papers are referenced in this work for comparison. This study is still very useful however as multi-model assessments in particular allow insights into the level of inter-model agreement and whether there are outliers that deserve further investigation. Specifically if the participating models have undergone improvements since the last assessment exercise and if they have been validated against observations this kind of impact assessments is of great value to the atmospheric science community. The scientific quality is very good and the presentation is, apart from a few instances noted below, very clear.

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.

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.

After these aspects have been addressed, together with the specific comments listed below, my recommendation is to accept this paper for publication in ACP.

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.

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.

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)?

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.

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.

Line 22: Cannot find van Aardenne et al, 2005 in reference listing.

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.

Line 23: Remove “the” before 12 new shipping routes.

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).

Page 16809, Line 8: Does this mean A1B HIGH experiments are not part of this study? Why was it mentioned then at all earlier?

Line 10 ff: It should be mentioned in the text that biogenic & soil emissions were kept constant for all years according to Table 1.

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?

Line 18 ff: Consider rephrasing this paragraph.

Why were new reference runs required for B1 ACARE? (Line 24)

Line 28: Should it not read “scaled” instead of “unscaled”? This is confusing. Scaled to what?

Line 25: Have the 5% scaling been applied to all emitted species or only to NO_x?

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.

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, NO_x, etc).

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?

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.

Page 16814, Line 5: “Scaling up the impacts” how much scaling is actually done here?

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.

Line 20: Replace “variability” with “difference”

Page 16820, Line 17: The method used in Hoor et al 2009 needs to be described.