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Interactive Comment

Interactive comment on "The global impact of the transport sectors on atmospheric aerosol in 2030 – Part 2: Aviation" *by* M. Righi et al.

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We are grateful to the reviewer for his/her constructive comments and suggestions which helped us to improve the manuscript. Please find below our replies (roman text) to the reviewer's comments (*italic* text).

Besides some clarifications that need to be done and that are mentioned later, my main concern is about how the emission scenarios were constructed and how they are connected to the RCP scenarios of IPCC. To me, and I would suppose that this will be the case for most readers, it is very surprising that BC and SO2 emissions from aviation increase most in the RCP2.6 scenario, which is the most optimistic

IPCC scenario in terms of lowest changes in radiative forcing. In addition, it is hard to understand why RCP6.0 globally shows the lowest increase in BC and SO2 emissions until 2030. I can follow the explanations that are given about the construction of the emissions between lines 11 and 23 on page 34042, however it seems to me that they were not constructed in a consistent way. While RCP 4.5 and RCP8.5 are based on older inventories developed in QUANTIFY, RCP 6.0 and RCP2.6 were constructed in a different way (and not the same way for both). Why couldn't you construct the RCP2.6 and RCP6.0 in the same way as it was done years ago for RCP 4.5 and RCP8.5? And if you have to construct two new scenarios, because they were not available in QUANTIFY, why don't you do this in a consistent way for both of them? In addition, the BC/NOx ratio is by far the highest for RCP2.6. You say this is the case, because the aviation share for BC in QUANTIFY is largely increasing between 2000 and 2030 (page 34042, lines 11 -23). How is this justified from a technological point of view? You claim that there won't be much technological development in the coming decades related to the aircraft turbines. So why is the BC/NOx ratio much higher in one of the scenarios compared to the others? In summary, I think that at least the naming of the scenarios following the RCP scenarios is misleading. Additionally, the way how they were constructed and why this was done needs far more justification than is available in the paper now. I would favor publication of the manuscript, but some major changes need to be done.

We fully agree with the reviewer here and share exactly the same concerns. We must however clarify that we are not the developers of these scenarios and we did not construct any emission inventory in the present study, nor in the companion papers. We have just applied the emission data generated by the developers of the RCP scenarios, as it has been done in many other studies (for example, Takemura et al., 2012; Rotstayn et al., 2013; Lamarque et al. 2011). Our goal is to quantify the impact of transport on aerosol and climate using a global aerosol model. As an input to our model simulations, we have used the CMIP5

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emission data for short-lived species (gas, aerosol and aerosol precursor species) and the future projections based on the RCP, as freely available on the web (e.g., at http://tntcat.iiasa.ac.at/RcpDb/dsd?Action=htmlpage&page=compare) and reported in the cited literature. The motivation for using the CMIP5 emissions and the RCP scenarios is that they were explicitly designed and developed for the IPCC and are therefore extremely relevant for the climate community, as demonstrated by the large amount of studies based on such scenarios. Before our series of papers (Righi et al. 2013, 2015 and the present study), a consistent analysis of the transport sectors under the RCPs was however lacking and this motivated our work.

While analysing and discussing the results for year 2030 we also spotted the inconsistencies raised by the reviewer and tried to address them in this (aviation) and in the Righi et al. 2015 paper (land-transport and shipping). We agree, for example, that the RCP naming is misleading when looking at the short-lived species, as they behave often in the opposite way than their number/ranking would suggest (see also Takemura, 2012 and Fiore et al., 2013). Based on the information available in the literature, we can consider RCP4.5 and RCP8.5 to be more reliable, since they are based on actual transport scenarios from QUANTIFY, whereas RCP2.6 and RCP6.0 were constructed based on very simplified scalings and questionable assumptions. We are still discussing the projections of all RCPs for completeness, but we have now added a statement in the paper (Sect. 2) to stress this important difference in the construction of the aviation projections: "Given these considerations, RCP4.5 and RCP8.5 can be regarded as more reliable concerning aviation emissions, since they are based on actual transport-oriented scenarios from QUANTIFY. The aviation projections in the other two RCPs are constructed on basic assumptions using very simplified scalings. The resulting projections shall therefore be interpreted with care. For completeness, in the present study we will discuss the results from all RCPs, but we will point out inconsistencies when appropriate."

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Title: I am aware of the fact that two papers were already published with the same main title. However, this study is on atmospheric aerosols and climate, which could have been mentioned in the title.

This is a good point, but as the reviewer says, we need to keep the title consistent. Please note, however, that aerosol and climate are mentioned in the running title.

page 34038, line 19-21: You mention short lived gases and aerosol precursors. NOx are important emissions from aviation and they are shown in Figure 2. I am missing a discussion on the effects of NOx on particle formation and ozone.

The quantification of ozone impacts is beyond the scope of the current study, which focuses on aerosol (and precursor species). It would also require a different model configuration, with a more detailed chemical mechanism, including, for example, a full representation of non-methane hydrocarbons and other important ozone precursors. For the present study, a simplified chemical setup has been adopted in order to reduce computational burden. This is acceptable for aerosol precursor chemistry, but does not allow a reliable assessment of ozone effects. The impact on NO_x on particle formation (nitrate) is included in the model and the results are discussed in the paper (Sect. 3 and Fig. 5). The aviation-induced RF effect reported here is however mostly driven by sulphur emissions, as revealed by a sensitivity experiment performed in Righi et al. (2013) with very low aviation fuel sulphur content. This sensitivity experiment is recalled in Sect. 4 of the present manuscript.

page 34040, line 22-26: If the RCP scenarios are not well suited for air quality projections, why are they used, here?

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As stated above and also in the companion paper (Righi et al., 2015), the RCP were designed in support of the IPCC and are therefore the reference climate scenarios in many IPCC-supporting studies. There is also a quite large number of aerosol studies under the RCPs (e.g., Bellouin et al., 2011; Lamarque et al., 2011; Chalmers et al.,2012; Fiore et al., 2012; Takemura, 2012; Lamarque et al., 2013; Rotstayn et al., 2013; Unger et al., 2013; Smith and Bond, 2014). Despite this extensive literature, a study focusing on the transport sectors under the RCPs was lacking, which motivated our series of studies closed by this manuscript.

page 34041, line 25: Why didn't you consider the introduction of low sulfur fuels in your scenarios? I would assume that this would reduce the indirect cooling quite substantially. Therefore it would be a very interesting case.

A low-sulfur case was analysed in a sensitivity study in the first paper (Righi et al., 2013). The sensitivity analyses of Righi et al. 2013 are not repeated for the future scenarios and only the reference case is considered here. This was mentioned in Sect. 4, but we recognized that it was not clear enough and we have extended it as follows: "In R13, we conducted two additional sensitivity simulations to quantify the uncertainties in the RF related to i) the assumption on the size distribution of emitted particles; and ii) the aviation fuel-sulfur content. The first was addressed in a simulation (NUC) where an additional nucleation mode for the emitted primary sulfate particles was considered, while a simulation (LOW) with a much lower fuel sulfur content (0.0052 instead of 0.8 $g(SO_2)\ kg_{fuel}^{-1}$) was performed to address the second point."

page 34042, lines 11 -23: see my comments above on the construction of the scenarios.

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We hope that the reviewer's concerns on this issue have been addressed in our replies above.

page 34057/34058: Why are the BC and SO4 concentrations from all other sectors (right column in Figures 3 and 4) so different in RCP6.0 compared to the other RCPs? Since this is your background and your reference for the aviation effects, you might need to briefly explain the reasons for the differences.

We agree with the reviewer that differences in the background could be important for the aviation effects and we have added a comment on that in Sect. 3: "We finally note that the changes in the background concentrations as induced by the other sectors (right column of Figs. 3–5) can be quite different among the RCPs. This has of course an impact on the background chemistry, especially for the secondary particles such as nitrate and sulfate. This means that the changes in aerosol concentrations discussed above are not always controlled by aircraft emissions only, but may also be due to the emission changes in the other sectors." However, the interpretation of such differences for each component and scenario analysed here is beyond the scope of the current work. We refer to the study by Takemura (2012) for a very comprehensive analysis of the aerosol loadings under the RCPs.

page 34044, line 12-13: sulfate should increase in the same way as BC in RCP2.6. Why isn't this noticeable?

Indeed it does, but it was not mentioned in the text. We have extended the discussion of the figure and we now point out that aviation-induced sulfate increases in all scenarios, in particular in RCP2.6: "Changes in aviation-induced aerosol sulfate (Fig. 4, middle column) range between 3 and 10 ng m^{-3} and are largest in RCP2.6,

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but hardly counteract the overall decrease driven by other sources (right column), which is typically around –30 to –100 ng m^{-3}."

page 34044, line 23-24: "it should be questioned whether the assumptions of high aviation emission shares in RCP2.6 are realistic RCP2.6 is unrealistically high": This is the point. Is there really a good reason why they were constructed this way? Then you need to explain it.

As we said in our first reply above, we now clearly state that RCP2.6 and RCP6.0 are based on questionable assumpions and therefore the corresponding results shall be interpreted with care. Since we were not responsible for the construction of these scenario, we can not explain the reasons behind these assumpions, but just report the information available in the literature, as we did in Sect. 2. We believe that RCP4.5 and RCP8.5 are reliable scenarios, since they are based on the QUANTIFY scenarios and are also used in a similar study on future impacts of aviation (Chen and Gettelman, 2016). As mentioned above, for completeness and for consistencies with the companion paper (R15) we wish to show the results for all RCPs, but we do point out inconsistencies where appropriate.

page 34044, line 25-27: Are there effects of NOx emissions on nitrate formation?

Yes, nitrate formation is controlled also by NO_x in the model. We have added this in Sect. 2.

page 34046, line 6 and Fig.8: It would be nice to have the numbers for the radiative forcing given somewhere. Some are mentioned in the abstract but not here.

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We added the numbers (for both all-sky and clear-sky RF) on top of each bar in Fig. 8. Thank you for your suggestion.

page 34047, line 8-9: "seems to support . . .": How do you see this in Fig. 6?

The reviewer is right, this was not clear from Fig. 6 only. We have revised this sentence with a better explanation: "The model results of Barrett et al. (2010, 2012), based on the GEOS-Chem model, show indeed a typical downward transport path for aviation-induced aerosol and aerosol precursors around 30°N, which is consistent with the pattern of aviation-induced changes in number concentrations simulated here (Fig. 6, middle column), hence supporting the latter mechanism."

page 34047, line 13-14: Are these numbers calculated by Unger et al. comparable to some of your numbers, e.g. to the clear sky RFs? How do they compare?

The results of Unger et al. are larger than our clear-sky values in Fig. 8. One should consider, however, that the clear-sky RF is only a proxy for the direct effect, since it does not include, for example, the effect of aerosol above clouds, which for aviation could be particularly important. We have added this to the text.

page 34047, line 13-14: " . . . with RCP2.6 being the most extreme one." Extreme in which way? Typically, one would expect that RCP2.6 is most extreme in emission reductions, here it is the other way round. This needs explanation.

As mentioned above, the RCP rankings refer to the long-lived species, whereas for the short-lived ones often the opposite behaviour is found.

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page 34048, line 15-17: "Future policies addressing the aviation sector should therefore focus on reducing its climate impact.": This is very general, could you be more specific? It looks like a sulfur reduction would reduce cooling, however, you would also reduce BC emissions which probably have warming effects? Can that be distinguished from your study?

As the reviewer correctly points out, the BC direct effect could become important in a low sulfur scenario. We have made this more evident in the conclusions ("The direct effect of BC could also become more relevant in such case"). In Righi et al. (2013), we run a sensitivity experiment with low sulfur but found no significant direct effect. This however could be also due to the compensation between BC warming and nitrate cooling. To isolate the BC direct effect additional sensitivity experiments would be required. Given the computational cost for running our global model, in this series of studies we put our focus on the cloud effects and performed a dedicated set of sensitivity experiments to better characterize it. Due to limited computational resources, additional simulations are unfortunately not possible at this stage.

page 34036, line 22-24: "more than doubled": 63 is more than four times 15. Maybe you add "in all scenarios" after "-15 mW-2".

Thank you for this suggestion. We have added that.

page 34037, line 7: "small fraction". I do not think that 2.6% is a small fraction for justone transport sector. This is in the order of all sectors from a big industrialized country like Germany.

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We have temperated this statement by writing “a relatively small fraction”.

page 34038, line 13-15: “simulate the aerosol cloud and aerosol radiation interactions”: please explain, here or somewhere else, which interactions are considered and which potentially important ones not.

We have extended this parts for clarity: “The model setup adopted in this study also includes aerosol-radiation and aerosol-cloud couplings (Lauer et al., 2007), which are essential for quantifying the aerosol impacts on climate. The first is realized by explicitly calculating aerosol optical properties on-line based on the Mie theory and using them to drive the radiation calculations (see also Pozzer et al., 2012). The latter follows the Abdul-Razzak and Ghan (2000) parameterization to simulate the number of activated cloud droplets as an input to the two-moment cloud scheme by Lohmann et al. (1999) and Lohmann (2002). This enables to track cloud particle number concentration and its aerosol-induced changes. It is important to mention that the current model setup does not include the representation of heterogeneous freezing process in ice clouds (this is intended to be the subject of a follow-up study).”

page 34040, line 15-16: Which aerosol quantities (number, mass, . . .) were represented “reasonably good”?

Both aerosol mass and number were evaluated. We have added this to the text.

page 34042, line 4: explain CMIP5

We have added it on the first occurrence of this acronym.

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page 34042, line 7-8: why are the relative changes only similar and not equal?

Because the scaling factor is altitude-dependent and the vertical distribution of the emission can slightly change between 2000 and 2030. We have added this information in the text for more clarity: "using the altitude-dependent ratio of the emission factors of the two components".

page 34046, line 11-13: rescaling means using the same percentages? Then you should write "same relative uncertainty" in lines 12-13.

Yes, this is correct. We have added it.

page 34049, line 21: aerosol number concentration, mass concentration or both?

We meant both and have fixed it accordingly.

page 34040, line 7: layers
page 34044, line 6: particles
page 34047, line 5: mechanisms

All fixed. Thank you for spotting them.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34035, 2015.