

Interactive comment on “Impacts of emission reductions on aerosol radiative effects” by J.-P. Pietikäinen et al.

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Pietikäinen et al. estimate changes in the aerosol radiative effect (direct+indirect) due to projected reductions in emission of SO₂, black carbon, and organic carbon, following four future scenarios. The authors discuss regional to global changes in atmospheric burden and radiation. The scenarios include current legislated and maximum feasible emission reductions.

While the results of this work are of great interest to the community, more detail is needed in some sections (as discussed below). The manuscript is generally well-written, but an edit by a native English speaker is recommended. This

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paper is within the scope of ACP and I believe it will meet its standards once the following comments are addressed.

We thank the reviewer for valuable comments for improving the manuscript. We have improved the English language of the manuscript. Throughout the text reviewers comments are marked with boldface and after each comment follows our reply.

General comments

The magnitude of emission changes in the four future scenarios is not specified, globally or regionally. A table of regional emission changes for each scenario would be useful for future comparisons and better understanding of changes in burden/radiative effect. In addition, it is not clear what emissions are being modified. Are CO, VOCs, NO_x, methane, etc. also being modified?

This was also pointed out by the referee 1 and hence the Table 1 has been modified to include all emissions species. Only the BC, OC and SO₂ emissions are included in this study.

The choice of 2005 as a reference year is an improvement over many studies that default to 2000. However, there have been significant emission-driven changes in aerosol distributions between 2005 and 2015. I recommend a couple sentences discussing the impacts of 2005 vs. 2015 as a reference year. Similarly, discussing changes between 2005 and 2020 as “future” changes is interesting since we’re already 2/3 of the way to 2020.

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There are some changes between 2005 and 2015, but in terms of BC and OC the changes are not very big. SO₂ is estimated to have changed slightly more, but not more than by 10-15% globally. Regionally, the SO₂ emissions seems to be decreasing in western countries and increasing in India and China. We have modified the end of first paragraph on Section 2.2.1: "...(FAO). We have used the 2005 as a reference year as emissions in this year have been well evaluated and the emissions do not change significantly between 2005 and 2015 for BC and OC (Granier et al., 2011). Over the same time period, SO₂ emissions have been estimated to slightly decrease globally (10-15%), although regionally, e.g. in India and China, the emissions may have increased (Klimont et al., 2013). For comparison of GAINS emissions against for example Representative Concentration Pathways (RCP), see Granier et al. (2011)."

The 2020 scenarios are very near-future estimates, but give valid information about the pathway to 2030. For example, from Tables 2 and 3 it can be seen (and is discussed in the manuscript) that some trends from 2005 to 2020 continue from 2020 to 2030, whereas some decrease or even change their sign.

Also, it would be interesting to know how close (or far) the 2005 to 2030 reductions are from pre-industrial levels. For example, are we at 50

Lamarque et al. 2010 (ACP, 10, 7017-7039) estimated global anthropogenic emissions in 1850's to be about 1 Tg, 5 Tg and 3 Tg for BC, OC and SO₂, respectively. These compare with our BCadd 2030 values 2.74 Tg, 4.97 Tg and 77.84 Tg for BC, OC and SO₂, respectively.

Comparing simulated burdens to Schulz et al. (2006) is great, but AeroCom simulated year 2000 conditions, which should differ from 2005. This should

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be mentioned. More recent multi-model intercomparisons include AeroCom II (Myhre et al., 2013) and AC- CMIP (Shindell et al., 2013). These intercomparisons also commonly default to 2000 as a "reference", but represent the most up to date multi-model estimates.

We made it more clear that Schulz et al. (2006) used 2000 conditions. Myhre et al., 2013 unfortunately only include the anthropogenic burden (and forcing), but from Shindell et al., 2013 we have included BC and SO₄ burdens. Based on the comments from referee 1, we also included a comparison of residence time of BC and SO₄ from Shindell et al., 2013. Moreover, all the results from other sources include the standard deviation (if it was given).

The calculation of the aerosol radiative effect needs a more thorough explanation. Nudging the model to ERA-Interim is fine, but then an effective radiative effect may not be particularly meaningful. However, it is unclear if the authors are estimating true radiative effect (parallel calls to radiation schemes) or the effective radiative effect (difference in TOA radiation between paired simulations). I believe it is the latter.

The aerosol radiative effects come from parallel calls of radiation scheme (with and without aerosols/clouds). These are then compared between different simulations.

We have changed the last sentence of the Chapter 3.2: "Additionally, the values given in the following sections refer to the top of the atmosphere and are obtained directly from the radiation scheme (parallel calls with and without aerosols/clouds). "

Terminology needs to be clearer as well. The direct radiative effect is presented

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in Fig. 5. This is different from radiative forcing and should be defined in the methods section, ideally when the forcing (or radiative effect) calculation algorithm is spelled out.

Also partly based on referee 1 comments, we have made the terminology more clear.

Changes in burden and DRE are compared to previous studies throughout this paper and it is rightly pointed out that comparisons are complicated by differing models, emissions, future scenarios, etc. But I think an important comparison would be to the more commonly used RCP scenarios, i.e., Shindell et al. (2013), which make different assumptions.

We have added comparison of burdens and aerosol residence time against Shindell et al. (2013), but as they use all-sky values for radiative forcing, comparison would not be straightforward and has not been added.

The authors may also be interested in comparing to a similar study by Kloster et al. (2008) using the same model.

A very good point. We have now added the comparison with Kloster et al. (2008) result in terms of burdens, residence times and radiative effects.

- The manuscript could be greatly improved with a more thorough discussion of the implications of the various scenarios for climate and air quality. Such an analysis, or a comparison of the four scenarios to the RCP scenarios (previous point), would strengthen the current paper.

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From the air quality point of view such an analysis would be interesting. However, in this paper we are focusing on forcing and related magnitudes. We only briefly discuss about air quality (mainly in terms of burden, which cannot be linked directly with surface concentrations, but gives indications how the atmospheric concentration overall change). For more information about the air quality implications, please see Anenberg et. al (2012).

We have also added two sentences telling where to find information about how the GAINS emissions historical comparison and how the scenarios compare against other pathways, such as RCP. We have added the following in the end of first paragraph on Section 2.2.1: "For comparison of GAINS emissions against for example Representative Concentration Pathways (RCP), see Granier et al. (2011)." and in the end of second paragraph: "More information about an overall emission scenario comparison can be found from Amann et al. (2013)."

Specific comments

The sign convention in the abstract and throughout the text is difficult to follow since aerosol RF is negative. Perhaps saying the magnitude decreases will provide clarity.

The text was thoroughly revised in this respect according to this comment and the comment by the other reviewer.

- In the abstract I am left wondering what the 2005 vs. pre-industrial RF is, i.e.,

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what percentage of the overall magnitude is a 0.06-0.4 W/m² decrease?

We have not calculated this. However, estimate from Kloster et al. (2008) is -0.82 W/m² (TOA clear-sky, 2000).

- The penultimate sentence of the abstract is a bit puzzling. Does this include air quality considerations or just climate effects?

In a way both, although air quality comes mainly from burden point of view. We have also analysed surface concentrations, for example, but this analysis is not shown here. Besides these, no further air quality analysis has been done.

Page 31901, line 3-6: But some models have been unable to simulate the magnitude of dimming with only aerosols [Wild et al., 2009; Koch et al., ; Leibensperger et al., 2012].

True, but the phenomenon still exists.

Page 31901, line 9: “Loosing” should be “losing”

Corrected as suggested.

Page 31902, line 10: “How these” should be “How do these”

Corrected as suggested.

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Page 31902, line 12: “To what extent these patterns can be influenced by...” should be “To what extent are these patterns influenced by...”

Corrected as suggested.

Page 31904: Is the difference, in terms of aerosols, between CLEC2020 and CLECC2020 the additional reductions in aerosols occurring because of decreases in co-emitted precursors? A sentence should be added here to clarify since only CO₂ is currently referenced.

We have added: “scenario relies on the 2 °C (450 ppm) energy scenario developed by IEA (IEA, 2009). The main reductions in aerosol species between CLEC and CLECC occur in the residential, transport, energy and industry sectors and are the result of shifts away from the use fossil fuels as well as improvements in energy efficiency (IEA, 2009). In addition, two more...”

Page 31910, lines 6: Redundant word “models models” and “resorting to” should likely be replaced with “using”

Corrected as suggested.

Page 31916, line 14: Also see IPCC AR5 for more recent DRE estimates.

We are using here clear-sky diagnostics which is not the case with AR5 report. This is why the comparison has been omitted.

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Page 31917, last paragraph: This is a very interesting paragraph, but I am having difficulty following the logic. I suggest a bit of editing.

We have edited the paragraph to make it more logical.

Figure captions: Acronyms (e.g., SA) should be spelled out

This was also mentioned by referee 1 and has been now corrected.

References:

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