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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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Review of "Impacts of emission reductions on aerosol radiative effects" by Pietikäinen *et al.*, submitted to *Atmos. Chem. Phys.*

The paper describes experiments with the ECHAM-HAM aerosol-climate model where four emission scenarios of the year 2030, representing increasingly efficient emission reduction measures, are used to assess their impact on aerosol burden and radiative forcing compared to the year 2005. The results are interesting and the link between emission and burden changes is well made (section 3.1).

The paper has weaknesses however. The emission scenarios, which are central to the study, need to be better described, both qualitatively and quantitatively. The writing should be more rigorous, and references to previous studies should be accompanied

with a quick summary of the relevant finding. Figures and their captions can be improved. Finally, although the level of English language is good, minor improvements will be required by a native speaker.

For those reasons, I recommend publication after the following comments are addressed, which should amount to moderate to major revisions.

1 Main comments

- The use of four emission scenarios to 2030 is a strength of the paper. Unfortunately, section 2.2.1 does a rather poor job at describing those scenarios, as it assumes that the reader is familiar with many scenarios, projections, and legislation. To improve the situation, the authors must:
  - Show a Table similar to Table 2, but for emission rates of aerosol and precursor species in the reference dataset, and how those change in the four scenarios. That Table will help the reader determine the size of the different emission reductions considered.
  - Are the aerosol emissions the only thing that changes in the perturbed simulation? It sounds like the CLECC simulations also include changes in CO_2, and also other climate forcers (methane?).
  - Why is the BCAdd scenario called like that if it targets short-lived climate forcers in general? What are those "most important measures" (31904, line 22) that are included, and the "principles of such scenario" (31904, line 23)? It must be possible to summarise the key points of UNEP (2011) and Shindell et al. (2012) in a couple of sentences.
  - Same remark for the MTFR scenario, but for Cofala et al. (2007) and Klimont et al. (2009). Those "end-of-pipe measures" are quite mysterious.

  - Table 1 needs to be extended to include emissions discussed in 2.2.2, and 2.2.3. In the present version of the paper, the reader has no idea of the size of aviation and biomass-burning emissions.

- Figures 2, 3, 4, 5, 6, and supplementary Figures: please give the global average alongside each panel. Please also avoid using acronyms in the captions: all Figures should be stand-alone. Figures S1, S2, and S3 should show differences in emissions compared to the 2005 reference: at the moment, it is difficult to determine precisely where emissions have changed, and whether aerosol burden changes (Figures 2, 3, 4) are consistent.

- The last paragraphs of sections 3.1.1 (31910, lines 3–12), 3.1.2 (31911, lines 21–29), and 3.1.3 (31912, line 27 to 31913 line 4) should be moved to section 2.1, because they suggest that the reference simulation is in line with previous model runs. Of course, I am sure that the authors are aware that such a comparison is a poor measure of skill: previous simulations are biased against observations in diverse ways. The section should also mention aerosol residence times for the three species studied in the paper. Residence times are key to understand aerosol transport and radiative effects, and how they differ among models.

- Page 31916, lines 4–5: This is a big surprise, and that limitation should have been mentioned earlier, including in the abstract. Why not give the all-sky DRE? That should be straightforward in a climate model.

2 Other comments

- Abstract, line 3: The authors' use of the terms "radiative effect" and "radiative forcing" is inconsistent. The title uses the former, the abstract and the rest of the paper use both without a clear logic. I recommend using a consistent convention

throughout. The IPCC terminology could be used: "radiative effect" refers to the contribution of aerosols in general to the radiative budget, while "radiative forcing" is reserved to anthropogenic aerosols, or for changes with respect to a reference state. Under that definition, "radiative effect" would be used for 2005 reference numbers, while "radiative forcing" would be reserved to changes with respect to that reference. Changes are needed throughout the paper, including Table and Figure captions. A good example of the confusion are lines 21–23 of page 31915: the first sentence is indeed a definition of the direct radiative effect, but the second is in fact a definition of the direct radiative forcing (see e.g. Myhre *et al.*, *Atmos. Chem. Phys.*, 2013).

- Abstract, line 9: A good way to summarise the results of the study is to remark that burden changes, and consequently radiative forcings, basically follow changes in primary and precursor emissions. Of course, to have a more complete assessment, one would need to include interactive chemistry (to account for possible changes in aerosol oxidants, e.g. Rae *et al.* [2007]) and consider the impact of climate change on atmospheric circulation.

- Abstract, lines 12–13: "The global values": Which global values? The DRE? And the "lowest" is ambiguous, as DRE is negative. "Weakest" is probably a better word.

- Abstract, line 13: "The cloud radiative effect": Again, the wording needs to be more accurate, as "cloud radiative effect" has a specific meaning (contribution of clouds to the radiative budget) which is probably not what the authors mean. Here, I guess the authors mean "aerosol indirect radiative effect" (or forcing, see above), since it is a shortwave effect (31919, line 11).

- Page 31901, line 4: "global dimming" is not an "enhanced aerosol cooling effect". It is the reduction of shortwave radiation reaching the surface caused by increases in aerosol loading. It may lead to a cooling.

- Page 31902, lines 1–2: “the role of different regions in these effects” is unclear. Do the authors mean the contribution of emissions from different regions to global aerosol radiative effects?
- Page 31902: Is the model an atmosphere-ocean coupled model? If not, does it matter, and how are sea-surface temperature fields prescribed?
- Page 31902, lines 20-21: HAM and M7 are not two different components of ECHAM. Rather, HAM is an implementation of the M7 framework.
- Page 31903, line 3: “stratiform cloud scheme”. So there is no aerosol indirect effects on clouds other than stratiform?
- Page 31903, lines 6-16: The evaluation studies need to be summarised in more useful details than just saying that the model is “realistic”. For example, Zhang et al. (2012) lists in its abstract important deficiencies: “*(i) positive biases in AOD over the ocean, (ii) negative biases in AOD and aerosol mass concentration in high-latitude regions, and (iii) negative biases in particle number concentration, especially that of the Aitken mode, in the lower troposphere in heavily polluted regions.*” Those deficiencies (and those identified by the other studies) likely have an impact on the results discussed here, so it is important that they are stated clearly.
- Page 31905, lines 8-9: What are those “other SO2 emissions not covered separately”?
- Page 31906, line 14: “same approach as was used by Dentener et al.”: please specify which approach you are talking about.
- Page 31908, line 22: The traffic sector doubles in CLEC2030, but not in the other scenarios? Why not? Pollution growth is presumably similar in all scenarios.

- Page 31908, line 23: I’m not sure why this fact is “noteworthy”. Would the different measures included in the four scenarios impact traffic emissions more than domestic emissions?
- Page 31908, line 25: BC burden decreases everywhere in those two scenarios. Why mention Eastern China specifically? It should be mentioned in the next paragraph.
- Page 31909, line 15: “due to atmospheric transport” – this implies that changes in transport to the South Hemisphere are dominated by changes made over India. Is that expected? How then to explain the increase in BC burden over Greenland in CLEC2030?
- Page 31909, lines 26–27: This sentence is ambiguous, as it suggests that BCAdd includes MTFR measures. What the authors mean is that the additional MTFR measures have only a small impact on BC emissions.
- Page 31910, line 21: This is the first time we hear of significant natural emissions of organic aerosols in the model. Wildfire emissions would not dominate in the North Hemisphere, so where do they come from? Biogenic processes? Those emissions need to be mentioned (including their annual rates) in section 2.1.
- Page 31912, lines 7–9: Interesting statement here on the impact of solar radiation on the distribution of sulphate aerosol burden. I would have expected atmospheric transport to compensate for that effect.
- Page 31912, lines 25–26: Duly noted, but it would be more helpful to tell the reader why changes are so small over China, for example.
- Page 31915, lines 12-19: I think this paragraph is more confusing than helpful. Basically, positive changes in DRE (a positive forcing) would translate into a weaker cooling by aerosols. However, it is improper to say that the difference

plots show “change in the cooling” – if that were really the case, they would be in units of temperature. The authors should refrain from using “cooling” as a synonym for “DRE” (31916, line 17; 31916, line 20, and so on...). They are different concepts: DRE is the trigger, which is quantified by the authors, cooling is the response, which is not quantified in the study.

- Page 31917, line 28: Note that this is not happening over India only: the competition between the opposite sign of BC and sulphate DREs happens everywhere, but it is particularly obvious over India in those simulations.
- Last paragraph of Conclusion, page 31924: This paragraph should mention the limitations of the studies, in particular that changes in atmospheric chemistry and atmospheric circulation are not included, and would affect the results. The lack of nitrate aerosols in the model is also an important limitation, as decreases in sulphate aerosol formation can favour nitrate formation and compensate for the change in sulphate aerosol radiative forcing.

3 References

Rae, J.G.L., C.E. Johnson, N. Bellouin, O. Boucher, J.M. Haywood, and A. Jones. Sensitivity of global sulphate aerosol production to changes in oxidant concentrations and climate. *J. Geophys. Res.*, **112**, D10312, doi:10.1029/2006JD007826, 2007.

Myhre, G. *et al.*. Radiative forcing of the direct aerosol effect from AeroCom Phase II simulations. *Atmos. Chem. Phys.*, **13**, 1853-1877, doi:10.5194/acp-13-1853-2013, 2013.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 31899, 2014.