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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.

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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.

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

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.**

  The Table requested has been made based on old Table 1.

– **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?).

In our simulations, only BC, OC and SO_2 are changing as we only concentrate on aerosol forcing. It is true that the scenarios themselves include more changing species (e.g. CO_2).

– **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.**

We have modified the second paragraph of Section 2.2.1: "..details of such scenario has been described in UNEP (2011) and Shindell et al. (2012). In short, the principles behind the development of the BCAdd scenario are a selection of measures which result in net reduction of radiative forcing calculated using pollutant-specific Global Warming Potential (GWP) values (UNEP , 2011). The measures reduce the emissions of BC, but also OC, carbon monoxide (CO), non-methane volatile organic compounds (NMVOC) and nitrogen oxides (NOx), and the reduced amounts vary across the measures. Key air pollutant measures include advanced emission standards on diesel engines (including diesel particulate filters), clean cookstoves, pellet stoves and boilers, more efficient brick kilns, and ban of agricultural burning. Thus, in terms of species used here, the reductions target BC and OC emissions. Measures with a relatively small net impact or increase in radiative forcing have been excluded from this portfolio. Lastly, the maximum technically feasible reduction (MTFR) scenario implements the maximum reduction potential of anthropogenic aerosol and SO2 emissions with currently available technologies by the year 2030 (simulation

MTFR2030). The MTFR scenario introduces the best available technology to a maximum extend while ignoring any potential economic and political barriers. In this scenario, no consideration is given to the direction of the change in aerosol radiative forcing, so also measures that reduce strongly the emissions of SO2 , e.g., fuel gas desulphurization, are included. The emission model used includes the end of pipe measures that remove pollutants from the exhaust. This means that it assumes that the use of most advanced particulate filters will reduce emissions of primary particular matter (PM), selective catalytic reduction (SCR) installations will bring NOx emissions down from industrial boilers, etc. For more detailed description of the current legislation and the MTFR scenarios, see e.g. Cofala et al. (2007) and Klimont et al. (2009). More information about an overall emission scenario comparison can be found from Amann et al. (2013)."

– **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.**

Please see previous answer.

– **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.**

These are now in Table 1.

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.

We have changed all the figures as requested.

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.

An interesting suggestion, but as Section 2 concentrates more on tools and methods, we do not see the point of moving the comparison part from the results sections to Section 2. This would require some initial explanation of the simulations and we believe that the current structure is clear already as it is. The comparison we show should not be taken as an detailed evaluation of the model as this was partly done before in Henriksson et. al. 2014 (this information has been added to the text in Section 2.1) and partly by ourselves (not shown).

Including the information about residence times is an excellent suggestion. We have calculated it for all of the species and simulations. Now chapters 3.1.1-3.1.3 include information about the residence time of the reference simulations and comparison to

previous studies.

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.

In this work, the clear sky approach for DRE was chosen, because we wanted to show how the aerosol changes translate into overall radiative effect. Including the clouds, changes in cloudiness would change the total DRE estimates. Although the purpose here is to show how the radiative effect are changing in current day climate conditions, all-sky DRE would be partly "twisted" for the scenario simulations due to clouds. Thus, the clear-sky was considered to give more information about the future DRE changes (it is quite commonly used). Nevertheless, we will add the information about using clear-sky values to the abstract.

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).

We have checked and corrected the manuscript for any inconsistencies related to this comment.

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.

We have modified the abstract: "ased on our results, aerosol burdens show an overall decreasing trend as they basically follow the changes in primary and precursor emissions. However, in some locations, such as India, the burdens could increase significantly..."

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.

Changed to: "The global changes in the DRE depends on the scenario and are smallest in...

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).

Changed to: "aerosol indirect radiative effect"

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.

Changed to: "i.e. the reduction of shortwave radiation reaching the surface"

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?

Yes, we mean the emissions. We have changed this to: "...the direct and indirect aerosol effects, the role of different world regions' emissions in these effects, and contrasting emission changes reflecting alternative emission control strategies."

Page 31902: Is the model an atmosphere-ocean coupled model? If not, does it matter, and how are sea-surface temperature fields prescribed?

No, it is not coupled. In our approach (using current day climate conditions), this does not matter as the sea surface temperatures were taken from the Atmospheric Model Intercomparison Project (AMIP II). We have added this information to Section 2.3. When prescribed SSTs are used, the aerosol influence is somewhat included in the forcing data and does not change even if the aerosol concentrations would change. As mentioned, we wanted to examine the burdens and forcing in current day climate conditions so the prescribed SSTs are not a problem.

Page 31902, lines 20-21: HAM and M7 are not two different components of ECHAM. Rather, HAM is an implementation of the M7 framework.

Indeed a bit confusing, now: "... (Zhang et al., 2012). This model version has the HAM aerosol module (Stier et al., 2005), which includes the M7 aerosol microphysical module by Vignati et al. (2004). ECHAM-HAMMOZ..."

Page 31903, line 3: "stratiform cloud scheme". So there is no aerosol indirect effects on clouds other than stratiform?

Ice phase clouds are also influenced. Changed to: "...large scale cloud scheme (no influence on convective microphysics)..."

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.

We have added the deficiencies reported by Zhang et. al 2012. The model version used here is exactly the same as in Zhang et. al 2012 so the deficiencies noted there are overall a good summary.

Page 31905, lines 8-9: What are those "other SO_2 emissions not covered separately"?

There are mostly industrial sources that are not included in the industrial sector. This information has been added to the manuscript.

Page 31906, line 14: "same approach as was used by Dentener et al.": please specify which approach you are talking about.

We have added: "In this approach, based on location and type, the emissions are divided into six altitude regimes: 0-100 m, 100-500 m, 0.5-1 km, 1-2 km, 2-3 km and 3-6 km."

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.

The activity scenario underlying the growth of number of cars (fuel consumption), etc., is the same for all included emission scenarios. However, the penetration of control measures varies significantly. The CLEC/C includes current legislation and so after the current fleet is replaced with new vehicles complying with existing standards, emissions start to grow proportionally to activity growth. In BCAdd and MTFR scenarios, more stringent controls are introduced everywhere, e.g., diesel particulate filters, and so emissions of several of pollutants will decline compared to the baseline CLEC/C.

We have modified the sentence: "Over India, the increase comes mainly from the traffic sector, which approximately doubles in CLEC2030. Even though the CLEC scenario includes current legislation measures, i.e. after some time new vehicles complying with existing standards will be in use, emissions start eventually to grow proportionally to the activity growth."

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?

Although the increase over India comes from the traffic sector (which almost doubles in CLEC2030), the biggest source sector is still domestic one. This should be mentioned to show the potential for further emission modifications.

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.

It explains the decreases mentioned for these regions already in the lines 20 and 21.

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?

As was commented before, the emission maps in supplementary material were not very informative for the scenarios and have been now updated. From the new maps, it is obvious that the emissions overall increase in Southern Hemisphere and the atmospheric transport increases the area of influence (as can be seen from Fig. 2). However, you point out Greenland, which has slightly increased burden in CLEC2030. As CLECC2030 does not show any increase in BC burden over Greenland, changes in shipping emissions does not cause the difference. The reason here is the increased emissions around India, which are higher in CLEC2030. These emissions cause increased BC concentration at higher altitudes (lifting) which are eventually transported to the Arctic area. Based on our analysis (not shown), the lower tropospheric burden change in CLEC/CLECC2030 compared to reference run is negative, but the transport to higher altitudes make the overall change of burden positive in CLEC2030. This pathway for BC transport to Arctic regions was also reported by Stohl. et al (2006).

Based on this comment, we have modified the paragraph: "...border area of Indonesia and Papua New Guinea. There changes are caused by the overall emission increases over land areas in the Southern Hemisphere, as can be seen in Fig. S1. Partly due to atmospheric transport from continental areas and partly due to increased shipping emissions, the BC burden also increases over Antarctica as well as over most oceanic regions in the Southern Hemisphere. Although the absolute BC values in these regions are low, the increased burdens could lead to changes in the surface albedo over snowy and sea ice covered areas. In the CLEC2030 scenario, the burden also increases over the Arctic region. This is due to transport coming from southeastern Asia (around India), where the increased emissions cause increased values of BC at higher altitudes (lifting) which are eventually transported to the Arctic regions. In our analysis (details not shown here, but for more information, please visit http://www.maceb.fi/result_viewer.html), we found that the lower tropospheric BC burden decreases in CLEC and CLECC over the Arctic, but the transported BC from southeastern Asia makes the overall burden change quite small, or even positive in the case of CLEC2030. A similar pathway for upper tropospheric Arctic BC from southeastern Asia has been discussed already in a previous study by Stohl (2006). In any case, since the albedo change due to BC deposition is not included in the current model version, further investigation concerning BC effects on snowy regions is left for future studies.

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.

Changed to: "The differences between the burdens in these two scenarios are quite modest also on regional scales (Table 2), which means that the targeted sectors (transport and especially residential combustion) in BCAdd include most of the reduction potential of BC and very little further reductions can be obtained with additional technological measures (as in MTFR)."

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.

The text in the original manuscript was unclear, so it was changed to: "...reference run

than the BC burdens (Figures 2 and 3). The main reason for this is that the current legislation measures do not have a major impact on domestic and agricultural sectors, which are two biggest sectors emitting OC (domestic is 5 times bigger than agricultural sector). This, together with unperturbed natural emissions, diminishes the differences seen in Fig. 3. On the other hand, the domestic sector will change quite dramatically (down to one fifth of the reference) in the BCAdd and MTFR scenarios, which mainly explains the larger differences in the OA burden for these scenarios. Furthermore, the difference between BCAdd and MTFR can be explained by the agricultural sector, which, as was mentioned before, does not include any emissions in MTFR."

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.

This sentence was not correct and has been changed to: "The latitudinal dependence of the burden over the continents follows directly the emission pattern (Fig. S3)."

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.

Actually, as BCAdd targets mostly BC and OC, it does not include much additional reductions when compared to CLEC2030 SA burden (CLEC is the baseline of SO_2 in BCAdd). Therefore, this sentence is not relevant and was removed.

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.

Thank you for this comment. Based on it, we have changed this part to: "As the radiative effects presented in the following sections are mostly negative, i.e. they have a cooling effect, positive changes in radiative effects translate into a weaker cooling by aerosols, and vice versa."

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.

This is true. Based on this comment, we added a sentence to the end of the paragraph: "Naturally, the same counteracting effects from absorbing BC and scattering sulphate can occur in other locations, but is particularly obvious over India in our 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.

We changed to first sentence to: "Our simulations predict a notable positive radiative

forcing change in the current day climate conditions..." and added to the end of the paragraph: "Moreover, the use of coupled aerosol-chemistry models with more detailed aerosol description (e.g. including nitrates) would give more detailed estimates of the future forcing of aerosols."

References:

Henriksson, S. V., Pietikäinen, J.-P., Hyvärinen, A.-P., Räisänen, P., Kupiainen, K., Tonttila, J., Hooda, R., Lihavainen, H., O'Donnell, D., Backman, L., Klimont, Z., and Laaksonen, A.: Spatial distributions and seasonal cycles of aerosol climate effects in India seen in a global climateâ€̀aerosol model, Atmos. Chem. Phys., 14, 10177-10192, doi:10.5194/acp-14-10177-2014, 2014.

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