Atmos. Chem. Phys. Discuss., 12, C12077–C12087, 2013
www.atmos-chem-phys-discuss.net/12/C12077/2013/

Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Evaluation of preindustrial to present-day black carbon and its albedo forcing from ACCMIP (Atmospheric Chemistry and Climate Model Intercomparison Project)" *by* Y. H. Lee et al.

Y. H. Lee et al.

yunha.lee.00@gmail.com

Received and published: 28 January 2013

We thank the reviewer for their constructive comments and suggestions and have made several changes to the paper to address the issues raised. Reviewers' comments are shown in italics with our response shown after each.

General comments

Lee et al. present a study analysing and evaluating the simulations of the ACCMIP *with respect to black carbon. Overall, this is a well-done model intercomparison and evaluation study. The evaluation using various data sources goes as deep as possible in the limits of one paper investigating several models, and the conclusions include a discussion of the resulting black carbon – albedo forcing and its uncertainty range. The manuscript is generally well written, with a good choice of Tables and Figures. My main suggestion is to include the statistical model-data comparisons now tabulated in detail in the Supplementary Material in a summarised form in the main manuscript.*

Specific comments

Title: I suggest to put the full name in the title and the acronym in brackets: "...from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP)"

Response: Changed as suggested.

p21617 l18: This is a possible definition, but its of course not unique.

Response: The definition is rewritten as follows; new part is shown in bold.
"Black carbon (BC) is **defined here as** the light-absorbing portion of carbonaceous aerosols" is changed to "the light-absorbing **and refractory** portion of carbonaceous aerosols".

p21717 l10: Sentence flawed

Response: The sentence is rewritten as follows.
OLD: "The BC albedo effect can be especially important because not only does it darken the surface but can also this cause a further warming as it initiates the snow albedo feedback by promoting snow melting."

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NEW: “The BC albedo effect can darken the snow/ice surface and cause a further warming via the snow albedo feedback.”

p21718 l11: Is there a specification missing for the factor 7?

Response: We realized our mistake on this reference. Reading the Watson et al. [2005] carefully, a factor of 7 is a variation in the EC measurements by the different analysis methods rather than the difference in BC and EC measurement. So we deleted the reference from the revised manuscript.

p21720 l13: consist l15: Does neither year show a strong El Niño / La Niña? Or does this not matter? l19: But the SST/SIC was repeated as the fixed year?

Response: The SST/SIC used in each timeslice is the decadal means from the CMIP5 simulations. Therefore, a strong ENSO event is unlikely to appear in our ACCMIP simulations. The following sentence in Section 2 is rewritten for the clarity.
OLD: “. . . driven by monthly mean sea-surface temperatures and sea-ice coverage either from observations or from the corresponding coupled ocean-atmosphere model integrations . . .”

NEW: “. . . driven by monthly sea-surface temperatures and sea-ice coverage that are averaged over 10 yr from the corresponding coupled ocean-atmosphere model integrations . . .”

p21721 l13: That is, in the lowermost layer? Which two models inject higher?

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Response:“the surface layer” is replaced with “the lowermost layer” and the two models are listed now.
OLD: “. . . biomass burning emissions above the surface layer, and two models inject. . .”

NEW: “. . . biomass burning emissions above the lowermost layer, and two models (GFDL-AM3 and NCAR-CAM5.1) inject. . .”

p21722 l22: “incorporated new information” l23: “apply”

Response: Corrected.

p21724 l10: and down to which depth for layer 4?

Response: We found that the model description on the snow layers was incorrect. The actual depth of each layer depends on the number of snow layers in the grid cell. The depth range provided for each layer is what we used for the evaluation – when the top depth of the snow sample fell in the range, we used the BC snow concentration in the corresponding layer. We revised the Section 3 with correct information, which is shown below.

“The CLM4 can have 5 snow layers at maximum, and the depth of each layer depends on the number of snow layers in the grid cell. For BC snow concentration evaluation, a constant depth is assumed for each model snow layer, shown in the parenthesis after each layer in the following: 1st layer (top surface to 2 cm), 2nd layer (2 cm to 7 cm), 3rd layer (7 cm to 18 cm), and 4th layer (deeper than 18 cm); the 5th (i.e., deepest) snow layer is not used. When the top depth of the snow sample falls in the range, we

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selected the corresponding layer for the evaluation. The sea-ice simulations applied the Community Ice CodE 4 (CICE4), using interannually varying atmospheric forcing data from different sources. The CICE4 model has two snow layers. The depth of the top snow layer depends on the total snow depth and can be as deep as 4 cm. For the evaluation, the 1st layer is assumed to extend to 2 cm and the 2nd layer includes snow deeper than 2 cm. Sensitivity studies based on the CICE model indicate that the assumption of fixed layer depths could alter the evaluation by at most several percent for the snow observations over ice surface; the fixed layer assumption could affect the evaluation more for the snow observations over the land surfaces."

p21725 l24: why is then still the ratio between dry and wet deposition in HadGEM2 the same as in most other models?

Response: In HadGEM2, wet deposition is still a dominant removal process than dry deposition. As we mentioned in Section 2, Line 24-26, BC particles from fossil fuel emissions still undergo wet deposition via diffusion scavenging. We introduce new sentence (in bold) for clarification.

"... slow wet scavenging **- they can not be removed via nucleation scavenging but still experience wet deposition via the collection by cloud droplets via diffusion and relative sedimentation between aerosol particles and precipitating droplets.** "

p21726 l2: Why not "Textor et al.", which would be the common formulation? l15: It is very unlikely that the precipitation characteristics are so different between a nudged and non-nudged model version, so this explanation does not seem credible.

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Response:l2 – Textor is changed to TXT06.
Response:l15 –We deleted “precipitation in particular”.

p21727 l1: From Fig. 3, it rather seems 1 or higher. l7: Rather, the column burden MMM shown in Fig. 3a l12: “Relatively large”: I think it really is surprisingly low compared to the large RSD in the burden. This should be discussed rather than the slight - and expected – difference in deposition RSD compared to the source regions.

Response:l1 and l7 – Corrected.
Response:l12 – The large RSD in the burden reflects the diversity of the BC removal parameterization, which determines the lifetime. Deposition rates are determined by the source strength (i.e. emission and net changes by advection) under the steady state assumption and are insensitive to the removal parameterization. We would like to make it clear that the large RSD in the burden is unlikely resulted from the burden present in the column aloft because of the wide spread shown in the Arctic BC surface mass concentrations among models.

p21730 l4: It would be better to convert the Table S1 into a figure and present it in the main text. It might be useful to summarize the full data to show just the average correlation coefficient, LMNB and LMNE for each model, for the Arctic, European and North American stations as discussed in the text. l25: “Unlike BC”: It seems that in fact the BC seasonality is not very different among the three stations either. Or does this statement rather refers to the model skills? If so, this needs to be explained better. Also the phase difference between models and observations needs explanation.

Response:l4 – Table 5 was added into the revised manuscript, which summarized the data in Table S1 to LMNB and LMNE for each model and for the Arctic, European,

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and North American regions. Table S1 will be retained in the supplementary materials.

Response:l25- The sentence is rewritten.
OLD: "Unlike BC, the CO seasonality is very similar among three stations and is fairly good even with some underpredictions during the late winter and early spring (see Fig. 8)."

NEW: "Unlike BC, the model CO seasonality is very similar among three stations and is fairly good compared to the measurements even with some underpredictions during the late winter and early spring (see Fig. 8)."

p21733 l6: As before, I think the average LMNB/LMNE should be reported in the main text. l8: ,.

Response:L6 - S-Table 2 is now presented as Table 6 in the revised manuscript.
Response:L8 - corrected.

p21734 l11: A central conclusion before has been that the transport to the Arctic is a main uncertainty. It comes thus at little surprise that the meteorology matters. It would be good to provide a deeper analysis of the differences in the two choices for the driving meteorology.

Response: The meteorology we referred to here is that used in the offline NCAR CLM4 and CICE4 simulations, which do not determine the spatial/temporal distributions of atmospheric BC transport and deposition. The reanalysis meteorology in the offline simulations can, however, affect the evaluation results by providing different

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cryospheric conditions (e.g., snowfall). We revised two parts in Section 4 into the following (the new parts are shown as bold):
"**However, we found that** model BC snow concentrations decreased somewhat when using 1996-2000 meteorology compared with 2000-2008, especially over Russia and the Arctic Ocean, **which is mainly due to the differences in snow/ice conditions between two periods.**"

"This suggests that the Arctic BC snow concentrations are not insensitive to the choice of the meteorology period applied **in the offline simulations**, especially over the Arctic Ocean and Russia, or to interannual variations in BC emissions."//

p21736 l3: The sentence needs to be completed.

Response: Changed as follows:
OLD: "Model overpredictions of BC snow concentrations at Law Dome are less than for BC deposition fluxes because of overprediction of precipitation (see Fig. 12)."

NEW: "At Law Dome, most models overpredict BC deposition fluxes more than snow concentrations because of overpredicted precipitation (see Fig. 13)."

p21745 l3: compared

Response: Corrected.

p21761 Table 3: The unit for the emissions is also "Tg yr-1", I believe? Why are emissions and wet+dry deposition not balanced?

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Response: The unit is corrected. About the gap between emissions and total depositions, we added the following sentences in Section 4.1 to explain the discrepancy and also in the supplementary materials for the source of the gap in each model.

In Section 4.1,

"Some models in Table 3 show the imbalances between the emission and total deposition rates seem to originate from a minor diagnostics problem or a minor undiagnosed term in deposition processes (see the supplementary materials for details). We believe this does not disqualify the models from the study."

In the supplementary materials,

"Five ACCMIP models show non-negligible imbalances between the emission and total deposition rates (see Table 3). Here we describe what we believe to be the source of discrepancy in each model. For GISS-E2-R and GISS-E2-R-TOMAS, the gap is due to a diagnostic error in wet deposition by large-scale clouds. One of the equations to determine tracer evaporation from cloud water was analytically correct, but the resulting value from the equation was slightly off due to inadequate numerical precision of equation terms (e.g., A- B = A when A »B). In the case of GFDL-AM3, the wet deposition from large-scale clouds had an opposite sign to that for convective clouds. This was not accounted for when the total deposition was calculated so that in columns with both large-scale and convective clouds the terms tended to cancel. For HadGEM2, the discrepancy is very likely due to missing a minor process when compiling the wet and dry BC deposition fields for a separate biomass-burning tracer. Finally, the discrepancy in MIROC-CHEM is under investigation."

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p21766 Fig. 3: The color scale for the RSD of the BC column burden is saturated throughout the Arctic region. How far does the RSD exceed 1? It may be useful to show an Arctic projection (such as for the deposition fluxes) also for the burden.

Response: The BC column burden exceeds 1 and we adjusted the color-scale in the burden map to show the RSD distributions over the Arctic areas. Also the Arctic-focused map for the burden is now shown in Figure 3. While doing this, we found a mistake that the left-bottom color-scale in Figure 3 is only for the deposition fluxes and the one for burden was missing in the discussion paper.

p21767 Fig. 4: It would be better to use a balanced color scale (i.e., 2 corresponds to 1/2, 3 to 1/3 etc.).

Response: We tried several color-scales but the current color-scale works best to show the differences.

p21768 Fig. 5: The reader should be reminded that for 1930, GFDL and HadGEM2 are not included. For color scale, see above.

Response: The following sentence is added into the caption.
"GFDL-AM3 and HadGEM2 did not run the 1930 timeslice."

p21770 Fig. 7: Since anyway a logarithmic scale is used, it seems preferable to use the same units (ng m-3) for all panels and extend the scale to 6000 nm.

Response: Several color scales were attempted but they didn't show the model spread

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so well. The current one works best to show the model spread.

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