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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraints on aerosol processes in climate models from vertically-resolved aircraft observations of black carbon" *by* Z. Kipling et al.

Anonymous Referee #1

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This manuscript compares vertical profiles of black carbon (BC) mixing ratios from two global models (HadGEM3-UKCA and ECHAM5-HAM2) with aircraft observations. The models are found to over-estimate black carbon mixing ratios, particularly in the upper troposphere and tropics. Results of two additional sensitivity tests related to biomass burning emissions and convective wet scavenging are presented. A greater sensitivity of the BC mixing ratio to the convective wet scavenging than to the biomass burning emissions is demonstrated. This manuscript is well written and addresses relevant scientific problems related to the examination and modeling of BC profiles. The manuscript should be suitable for publication provided that the following points are satisfactorily addressed.

Major Comments:

The authors have taken great care in conducting a point-by-point comparison of the model with the observations along the flight tracks for a large region of the Pacific. The authors state that this is a powerful tool for evaluating BC, whereas previous studies have compared monthly and grid mean model mixing ratios with aircraft profiles. A presentation of these comparisons under the previous methodology relative to the new methodology could help to document and support the argument that this new approach is worth the additional effort.

In evaluating a global model, there is the possibility that compensating errors can occur, which can yield a closer agreement with observations for the wrong reasons. Thus, while these vertically-resolved aircraft observations provide a constraint on aerosol 'profiles', it is more difficult to argue that they provide a constraint on aerosol 'processes'. The authors should address this concern in the manuscript.

Also, the manuscript does not include an examination of the potential role of the convective transport parameterization itself on the comparison with the observations. There is no indication of the entrainment and detrainment rates used in the models and how rates these might differ between the models. There is the possibility that errors in the entrainment/detrainment rates might be compensated by errors in a given parameterization for wet scavenging, or other aerosol processes. Some of these other processes are noted in the specific comments below. These issues should be addressed in the discussion.

Specific Comments:

1) Abstract: End of first paragraph: Please quantify what is meant by 'a rather different structure'.

2) Abstract: second paragraph: Quantify what is meant by 'significantly improved with respect to observations'.

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3) Page 444, lines 3-4: Dust is omitted in HadGEM3. What influence does this have on the results? This excludes a considerable amount of aerosol surface area from the tropics. Could there be any feedbacks on the removal processes with this large coagulation sink excluded?

4) Page 444, lines 18-21: The HadGEM3 model has a different threshold for aging from insoluble to soluble aerosol relative to ECHAM5-HAM2 (10-mono-layer versus 1-monolayer). How does this influence the results? Does this delay the wet removal for HadGEM3 relative to ECHAM5-HAM2?

5) Page 444, line 29: Please indicate that this is the large-scale cloud parameterization, if this is the case. How are the insoluble and Aitken modes treated? Is in-cloud impaction scavenging included in the parameterization? How does the fractional contribution of stratiform wet removal to total wet removal differ between HadGEM3 and ECHAM5-HAM2?

6) Page 445, line 4: How does the assumption of a fixed 30% cloud fraction influence the results? Is this also the precipitation fraction?

7) Page 445, lines 8-10: Is the removal from the environmental layer at the level that the precipitation formed or otherwise?

8) Page 447, lines 1-3: How vigorous is the below-cloud scavenging in ECHAM5-HAM2 relative to HadGEM3? How might differences between the two models in the parameterization for this process influence the comparison of the resultant mixing ratios between the two models?

9) Page 448, lines 4-6: A reference to the figure number would be helpful here.

10) Page 449, lines15-19: How well does nudging reproduce the observed synoptic conditions in terms of the cloud and precipitation fields? This approach is not exactly the same as using assimilated meteorological fields, such as in a chemical transport model. With nudging of the vorticity, divergence, temperature and pressure fields in

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a global climate model, how tightly are the cloud and precipitation fields controlled between simulations, how well do they match to the actual observed meteorology and how does this influence the comparison with the observed aerosol mixing ratios?

11) Page 451, lines 16-21: The biomass burning emissions are emitted uniformly between 50m-3km in HadGEM3, while the ECHAM5-HAM2 emissions are within the boundary layer. How does this difference in altitude of emissions contribute to differences between the models?

12) Section 5.2: In the description of the convective scavenging, please include some details about the entrainment and detrainment rates used in the two models and how these might differ for different types of convection, and at different latitudes and how this might influence the results.

13) Page 453, line 15: Can the problem of too much aerosol aloft be related additionally to errors in the amount of aerosol that is entrained and detrained by the simulated convective plumes?

14) Page 453, line 18: Reference to the figure and row numbers might be added here.

15) Page 453, lines 26-29: The poor agreement is attributed to lack of realistic convective scavenging – but could other factors also play a role in this comparison? How well do the simulated precipitation fields agree with the observed precipitation? The lack of improvement for HIPPO3 illustrates the possibility for other factors/processes to confound the analysis. Can the possibility of compensating errors yielding a closer agreement for HIPPO1 and HIPPO2 be excluded? Could there be errors in the stratiform wet removal, which compensate errors in the convective wet removal?

16) Each of the 3 HIPPO campaigns occurred in a different season. Could the authors comment on the seasonal cycle of convective precipitation and wet removal, particularly towards the mid-latitudes and how this might influence the results?

17) Page 454, line 10-11: The observed high burdens are attributed to localised

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biomass-burning plumes – are these plumes expected to be consistent between all seasons since this is seen for all campaigns? Could the models be transporting too much aerosol aloft, and as a result not allowing enough transport to the Arctic?

18) Page 454, line 23: Please indicate where/when these strong biases are reduced and quantify the reduction.

19) Page 455, line 15-17: This sentence is confusing. The authors state that clearly the error might be in the parameterization of the scavenging process or in some other process. This indicates the problem. There is a difficulty to clearly attribute these discrepancies between the model and the observations to a single physical process. I am left unsure if we can actually use these observations to constrain aerosol processes in a global model – but they do provide a useful constraint on the aerosol profiles. Here and throughout, the authors should be careful to acknowledge these difficulties in their analysis.

20) Page 456, lines 25 onwards: Does a scavenging scheme that performs better in a nudged model environment capture reality more closely? Does nudging suppress any feedbacks in a global model, which might limit the validity of the conclusion that this is a more 'realistic simulation of the aerosol' during a flight. Nudging makes certain meteorological fields more consistent between simulations, but can you demonstrate that the cloud and precipitation fields are closer to reality and more consistent with each other between the simulations relative to the free-running simulations? Also, climate studies will generally use free-running simulations. Do the results indicate that the scavenging revisions should be adopted by the model, given the possibility for compensating errors in other processes and the lack of improvement for free-running simulations?

21) Page 458, line 9-11: Are the free-running simulations re-tuned between the implementation of the two scavenging schemes?

22) Page 458, lines 18-20: While the observations are useful in evaluating aerosol

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distributions, I think that the insight into physical processes (e.g. convective wet scavenging) is more difficult to obtain within the framework of a single global model. This should be more carefully considered in the text and title of the manuscript.

23) Table 1: What is meant by aerosol feedbacks enabled and disabled (last line of table) and how does this influence the results?

24) Fig 5: Were any free-running simulations conducted with the ECHAM5-HAM2? Is there the possibility for a significant difference between free-running simulations with different emissions in ECHAM5-HAM2?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 437, 2013.