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Geoscientific Model Development Discussions
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Interactive comment on "Concentrations and radiative forcing of anthropogenic aerosols from 1750–2014 simulated with the OsloCTM3 and CEDS emission inventory" *by* Marianne T. Lund et al.

Anonymous Referee #1

Received and published: 30 August 2018

General comments:

This paper evaluates the present-day aerosol distributions and radiative forcing in the latest OsloCTM3 chemical transport model. Surface concentrations, vertical distributions as well as aerosol optical depth are evaluated for the year 2010 against an extensive set of ground-based and satellite measurements. Uncertainties in the aerosol distributions are also assessed through a number of sensitivity studies assessing the uncertainty in the aerosol emissions, removal processes, meteorology and model resolution. The model is then used to assess the historical aerosol direct radiative forcing

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(RF) from 1750-2010. The net radiative forcing as well as the RF due to individual species are compared against the most recent published literature and CMIP5 estimates. Aerosol-cloud interactions are not assessed in this study.

This is a well-written, clearly presented, model evaluation manuscript. This paper does not document the latest updates to the OsloCTM3 model, but appropriate references are provided for this. Instead it documents the latest aerosol simulations with this model using the latest CMIP6 emissions inventory and subsequently derives the latest direct aerosol RF estimate. So I believe it is within the GMD scope as a Model Evaluation paper. This paper is timely given the CMIP6 project is now well underway and will provide a useful update to direct aerosol RF estimates in this regard.

I recommend this paper to be published subject to a number of revisions I detail below.

Specific comments:

I struggled to appreciate the motivation for the additional sensitivity simulations conducted in this study. While these studies are useful and worth reporting the motivation for conducting these studies and link with the rest of the paper needs to be made clearer. This is probably most easily achieved in the Introduction.

In Sect 2.3 Please report the length of each time slice simulation and how this impacts the signal to noise in resulting RF estimates. Are you running 20-30 years for each time slice?

In Sect 3.3 please describe clearly either explicitly or through appropriate references how you calculated the RFari. Have you calculated an effective radiative forcing or used the more traditional radiative forcing metric? It is currently unclear. Also, how are the individual species RFari determined - through species only runs or is the RT code able to output this?. Please provide this detail in the manuscript.

Use of older version of CEDS emission inventory: I am not convinced that the historical RFari will not be impacted at all by the choice of CEDS emission inventory. Changes

in spatial distribution of emission could impact aerosol removal, transport and thus lifetime, temporal shifts in the distribution could also potentially impact the historical evolution of the ERF. While I understand the computational burden of repeating all tests, two runs using the new CEDS version with 1750 and for instance year 2010 emissions would allow you to quantify the impact on the RF fairly easily to allow you to justify it in the text. It looks like you may have the 2010 simulation already from you Fig S4 plots.

Furthermore limiting your evaluation to BC is also questionable as I would expect notable differences in for instance SO2. I would request that you extend this evaluation over the USA to all aerosol species where you have observations.

Changes to the large-scale ice scavenging efficiency have a large control on the global aerosol distribution. The paper would benefit from a more detailed description of how the LS ice (and liquid) scavenging is parameterized in the model in Sect 2.1

Technical corrections:

Line 70: 2011 - this should be 2010

Line 170: It would be worthwhile to report here what global scaling factor you have used in the Gantt parametersation of marine OM. Gantt et al. 2015 I think use a global scaling factor of 6 but this is believed to be highly model dependent.

Line 281: Explicity state the aerosol concentration threshold below which you apply the Bond and Bergstrom method. Why do you change approaches? Again state the motivation for these different approaches in different regimes and why they need to be made. Does Zanatta lead to too high a MAC in the low aerosol regimes? Has this been constrained by observations - if yes, provide appropriate reference here. Some discussion of the uncertainties in both approaches as this links to your uncertainties in BC RFari discussed in Sect 3.3

Line 329-330: It would be useful here to be clear on what model variables you've

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sampled at 3 hour resolution and what ones you've sampled monthly and why you needed to do this.

Line 452: EBAS - I think this should refer to EMEP/ACTRIS. EBAS has not been previously mentioned.

Line 468: Remove "as for surface concentrations,"

Table 3: It would be useful to have the % change in burden listed alongside the burdens of all the sensitivity experiments. Perhaps in parentheses next to the burdens.

Line 541-544: Can you actually make this statement? You do not show any impact on surface concentrations. I do not think this paragraph adds any value to the manuscript and would remove.

Line 555: Could the higher correlation and lower bias found in the 1x1RES test not just be a consequence of improved spatial sampling in your comparison and not actually due to an improved distribution of AOD? As you do not evaluate the species whose emissions would depend more strongly on resolution (ie: dust, sea salt) it's hard to quantify the benefit here.

Line 638: OsloCTM3fast - what is this?? There is no prior mention of a fast configuration!!

Line 688: "These results emphasize the importance of assumptions related to the BC absorption" –> these results emphasize the importance of assumptions and uncertainties related to the BC absorption.

Figure 6: It would be more informative to show obs +/- 1 standard deviation instead of just + 1 std.

Table S2: Column 2 you use MNB instead of NMB

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