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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 #2

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In this paper, the authors describe updates made to aerosol representations in the Oslo Chemistry Transport Model, compare the simulated aerosol to various observations, and use that model with a recent emission dataset to derive the time series of radiative forcing of aerosol-radiation interactions (RFari) over the industrial era. Their estimate of RFari is at the weaker end of latest AeroCom and IPCC assessments, and the authors work suggest that globally-averaged RFari has weakened since the 1980s.

The paper is nicely written and to the point. Figures are well chosen and illustrate the discussion well. Sensitivity studies are interesting. There are a few aspects that can

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Discussion paper

be clarified however: The justification for model parameters is often lacking and there is also a need to better link section 3.3 on RFari timeseries with model evaluation.

Below I list the clarifications that I would like to see ahead of the paper's publication. Addressing those comments should not require additional analysis, so represent minor revisions.

1 Comments

- Abstract, line 38: Be more specific about what is new about the treatment of black carbon in OsloCTM3
- Abstract, line 47: Aren't the AR5 forcing estimates for 2011?
- Introduction, line 76: Are those the CMIP5 historical emissions documented by Lamarque et al., doi:10.5194/acp-10-7017-2010, 2010? If so, then reference the paper here.
- Introduction, line 77: I seem to remember that historical finished in 2005, not 2000, in CMIP5.
- Introduction, lines 114–116: Is that sentence still talking about scavenging?
- Section 2.1, line 157: What are the finer spatial and temporal resolutions of the new transfer rates?
- Section 2.1, lines 161–163, line 203, and Table 2; and Section 2.4, lines 284–285: The numbers selected for OC-OM conversion, hydrophilic fractions of BC and OM, aerosol solubility fractions, and absorbing OM fractions need to be justified. Where do they come from?

- Section 2.1, lines 169–170: How is the scaling of marine OM emissions done in practice? Sea spray is dependent on wind speed, so do you need to run the model a first time to get OM emissions, then scale them, then run again?
- Section 2.1, line 195: Why does the DEAD scheme need energy budget calculations?
- Section 2.1, paragraph starting line 198: Does the model account for re-evaporation of precipitation? And re-suspension of dry-deposited aerosol?
- Section 2.3, line 249: Why fix the meteorology to 2010? Once reanalysis data is available (from 1979 for ERA, I believe), then it would be good to use the meteorology that corresponds to the actual year. There is a sensitivity study dedicated to the impact of meteorology, and that impact is fairly sizeable (lines 556–558). So using 2010 for all years does not seem ideal.
- Section 2.3, paragraph starting line 254: It would be good to have more insight into the choice of sensitivity studies of removal. Why no SOLINC sensitivity study, for example?
- Section 2.3, line 258: I do not think that the resolution of the control simulation has been given at this stage, but I may have missed it.
- Section 2.4, paragraph starting line 270: It would be useful to have a Table summarising the assumptions made about size distribution and refractive index (or MEC/MAC if that is what the model uses) for all modelled species, with references where appropriate. That information is crucial to understanding differences in radiative forcing efficiencies between models, yet is rarely given in model description papers.
- Section 2.5, line 297: "*frameworks*" – did you mean networks?

- Section 2.5, lines 321–322: What does the AeroCom validation tool do?
- Section 3.1 starting line 354: It would be most useful to also include residence times for each species in Table 3. Perhaps in parentheses after the burden? Residence times are also crucial to understanding differences in aerosol distributions among models.
- Section 3.1, paragraph starting line 356: The authors try to explain differences with the previous version of the model, which is great, but that is not done consistently. Why is sulfate burden 35% higher than in OsloCTM2? (Line 360.) Is that understood? And what explains the 25% higher OA burden? According to lines 369–370, the marine POA contribution is too small to explain that large difference.
- Section 3.1, line 377: "*against output*" – it is the other way around, model versus observations.
- Section 3.1, line 384: What correlation? Need to rephrase to clarify that sentence.
- Section 3.1, line 414: Is being close to the AeroCom nitrate multi-model mean a good thing? How do models compare to observations in Bian et al. 2017?
- Section 3.1, lines 426–428: Erroneous emissions have a big impact on model performance. Can't you re-run at all? That would be a shame.
- Section 3.1, Figure 2: Could you show numbers in each slice of the pie chart in Figure 2? That would help make the comparison more quantitative.
- Section 3.1, paragraph starting line 453: Figure 3d suggests that the model overestimates low optical depths but underestimates large optical depths. Is that correct? From experience, it is a common model deficiency, It may have even been noted in an AeroCom paper.

- Section 3.1, line 468: Small addition for clarity: "*there are notable difference* in model performance"
- Section 3.2, line 502: The analysis should point to Table 2 to make sense of sensitivities to wet removal. Species with the same scavenging fractions are likely to behave similarly in such sensitivity studies.
- Section 3.2, lines 519–520 and Figure 4: What is happening in North America? The change is in the Midwest, whereas I would have put the source regions more to the East.
- Section 3.2, lines 526–527 and Figure 4: What is happening with dust in MERRA? Also a teleconnection with ENSO?
- Section 3.2, lines 530–531: Why such a strong dependence of nitrate on emission inventory? The vertical profile of ammonium nitrate formation is determined by temperature and competition from ammonium sulfate, so the role of emission inventories is not obvious. Which emission differences matter: ammonia or sulfur dioxide?
- Section 3.2, lines 537–540: Again, should link to Table 2.
- Section 3.3 starting line 618: It would most interesting to link specific model deficiencies to errors in RFari estimates. This is done briefly in the discussion, but could be done more clearly in section 3.3. For example, he model generally underestimate surface concentrations. Does that bias cancel out when taking differences over the industrial era? Another example is that the model underestimates aerosol in Asia. Does that mean that recent RFari is underestimated? Or that similar underestimations would have happened over US/Europe when sulfate was high in those regions in the 1950-1980s? A final example is about high latitudes. There is a confident conclusion about RFari changes north of 70°N (lines 679–681), but do you really have confidence in your model in that region?

- Section 3.3, line 638: What is "*OsloCTM3fast*"?
- Section 3.3, line 675: "*clearly*" – Not that strong a signal really. Did the radiative forcing efficiency change with the change in emitting region? Combining by eye the two panels of Figure 8 would suggest that the change is rather small.
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