Geosci. Model Dev. Discuss., 7, C1909–C1917, 2014
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Geoscientific Model Development Discussions
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7, C1909–C1917, 2014

Interactive comment on "ModelE2-TOMAS development and evaluation using aerosol optical depths, mass and number concentrations" *by* Y. H. Lee et al.

Anonymous Referee #1

Received and published: 6 October 2014

Review of "ModelE2-TOMAS development and evaluation using aerosol optical depths, mass and number concentrations" by Lee *et al.*, submitted to *Geosci. Model Dev.*

The paper is an evaluation of the aerosol distributions simulated by the TOMAS aerosol scheme used in the GISS modelE2 global climate model. The evaluation is similar to most papers of its kind, but covers a larger number of observational datasets to address more aspects of the aerosol model (for example including size distributions). The paper is most interesting and useful to other aerosol model developers when it clearly explains the reasons behind the choices of parameters (e.g. size distribution of emissions, parameterisation of DMS emission rates, fraction of primary sulphate emissions, differences with bulk mass scheme, etc.) and behind model skill, or lack thereof,

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when compared to observations. This good level of explanation is not always present, however, and most of my comments below suggest improvements in that direction.

The paper could have taken a different – and arguably more original – direction, looking at how improvements in the *host* model, from II-prime to ModelE2, have affected the skill of the aerosol scheme at reproducing observed aerosol distributions with fidelity. This is an important aspect of global aerosol modelling, as yet almost unexplored in the literature.

The paper is long, with a large number of Tables and Figures. However, considering the breadth of the model evaluation, it is difficult to recommend shortening the discussion or removing figures (with the possible exception of Figure 25). The conclusion (section 6) is a good summary of the findings. I recommend publication after the following comments, aimed at improving the discussion, are addressed by the author.

1 Main comments

- Section 2: Since the main motivation for developing ModelE2-TOMAS is to be able to use TOMAS in a better model than II-prime (page 5835, lines 8), one would have expected a more complete discussion of ModelE2 compared to II-prime, especially on those aspects that are relevant to the life cycle of aerosols and their radiative effects. So section 2 should be extended with a discussion of changes in cloud, precipitation, and transport schemes, summarising the improvements in those and how they are expected to impact on the quality of the aerosol simulation. A lot of a model's skill at simulating aerosols does not depend on the aerosol scheme itself, but on the host model – and this dependence has been little investigated in the literature so far. I would strongly encourage the authors to look into that aspect.
- Section 5.2: The AeroCom 1 simulations are a decade old now, and comparing

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against those does not mean much – aerosol modelling has progressed since then, and comparing to older – presumably poorer – models does not really demonstrate skill. The comparison should therefore be restricted to AeroCom 2 models (comparing total carbonaceous if a more detailed split is not possible) and ACCMIP. Also, since emissions vary between the different studies, comparing absolute values for burdens and mass fluxes is not really useful: the focus should only be on lifetimes and relative contributions to deposition rates, with burdens given on Table 5 for information only.

2 Other comments

- Page 5831: The title is misleading, because the paper is really about evaluation. Development details are delegated to previously-published papers. I would therefore drop the word "development" from the title.
- Page 5833, line 16: Strictly speaking, the pre-industrial atmosphere matters only for the radiative forcing, not for the radiative effect.
- Page 5834, line 11: "very accurately": I'm not sure what the point is here. Are the authors suggesting that single-moment representations are inherently less accurate in what they can simulate?
- Page 5837, section 2.1: What is the mixing assumption in the bulk model: external? Same question for TOMAS: how are aerosols mixed within each size bin (Page 5838, line 11)?
- Page 5838, line 17: Ammonium has just been discussed, so this statement seems redundant.
- Page 5838, line 23: How is hygroscopic growth represented then?

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- Page 5839, line 5: Why are the other kinds of nucleation unused? Ok, it is mentioned in section 4.2: it may be useful to point out here that the reason for not using a given parameterisation will be discussed later in the paper.
- Page 5839, lines 6–19: For the benefit of other model developers, it would be interesting to say how the size bin/cutoff configurations are selected. I assume it is a compromise between computation cost and fidelity of the model, but how is that latter quantified?
- Page 5840, lines 1–3: Is the fraction of precipitating cloud water computed in each model layer? Does the wet deposition flux account for re-evaporation of precipitation?
- Page 5882, caption of Figure 1: The caption could be improved to make clear that the Table lists aerosol and precursor emissions, and how nucleation is accounted for.
- Page 5841, lines 15–18: I understand the need for pragmatic choices like this one, but it would be useful to offer an explanation as to why the Nightingale et al. (2000) leads to an overprediction in the Southern Hemisphere in both aerosol schemes. Is it because of other aspects of the model?
- Page 5883, Table 2: Need to define GMD and GSD in the caption.
- Pages 5884 and 5885: It would make sense to merge Tables 3 and 4, since they are analysed together.
- Page 5846, lines 24–25: It would be useful to remind the reader that anthropogenic emissions are supposed to be representative of the year 2000, which justifies the choice of period for observations.

- Figure 2: The red and blue signs are difficult to distinguish. Perhaps use filled circles, or another, thicker, symbol? Also, the model overestimates concentrations by more than 10 times for 5 sites of the EMEP network. Do those 5 sites have some common characteristics that could be the signature of a specific weakness of the model?

- Page 5848, lines 4–5: This is interesting, but also worth an explanation. Why would DMS concentrations increase aloft? Lack of oxidants?

- Page 5848, lines 16–17: It looks like volcanic emissions help the model do the right thing in Mar-Apr, but not Aug-Oct. Is the agreement in Mar-Apr coincidental, or should one apply a seasonality to volcanic emissions? Also what about the peaks in observed SO_2 visible above 4 km over Tahiti and Easter Island: that looks like transported aerosol layers. Are those mentioned in the papers on the PEM-Tropic campaigns? Where do they come from?

- Page 5849, lines 5–6: TOMAS sulphate lifetime is one third longer than the AeroCom mean. I would not qualify that as “slightly longer”.

- Page 5849, line 7: Are we to understand that weak dry deposition rates explain the longer lifetime of sulphate in TOMAS?

- Page 5849, line 9: Is the remainder of wet deposition caused by large- scale precipitation? Also, I guess “moist convective clouds” are in fact simply convective clouds.

- Page 5849, line 13: Sulphate lifetime is given at 5.6 days on line 6 and in Table 5.

- Page 5850, lines 1–14: Comparing lifetimes of coarse mode aerosols is only meaningful if the models cover the same size ranges. Is that the case here?

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- Page 5851, line 6: Readers are left to draw their own conclusion, here, so the paper should be more affirmative: is the dry deposition parameterisation in TOMAS better – or equivalently, are dry deposition rates in other models likely overestimated?

- Page 5896, caption of Figure 6: Are OC surface concentrations given in terms of [C] or [OM]? There is a factor 1.4 between the two in TOMAS.

- Page 5851, line 24: OM aerosol concentrations are high over North Hemisphere continents, including in regions I would not particularly associate with industry or biomass-burning, especially on an annual average (midwest US, central Siberia). Are those biogenic sources?

- Page 5897, caption of Figure 7: I presume that units are the same as in Figure 6?

- Page 5852, line 6: Figure 7 really shows zonal cross-sections – but the vertical aspect of the Figures is not discussed.

- Page 5898, caption of Figure 8: Sulphate in ug[S] m-3 as before?

- Page 5853, lines 18–20: Isn't that statement in contradiction with the statement on lines 11–14, where TOMAS overestimated sulphate?

- Page 5854, lines 1–13: The sea-salt evaluation lacks discussion. The overestimations inland are probably due to the model resolution, which would produce an abnormally large transport from the ocean. The low bias in the Tropics is more interesting: perhaps a consequence of poor simulation of near-surface wind speeds in the host model?

- Page 5854, from line 14: Again, lack of discussion, this time for mineral dust aerosols. It is interesting that TOMAS seems to be doing better than the bulk

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aerosol model here. Could that better performance be explained by specific differences between the two models?

- Page 5855, lines 24-26: Why not show the comparison against IMPROVE where the OC:OM ratio is consistent with the choice made in the model? The other comparison is of little interest and can be removed. Again, the paragraph could discuss the results of the comparison of EC and OC – the comparison is not too bad in fact. Emissions will probably be the main source of error for the networks used here, which are located relatively close to the source.
- Page 5856, line 7: It does not mean that they don't contribute to PM2.5 – so sea-salt and mineral dust aerosols may be partly to blame for the under-prediction.
- Page 5856, lines 16–19: Please give the full details of the satellite products used here: collection for MODIS, version for MISR, and whether level 3 (monthly, gridded distributions) were used.
- Page 5856, lines 25–26: This statement is unclear: does that mean "in the cloud-free fraction of gridboxes" or "in cloud-free gridboxes"?
- Page 5857, first paragraph: It should be said that the satellite products do not seem to support the bands of large sea-salt AOD at high latitudes. In that respect, TOMAS does better than the bulk model. Also, the quality of the comparison will depend on the host model simulating clear skies in the right regions and seasons.
- Page 5859, line 15: Number concentrations from the bulk aerosol model could be derived from the simulated mass and prescribed size distributions, but the comparison would probably not be useful.
- Page 5861, lines 2–4: Why is that surprising?

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- Page 5861, line 16: How was the 30% number obtained? By reducing the altitude where simulated aerosol numbers are taken until the agreement with observations is satisfactory? If so, isn't that being too easy on the model?
- Page 5861, line 25 and Figure 21: It is difficult to tell which of the three simulations does best. Is it BASE? If not, wouldn't that be a reason to redefine the scientific configuration of the BASE simulation, at least as far as nucleation is concerned? Figure 24 strongly suggests that LowNUC would make a good BASE.
- Page 5862, line 6: It is difficult to reconcile Figures 21 and 23. The model seems to a have a high bias in aerosol number over Europe - something that Figure 21 doesn't really show. Should we conclude that this is specific to that region?
- Page 5863 and Figures 25 and 26: Both Figures show similar things, but Figure 26 does it better. With Figure 25, I guess the authors wanted to show the extend of observational variability, but that backfires since the observations are shown to provide little actual constraint. So perhaps Heintzenberg et al. (2000) is not suited for this kind of comparison.
- Page 5863, lines 22–24: It should be easy to check that convective clouds are indeed more frequent in ModelE2.
- Page 5864, line 6: Does the "rest" cover non-marine environments, non- boundary layer locations, or both?
- Page 5864, lines 19–21: Again, the authors rely on the fact that the observational dataset does not provide a strong constraint. This is a bit unfortunate – could a better use of the dataset be made?
- Page 5866, line 25–28: Saying that aerosol modelling produces large differences is not uninteresting, but if host model impacts had also been discussed in details in the paper, it would have been possible to tell which of the two is the dominant

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factor. Also, it is possible that the two aerosol schemes are affected by the host model in different ways. Host model effects would then be misattributed to aerosol modelling differences.

3 Technical comments

Page 5840, line 22: Typo: “InitiAtive”.

Page 5842, line 12: Typo: Stier et al., 2005.

Page 5844, line 5: Delete “that”.

Page 5886, caption of Table 5, typo: “standard” deviation.

Page 5853, line 1: The sentence does not read well. I suggest: “For details of the GBD PM2.5 dataset, the reader is referred to...”

Page 5859, line 4: Typo: “underprediction”

Interactive comment on Geosci. Model Dev. Discuss., 7, 5831, 2014.