Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-191-RC1, 2018

Geoscientific Model Development Discussions
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EGU

Interactive comment on "The Chemistry Climate Model ECHAM6.3-HAM2.3-MOZ1.0" *by* Martin G. Schultz et al.

Anonymous Referee #1

Received and published: 17 January 2018

The manuscript presents a description and assessment of chemistry and aerosols from the most recent version of ECHAM6. A fairly extensive presentation of the treatment of gas-phase chemistry in the model is presented, with a more limited presentation of aerosols. The gas-phase chemistry is also assessed by comparing a nudged simulation for 2008 against a variety of satellite, sonde and surface observations and the sensitivity of the results to the assumed global total of lightning NOx emissions is investigated. Additionally, three recent versions of the model are compared for a variety of aerosol-related quantities.

The description of the gas-phase chemistry is quite thorough and the comparison of the gas-phase chemistry against observations includes many of the widely used datasets. My only really significant criticism is that a few of these comparisons seem to have

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Discussion paper

been made without a great deal of rigour. There is very clearly differences in total column ozone between IASI and OMI, but the discussion of the model results concludes that '...careful examination shows a model bias of up to +20 DU compared to OMI, whereas the model TCO show an overall negative bias to IASI (Figure 1). Both, the apparent model underestimation with respect to IASI and the overestimation with respect to OMI, are within the uncertainties of the retrievals.' While I am not an expert on satellites, 20 DU would be something in the neighbourhood of 5% error and I believe OMI uncertainties are much smaller than that. I am also concerned about the comparison against IASI column CO as it is not clear whether the model column was treated with the IASI averaging kernel. The discussion of the large discrepancies with IASI CO column concludes that 'These differences can be explained by the limited sensitivity of IASI in the lowermost layers (George et al., 2015).' The averaging kernel of IASI applied to the ECHAM CO fields should have accounted for the vertical sensitivity of the IASI column CO, so it is unclear whether the differences are due to a lack of applying the IASI averaging kernel or whether the vertical distribution of CO is radically different in ECHAM. These concerns, along with other minor concerns, are given below.

Page 3, Line 5: A good reference for the general state of CCMs would be Morgenstern et al. (Geosci. Model Dev., 10, 639–671, 2017).

Page 5, Lines 12-13: I'm having trouble understanding the significance of the sentence 'To accommodate SALSA, aerosol processes, which are handled by HAMMOZ, i.e. emissions, wet and dry removal, particle phase chemistry, and radiative properties are treated using the sectional approach.' I assume the significance derives from the fact these processes are treated in a part of the code you would not refer to being as part of SALSA and would be treated the same whether SALSA or the modal M7 model are used?

Page 9, Line 20: The reference to the historical emissions of Lamarque et al. (2010) are described as being from ACCMIP, but I think it is more accurate to refer to these as the CMIP5 emissions.

Page 12, Line 23: Does the leaf area index used for dry deposition follow the seasonal cycle of LAI that is simulated by JSBACH? Is stomatal uptake similarly tied into the short-term variations in stomatal resistence that would, presumably, be calculated by JSBACH?

Page 13, Line 12: While three months of spin-up would be sufficient for the troposphere, the stratosphere would still be a long way from settling down. Was there a 'close' initial state specified for the chemical species derived from earlier simulations?

Page 14, Lines 10-22: Some of the really specific details on the IASI satellite are not really applicable to the subject of the paper and should be trimmed back.

Page 15, Lines 5 – 22: I find the comparison of the TCO to IASI and OMI products to be a bit unsatisfying. At lines 20 - 22 it is stated: 'IASI TCO have been found to be larger by 10–11 % compared to TCO from another UV-vis satellite sensor, the Global Ozone Monitoring Experiment-2 (GOME-2) instrument, and from ground-based UV Brewer-Dobson data (Boynard et al., 2016).' And this statement could be contrasted with the findings of McPeters et al. (Atmos. Meas. Tech., 8, 4845–4850, doi:10.5194/amt-8-4845-2015, 2015) from an assessment of OMI TCO that 'Comparison with a network of 76 Northern Hemisphere ground-based Dobson–Brewer instruments shows very good agreement over a 10-year comparison period. The bias of OMI relative to other observations of about 1.5 % is due mostly to the use of the older Bass–Paur ozone cross sections.' Given the good agreement of OMI with independent measurements of total column ozone, I find it difficult to understand how the model TCO can fall within the observational uncertainties as stated on lines 15-18: 'However, careful examination shows a model bias of up to +20 DU compared to OMI, whereas the model TCO show an overall negative bias to IASI (Figure 1). Both, the apparent model underestimation with respect to IASI and the overestimation with respect to OMI, are within the uncertainties of the retrievals.

Page 15, Lines 28-30: The comparison of temperatures is certainly an important part of the discussion of Antarctic chemistry that follows, but I think it is worth reminding readers that the temperature in ECHAM was nudged to reanalysis and is, therefore, not a test of the model itself.

Page 15, Lines 32-34: Is there any explanation for the low bias in HNO3 before the onset of denitrification that can be seen in Figure 3, row 2?

Page 23, Lines 3-6: The difference in the total column of CO between ECHAM-HAMMOZ and IASI, for both the absolute amounts and seasonality, are explained as being due to the different vertical sensitivity of the IASI observations. I am most familiar with comparisons to MOPITT CO, and for MOPITT it is necessary to convolve the model CO with the MOPITT averaging kernal, taking into account the a priori used in the MOPITT retrieval, to make a valid comparison between the model and observations. I would imagine a similar procedure would be necessary for IASI but the description sounds as if there was no application of the IASI averaging kernal to the model fields before the comparison in Figure 9 was made. It would be very helpful to have more details of how the model data was treated for the comparison against IASI column CO. Assuming the model CO was not treated with the IASI averaging kernal, the explanation of the discrepancy against IASI column CO makes sense but the comparison itself seems to have little quantitative value.

Page 24, Lines 4-12: The discussion of the reasons for the model bias against surface observations seems perfectly valid. I would just add that a similar pattern of difference was seen across the suite of ACCMIP models, as described in Naik et al. (Atmos. Chem. Phys., 13, 5277–5298, doi:10.5194/acp-13-5277-2013, 2013).

Page 25, Table 5: For ozone the sum of Loss + Deposition for the reference run is comparable to that shown for ACCMIP multi-model mean (Young et al) and the burden is smaller, yet the ozone lifetime is longer. Is this because the lifetime was calculated for each of the six models and averaged, or is there a problem with the values in the table? I will note, using values for the reference run, 321/(4866+791) gives a lifetime of

20.7 days while the table quotes 24.1 days.

Page 27, Figure 11. On my screen, at least, the contour plot of precipitation against ERA-Interim shows grey shading for near-zero values but the colour bar does not seem to indicate an interval shaded grey.

Page 28, Line 13 – Page 30, Line 18: This is a very dense section that rapidly covers a lot of different aspects of these simulations: nudging with and without temperature, different sea-salt emissions schemes, with MOZ1 and without, and M7 versus SALSA. Then the effects of all these changes are discussed and tied to differences that are itemized in Table 8, with reference to figures in the supplementary material. While I am very sympathetic to the need to document the differences between succeeding versions of the model, this is quite the collection of overlapping changes. While it was possible to untangle it all with a bit of rereading, this section might be helped with a bit of re-organization.

Page 28, Line 18: When the HAM2.2 and HAM2.3 simulations are described it is stated that '...the temperature is not nudged.' Could you reword this to make it more clear that all three simulations have nudging, but that the exact quantities nudged are not the same across all simulations? It becomes apparent in the discussion that follows, but initially it was unclear if the HAM2.2 and 2.3 simulations were free-running.

Page 29, Line 3: I am not familiar at all with the use of 'resp.' in 'The shortwave (SW), resp. longwave (LW) cloud radiative effects (CRE)...' If it is to indicate a second parallel argument, a construction I am more familiar with is to put the second option in brackets like 'When the balloon goes up [down] the volume will expand [decrease].'

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