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

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Author comment in response to Anonymous Referee #1

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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

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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 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.

We thank the referee for the thorough review and appreciate his/her concerns regarding the evaluation with satellite retrievals for TCO and CO. These analyses have been enhanced with proper use of averaging kernels, and the corresponding manuscript sections have been rewritten. Furthermore, in response to referees #2 and #3, we included additional analyses on total column ozone and the tropospheric ozone budget covering the whole 10-year simulation period from 2003 through 2012.

Before answering the detailed comments, we must point out the correction of an error in the previously submitted manuscript: contrary to the description in the older manuscript version, the simulations described in this paper were performed with the M7 aerosol scheme, not with SALSA. The text has been modified accordingly. We performed shorter test simulations comparing the two schemes, and found only small differences with respect to the gas-phase chemistry. A closer investigation of aerosol differences is beyond the scope of this paper. We apologize to the reviewers for any confusion this may have caused.

All detailed comments were addressed as explained 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).

This reference was added, and also the recently published article by Young et al. (2018): https://www.elementascience.org/articles/10.1525/elementa.265/

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?

This subsection has been rewritten in order to provide a correct model description. In the previous manuscript version we had erroneously stated that simulations were done with the SALSA scheme, when in effect they had been performed with M7 microphysics. This sentence is therefore now obsolete.

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.

This is debatable. Lamarque et al. (2010) in their abstract state "The primary purpose of this inventory is to provide consistent gridded emissions of reactive gases and aerosols for use in chemistry model simulations needed by climate models for the Climate Model Intercomparison Program #5 (CMIP5)". In fact, the chemistry simulations themselves were not part of CMIP5, but they were the focus of ACCMIP. We prefer to leave this as is.

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?

No. This is an area, where the interaction between model components has not yet been fully realized. To make this explicit, we have added ", but this coupling has not been used in the simulations described in this study." after the reference to Stanelle et al. (2014), and we added a sentence: "A dynamic coupling of stomatal resistance with JSBACH has not been implemented." in the paragraph starting with "Surface resistances . . .".

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?

Yes. We now added: "The simulation started with initial conditions for October 2002 taken from the Monitoring Atmospheric Composition and Climate (MACC) reanalysis described by Inness et al. (2008)." to clarify this. Furthermore, an additional figure in the supplementary material shows the evolution of the model bias with respect to total column ozone, and this is mentioned in the text.

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.

This section has been rewritten and the technical description has been shortened.

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.

We restructured the TCO discussion and avoid to discuss model biases with satellite products other than IASI. It is now made clear that the OMI evaluations are solely meant to assert the correct representation of chemical and dynamical processes. We also state explicitly that a comparison of satellite retrievals and their biases is beyond the scope of this paper.

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.

We added a sentence to this effect. However, we would like to point out that even a nudged model may show temperature differences with respect to the observations due to two reasons: (i) even ECMWF's data assimilation is not perfect, and the IFS may generate some biases (see for example Berkes et al., ACP, 2017), and (ii) nudging doesn't strictly overwrite the model's own temperature calculation, but only draws it closer to the analysis. This is illustrated in the attached figure, taken from the master's thesis of Jonathan Krefting, one of the study authors (University of Bonn, 2017).

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?

From further analysis we can exclude errors in the denitrification or problems with downward transport from the mesosphere as possible reasons. At this point we can only speculate that the low bias is related to tropical NOy formation and subsequent meridional transport.

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.

This analysis was redone and now includes averaging kernels. The text has been rewritten accordingly.

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).

A reference to the Naik et al. study was added with the comment suggested by the referee.

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.

We thank the reviewer for spotting this error in the table. The lifetimes had erroneously been calculated without taking the deposition term into account. The table has been updated. All lifetimes are now around 21 days. As a result, the text on page 25, lines 15-16 has also been slightly modified. It now reads: “Considering that our base run has very low lightning NOx emissions (at the low end of the models described in Young et al., 2013), it is somewhat surprising that our ozone chemistry is so active. The ozone lifetime is slightly shorter than the multi-model averages of Stevenson et al. (2006) and Young et al. (2013), but well within the range of these studies. Yet, the ozone lifetime of ECHAM-HAMMOZ is substantially shorter than the result reported by Lamarque et al. (2012) (19.2 days versus 26 days if we use their tropopause threshold [. . .])”

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.

Grey shading denotes areas that are not significant according to are not significant according to a t-test (95\% confidence interval). This information has been added to the figure caption.

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.

This section has been rewritten in order to improve its readability, and the motivation for this analysis has been made more explicit. Furthermore, the erroneous references to SALSA were removed.

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.

This has been clarified as part of the rewrite.

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].'

This sentence was changed according to the referee's suggestion.

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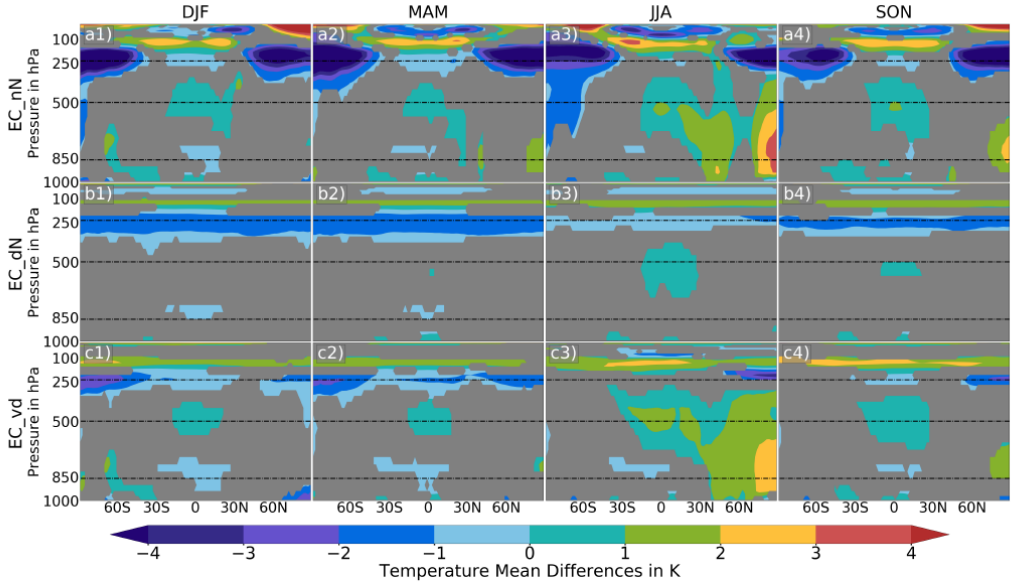

Figure 6.1.: Vertical profile of the zonal averaged seasonal differences of temperature climatology to ERA-Interim. Plots show differences between the model simulations *EC_nN* (a), *EC_dN* (b), *EC_vd* (c) and ERA-Interim over the boreal winter (DJF, 1), spring (MAM, 2), summer (JJA, 3) and autumn (SON, 4) according to the two sided Welch-test. Differences between the two fields which are not statistically significant to the t test (95% confidence interval) are masked in gray. The dashed lines mark pressure levels 250 hPa, 500 hPa and 850 hPa and are provided later on a global perspective.

Fig. 1.

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