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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 #2

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General Comments

The manuscript of Schultz et al aims at thoroughly describing and evaluating the chemistry-climate model ECHAM-HAMMOZ, focusing on the tropospheric gas-phase chemistry. Overall, it is well written and structured, and hence pleasant to read. The

description part of the model is clear, with the right levels of details. The evaluation part is most of the time clear too, and the authors honestly point out the issues they encounter, and propose explanations for them. At the end, the authors show that ECHAM-HAMMOZ performs well for most of the parameters/regions they look at, and I agree with them. There is no doubt that this paper is of great interest for the community, for the evaluation of this model, but also for questioning the '5Tg of LiNOx' law.

However, I have a major concern that, to me, prevents publication of the actual version of the manuscript : ECHAM-HAMMOZ is supposed to be a Chemistry-Climate Model. The authors mention that a 10 year simulation was made, from 2002 to 2012. But, apart from the 6.3 section which clearly refers to a 10-year mean and maybe the 5.2 section (not clear for me), the whole evaluation lies on the year 2008. Even if this simulation is made with nudging to ERA-Interim data, I would expect a more long-term evaluation. Is the chemical part of the model able to run these 10 years steadily? The authors never answer to this point, that is, to me, a key point when we talk about chemistry-climate modeling. I disagree with the conclusion sentence 'A ten year simulation from 2003 to 2012 (. . .) has been evaluated with various observational data and compared to other studies'

To conclude: the 2008 evaluation part is very good, but another 'long term' part is lacking.

We thank the referee for her/his constructive criticism and the suggestion to further improve our manuscript by evaluating some aspects of the simulation over the entire 10-year duration. In response to this referee (and referee #3) we added a comparison of total ozone column with the MERRA2 reanalysis (Hovmöller plot in section 5.2 and a bias plot in the supplementary material), and we added a brief discussion about variability of the global tropospheric ozone budget in section 6.1, including a figure showing monthly variations of the dominant ozone production and loss terms, which demonstrate the stability of the model and its chemical mechanism. We refrain from adding more evaluation with additional tropospheric observations, because this

would necessitate some elaborate discussion of the simulated and observed variabilities, which are clearly beyond the scope of this paper. We also added 2 sentences in the description of the model set-up to allow readers to relate the year 2008 to other years in the early 2000's.

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.

Specific comments

I have also specific comments/questions : Section 3.2

Please add references to the sentence page 11, line 12 : 'Recent modeling studies tend to adopt lower global lightning NOx emissions'

This sentence was removed. It had been erroneously kept in the manuscript as a result of a mis-communication.

Section 5.1 Many details about IASI and OMI data are given. But I wonder if these informations 1) are really usefull in this manuscript, however interesting they are 2) are used in the computations of the columns : is the sensitivity of the sounder taken into account, for instance applying averaging kernel profiles to the model ozone ? (Same question for total column of CO in section 5.4) Figure 1: a map of the differences would help Figure 3 : again, a map of the differences would help the reader. Please add in the caption of the figure that it corresponds to a profile at 81S. The lack of comment on ClO is frustrating. If it is panned to work on it in a next paper I suggest not to show the

ClO plot (not really usefull)

The section on TCO evaluation with different satellite products was rewritten and the technical descriptions of the instruments shortened. We have now also include averaging kernels in the calculation of the model columns. We removed the ClO plot and expanded the discussion on HNO3 instead.

Section 5.2

Do these annual values correspond to 2008 or to the average over 2002-2012? If only 2008, it would be valuable to do it on the ten years, it would make results more comparable to the climatology (except if climatological data is taken only for 2008).

This comparison indeed shows simulation results of the year 2008 in comparison to the climatological average of the sonde data.

Section 5.3

Figure 8 : January and July do not have the same scale (both horizontally and vertically). That is confusing.

We apologize for this confusion and will try to update this figure before publication of the paper.

Section 5.4

The differences between model and obs for CO can also be attributed to the emissions. That is obvious and refered to in the section 5.5 as if it has been explained in 5.4, but it is not. Same question as for ozone : is the sensitivity of the sounder taken into account to compute the column before comparing to observations ?

CO columns are now compared with averaging kernels applied. The text in section 5.4 has been modified to refer to the potential emission issues which are then discussed in section 5.5.

Section 5.5

The authors mention that the discrepancies between observations and model for surface CO in April could be attributed to too high values of dry deposition. It is not clear to me why the deposition would be too high in April and not in October.

As discussed in Stein et al. (2014), changes to the dry deposition of CO do impact on the seasonal cycle of simulated surface CO. Given that this is a model description paper, we believe that a more in-depth discussion of this aspect is not warranted here.

Section 6.1

Table 5 : Is there a reporting mistake or does the lifetime of ozone remain the same when lightning Nox are enhanced, and net production of ozone goes from 443 Tg/yr to 563 Tg/yr ? In a general point of view, I agree that having a more active (because more complex) chemistry can give higher production and loss terms, and that being not strictly comparable for all parameters to previous studies is not necessarily a problem. However, the results if Run lght*2 and Run lght*4 are really questioning. An increase in the global burden of troposheric ozone of 19 % when adding 3,8Tg of N via Nox is estonishing. Did the author test a version withour any lightning NOx? The author seem to be uncomfortable with this part, hypothesizing that there is 'some issue with the dynamics or physics of ECHAM in the tropical troposphere that impacts on its ability to reproduce the global budgets of reactive trace gases'. I am not convinced by the arguments of this section: no evidence is given that it is not a question of chemistry. I would suggest the author to look at these annual budgets for the ten year of simulations. This would determine if the problem is general or related to the 2008 particular year, both for chemistry and for precipitation.

We concede that this aspect of the model is not fully resolved, but we maintain our view that the reasons for ECHAM-HAMMOZ's relatively sensitive ozone budget are primarily of a dynamical nature. A brief discussion on the 10-year variability of the ozone budget terms has been added (these are very stable), and further supporting evidence from

the evaluation of cloud liquid water and cloud ice in ECHAM simulations.

The reported result, that the tropospheric ozone lifetime is indeed rather independent of the lightning NOx scaling, is correct, even though we had to correct an error in our lifetime calculation (see response to referee #1). This is the result of a slight increase of the ozone lifetime in the NH ($\sim$12%) and small decreases in the SH and in the tropics. Our explanation is that the ozone lifetime is a property of the chemical mechanism rather than the emissions. The additional NOx "accelerates" the chemistry leading to larger production and loss terms, but also to a larger burden. The increases in loss and burden compensate each other.

Section 6.3

This section is the only one that refers to an evaluation of the ten-year simulation. It seems a little unlogical : either the authors consider that the 2008 evaluation is sufficient (and then the 'radiation, clouds and aerosol' evaluation should be made on 2008), or they consider a ten-year evaluation is needed for the whole manuscript. Again, the second option is the better one to me.

We have added two paragraphs and two figures to the manuscript (plus one to the supplement) describing the variability of total ozone column and tropospheric budget terms. This extra material clearly shows that ECHAM-HAMMOZ produces stable results throughout the 10-year simulation period, which answers the main concern of referee #2. The more detailed comparisons with observations are left unaltered. A more thorough evaluation of the 10-year period with independent observations would be out of scope for this model description paper and merit an independent study, not least because this would entail the necessity to relate to a large set of previous studies which discuss the variability of tropospheric trace gases and their underlying reasons.

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