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

Anonymous Referee #2

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General Comments

The manuscipt 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.

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Discussion paper

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.

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'

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)

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)

Section 5.3

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

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 ?

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.

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.

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.

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