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

Martin G. Schultz et al.

m.schultz@fz-juelich.de

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Author comment in response to Anonymous Referee #3

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Schultz et al. present a thorough description and evaluation of the gas-phase chemistry in the latest version of the ECHAM-HAMMOZ chemistry climate model. The manuscript is very well written and includes ample description of the model and simulations (leaving room for the other manuscripts to be published). My only major comment is the focus on a single year for the model evaluation for most of the quantities. Although

the analysis is very well done, in order to fully evaluate a chemistry climate model, I'd think it would be necessary to evaluate it beyond a single year of a nudged run – i.e., 10 year climatology from a free-running simulation. I understand that a single year using nudged fields makes the comparison easy and straightforward, but any sense of stability and interannual variability is lost. That said, I think the manuscript should be published with minor revision.

We thank the referee for his/her positive comments. In response to the major comment from this referee (and also referee #2) we have added two short paragraphs and two figures to the manuscript (plus one in the supplement) describing the variability of total ozone column (section 5.2), and of the major chemical tropospheric ozone budget terms (section 6.1). The more detailed comparisons with independent observations are left unaltered.

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:

Page 14, lines 5-24: Some parts of the discussion of the IASI instrument are probably excessive for this paper, however, it would help to talk more about how the comparison to the model was made.

This section has been rewritten (see also comments to referee #1).

Page 15, line 28: May be useful here to reiterate that the model temperatures are nudged.

A sentence was added to this effect.

Page 16, line 5 and elsewhere: stating "however, a close examination..." - where possible it would be helpful to just have a 3rd column/row that shows (model minus observed) so that the differences can clearly be seen.

The figures comparing TCO and CO columns with IASI were redrawn. They now include application of averaging kernels to the model results and a third column with relative differences was added. The ClO plots were removed.

Page 17, line 5: Is this comparison for the full 10-year simulation or only with 2008 - ?

Like the other comparisons with independent observations in this manuscript, the ozonesonde evaluation only refers to the year 2008. The newly added discussion of the decadal variations of the tropospheric ozone budget in section 6.1 lends confidence that ECHAM-HAMMOZ might perform similarly well during other years. A more detailed analysis of ozone variability is beyond the scope of this model description paper. We added “year 2008” to the first sentence in section 5.2 to make this more explicit.

Page 18, lines 2-4: How does the sensitivity no_het_HNO3 compare for other metrics (e.g., ozone burden, methane lifetime, etc.)? What might be a way of fixing this - Possible that the uptake coefficient is too high?

Section 6.2 discusses the no_het_HNO3 simulation. Possible fixes could indeed be an adaptation of the uptake coefficient, or alternatively an explicit parametrization of re-evaporation. However, the latter would require the addition of nitric acid to the HAM aerosol scheme.

Page 22, lines 15-16: Which grid cells?

We added two examples to the text as “[. . .], for example over the Mediterranean or in Nebraska, US.”

Page 23, line 3-4: Is it possible the differences could also be due to inaccurate emission data, as in Sect 5.5?

Yes. This is a possibility. The text in section 5.4 has been slightly expanded to allude to this possibility. Also note that the CO column comparison now makes use of the IASI averaging kernels.

Page 29, line 3: What is resp.?

As suggested by referee #2 we changed this to read “The differences in SWCRE (LWCRE) are -6 W m-2 (-2 W m-2) with respect to ECHAM6.3-HAM2.3 [. . .]”. Please note that this entire section has been rewritten to improve clarity.

Page 30, line 2, values "of" Textor et al. (2006)

corrected.

Table 5: seems to be a mistake with the reported ozone lifetime values (24.1 days for reference and both LNOx sensitivity runs)

We thank the reviewer for spotting this error. Indeed, the calculations were done incorrectly as we omitted the dry deposition term (see also response to referee #1). However, the fact remains that the ozone lifetime remains rather constant across the various sensitivity runs. Reasons for this are discussed in response to referee #2.

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