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Interactive comment on "Simulation of tropospheric chemistry and aerosols with the climate model EC-Earth" *by* T. P. C. van Noije et al.

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We would like to thank the reviewer for the positive and constructive review. The reviewer's comments have been very useful to further improve the paper. Below we address the specific points raised by the reviewer. The reviewer comments are given between quotes.

"The paper itself is well structured and well written. The rationale behind implementing chemistry and aerosols in Earth System models is established well. The experimental set up of the various present-day simulations carried out is described in sufficient detail. However, there are aspects of the gas-phase chemistry evaluation which could be more comprehensive and discussed quantitatively rather than qualitatively, and could include more comparisons with observations rather than solely relying on comparisons

with the offline simulations. Details can be found under "Specific Comments". In relation to the aerosol evaluation, the focus is solely on aerosol optical depth and some recommendations for further evaluation are detailed under "Specific Comments"."

We have included more comparisons with observations and extended the evaluation of the aerosol simulation as suggested by the reviewer. See our answers below.

Specific Comments:

1. "The introduction includes aspects on the role of stratospheric chemistry and stratospheric aerosols in the Earth system. Given that TM5 does not include these aspects, I suggest that they be removed or reduced due to lack of relevance for the current model description."

The introduction is meant to give a general motivation for coupling an atmospheric chemistry and aerosol model to a climate model. Stratospheric ozone is an important element of this, which is why it is discussed in the introduction. We believe it is important to present the importance of tropospheric chemistry and aerosol in a broader context, and the discussion of stratospheric ozone is useful in this respect.

2. "The version of TM5 being coupled to IFS includes aqueous-phase chemistry for the oxidation of dissolved SO2 by O3 and H2O2 but details of this chemistry haven't been included either in this paper or that of Huijnen et al. (2010). Please add sufficient details."

It is true that the details of the aqueous-phase chemistry in the model are not given in the paper by Huijnen et al. (2010). In the revised manuscript, we will include more details and add the proper references.

3. "Can you provide some indication of the increase in computational cost of EC-Earth when TM5 is included? In particular, it would be useful to know what additional cost comes from the OASIS coupler."

We will provide such an estimate. The cost related to the use of OASIS is expected to

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be small compared to the cost of running TM5.

4. "Although this paper isn't detailing aspects of the TM5 model, it would still be useful to include some information on deposition processes. Can you also include an explicit statement on whether there is any coupling between convective transport, for example, and wet deposition?"

The deposition processes are described in the papers by Huijnen et al. (2010) and Aan de Brugh et al. (2011), and references therein. We will clarify this further in the revised article. In these papers it is explained that there is indeed a coupling between wet deposition and convective transport, for example.

5. "The implementation of emission heights has been altered in TM5 since the publication of Huijnen et al. (2010). Can you discuss the rationale behind these changes? What were they based on? Are the emission heights identical between the offline and online simulations? Further details would be useful here."

We will provide some further details on the emission heights. The same emission heights are used in the different simulations.

6. "Given the importance of the specific humidity bias in EC-Earth on global mean OH and the oxidizing capacity of the atmosphere, can you include an equivalent plot to Figure 1 but for specific humidity? Some model physics changes (e.g. convection) can affect humidity without a corresponding change in temperature. It is also worth putting these biases in the context of other climate models."

We have included the corresponding plots for specific humidity. We will discuss these biases in the context of other climate models.

7. "The evaluation of 222Rn consisted primarily of comparisons with the offline simulations. It would greatly help if the comparisons could be extended to include observations. Despite the simplicity of the tracer experiment and the emissions used, it is still a useful tool for model assessment. Does the online simulation perform worse or

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better than the offline simulation relative to observations? The assessment could also usefully be extended to include that of 210Pb."

We have added a comparison of the 222Rn concentrations from the different simulations with a number of station observations and will include a short discussion of the results. The main reason for including 222Rn in this paper was to compare the vertical transport in the different simulations and to show that the differences are mainly due to the representation of cumulus convection. An evaluation of 210Pb is beyond the scope of this paper.

8. "In section 4.3, one potential difference between the offline and online simulations is that of lightning emissions. Can you include further details on differences in the global distribution and global annual emission totals for lightning emissions from the simulations?"

The total NOx production from lightning is 10-11% higher in the EC-Earth simulation compared to the simulation with ERA-Interim. The difference is significant and is associated with a shift from the tropics to the extratropics. In the revised manuscript we will provide the simulated mean annual NOx production together with the standard deviations calculated from the interannual variability. We will also include a short description of the shift in the distribution.

9. "In Section 4.4, the offline and online simulations underestimate observed concentrations of CO in the NH. Other modelling studies have also shown similar biases, using identical anthropogenic and biomass burning emissions (e.g. Lamarque et al., 2010). It would be worth mentioning that EC-Earth/TM5 is not unique in this respect."

We will include a statement about this.

10. "The evaluation of the aerosol component of TM5 in the online simulations has focussed solely on aerosol optical depth. No comparison of aerosol precursor gases (e.g. SO2) with observations is included. No comparison of component aerosol burdens

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(e.g. sulphate, dust, organic carbon) with observations is included. No comparison between aerosol budgets (e.g. primary production, secondary production, burdens, lifetimes etc..) between the offline and online simulations is presented. These would greatly extend the evaluation of the aersosol component of EC-Earth and would make a useful and valuable addition to the manuscript. It would also improve the balance of the paper between the gas-phase chemistry and the aerosol evaluation."

We have followed the reviewer's suggestion. In the revised manuscript we have included maps of the vertically integrated burdens of the different aerosol components and their contributions to the AOD at 550 nm, together with the differences between the EC-Earth and ERA-Interim simulations (Figures 12 and A1). We have also included a comparison of global budgets (including chemical production and destruction), burdens and lifetimes of the different aerosol components, precursor gases (DMS, SO2, NOy and NHx) and total reactive sulfur with published multi-model results from ACCMIP and AeroCom. Moreover, the simulated contributions of individual aerosol components to the global mean optical depths at 550 nm are now compared with results from the MACC reanalysis. The simulated burden and optical depth of nitrate are compared with results from the HadGEM2-ES climate model. Three new tables have been included in the revised manuscript (Tables 6, A3, and A6).

11. "In a number of instances, there are differences between the offline and online simulations (e.g. CO lifetime, chemical destruction of CO, CO burden, as examples from Section 4.4). It would be useful to establish whether some of these differences (and those in other sections) are statistically significant and at what confidence interval."

For the most relevant quantities we will include standard deviations (calculated from the interannual variability) to indicate if the differences between the simulations are statistically significant or not.

12. "Finally, there are a number of instances in the manuscript, where the comparisons between simulations or comparisons between simulations and observations could be

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made more quantitative. As an example, in Section 4.1 (pg 1952, line 11), cold and warm biases in EC-Earth are discussed but there is no detail in the text on how large these biases are and in which seasons they apply? The same is also applicable in Sections 4.3, 4.4, 4.5, and 4.6 - the inclusion of quantitative measures of skill in the manuscript will provide a useful benchmark against which successive model improvements can be assessed."

We will adapt Section 4.1 and see if we can include more quantitative measures in Sections 4.3-4.6 as well.

Technical Corrections:

1. "Use of sulphate/sulfate, please use sulphate consistently throughout the manuscript (e.g. Section 2.2.5, line 12). The same applies with sulphur/sulphur (e.g. Section 2.2.8, line 20)."

We now write "sulphate" and "sulphur" consistently througout the manuscript.

2. "Section 2.2.5, line 18, replace "optical properties fields" with "optical properties""

We have made the change.

3. "Section 2.2.5, line 20, replace "aerosol nitrate" with "nitrate aerosol"."

We have made the change (twice).

4. "Section 3, line 28, replace "operation" with "operational"."

We have made the correction.

5. "Section 4.1, line 8, the year of the Hazelenger et al. reference not consistent with bibliography."

We have corrected the year of publication to 2012.

6. "Section 4.2, line 10, replace "on the NH in the lower parts of the troposphere" with "in the NH lower troposphere"."

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We have changed the text as suggested.

7. "Section 4.3, line 6, replace "Assuming a lifetime of 120 and 160 years for respectively the chemical loss" with "Assuming a lifetime of 120 and 160 years, respectively, for the chemical loss"."

We have changed the text as suggested.

8. "Section 4.3, line 9, "lifetime of CH4 is 9.4 years" with "lifetime of CH4 of 9.4 years"."

We have made the correction.

9. "Section 4.4, lines 1-7 on page 1958, replace "on the SH" with "in the SH" and "on the NH" with "in the NH"."

We have made these changes throughout the manuscript.

10. "Section 4.5, line 14, replace "sources of ozone in troposphere" with "sources of ozone in the troposphere"."

We have made the correction.

11. "Section 4.5, line 21, Table 6 mislabelled. Should be Table 5."

We have corrected the number.

12. "Section 4.5, lines 2, 11, 18, the term "resp." is misplaced. For example, on line 2, replace existing line with "outside the ranges of 22.3 $\pm$ 2.0 days and 22.2 $\pm$ 2.2 days estimated by Stevenson et al. (2006) for the full ensemble of ACCENT models and a subset of models, respectively.""

We have changed the text as suggested and corrected the use of "resp." throughout the manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 7, 1933, 2014.