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Geoscientific Model Development Discussions

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

Anonymous Referee #1

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Review of "Simulation of tropospheric chemistry and aerosols with the climate model EC-Earth" by T. P. C. van Noije et al. Submitted for publication in Geoscientific Model Development

General Comments: The submitted manuscript by van Noije et al. describes the incorporation of the well-established chemical transport model TM5 (with descriptions of gas-phase chemistry and aerosols) into the global climate model EC-Earth, whose atmospheric model is that of the European Centre for Medium-Range Weather Forecasts (ECMWF), the Integrated Forecasting System (IFS). The paper initially describes the main components of the IFS and the chemistry and aerosol components of TM5. It then sets out how the coupling and/or data exchange between IFS and TM5 is handled. The main focus of the paper, however, is on evaluating the chemistry and aerosol

performance of a present-day simulation of TM5 coupled to IFS, where the coupling is one-way i.e. chemistry and aerosols themselves do not feed back onto the dynamics of the IFS. To aid in this evaluation, parallel simulations with TM5 driven by ECMWF reanalysis ERA-Interim are used.

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". However, on balance, once the specific comments are adequately addressed, the paper will be a useful addition to the scientific community in documenting the development of the EC-Earth climate model and will be wholly suitable for publication in Geoscientific Model Development.

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.

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.

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.

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?

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.

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.

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

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?

9. In Section 4.4, the offline and online simulations underestimate observed concentra-

tions 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.

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 (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.

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.

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

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).

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

3. Section 2.2.5, line 20, replace “aerosol nitrate” with “nitrate aerosol”.

4. Section 3, line 28, replace “operation” with “operational”.

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

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

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”.

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

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”.

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

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

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.”

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