

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 thank the reviewer for the positive review and his/her constructive comments.

Concerning the reviewer’s remark that it is impossible to conclude from the paper if the EC-Earth model is reliable for its purpose because the precision requested for the future application are not introduced, we would like to point out that the formulation of such requirements would be a very difficult task. EC-Earth is used for seasonal and decadal predictions as well as for long-term climate simulations. Each of these applications would have different requirements in terms of the representation of the atmospheric composition and short-lived climate forcers. For long-term climate simulations it is important that the concentration changes of greenhouse gases and aerosols and the associated climate forcings are captured. The evaluation presented in this

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paper focuses on the model's ability to simulate chemistry and aerosols under present-day conditions. Evaluation of the fully coupled model over longer time scales is beyond the scope of this paper. Another complicating factor is that a tuning procedure will be applied before the model will be used to make climate projections. This will reduce or remove some of the biases in the simulated climate, also those introduced by biases in concentrations of trace gases and aerosols.

The reviewer also points that "the discrepancies between the two configurations of the model should be discussed more by comparisons with recent results of model intercomparison projects available in the literature and easily accessible on databases."

We have extended the comparison of results from both simulations with estimates from the recent literature, and included results from recent model intercomparisons and the MACC chemical reanalysis. Global budgets, burdens and lifetimes of different aerosol components, precursor gases (DMS, SO₂, NO_y and NH_x) and total reactive sulfur are now compared 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).

Below we address the specific points raised by the reviewer. The reviewer comments are given between quotes.

Abstract

"The 2nd sentence is technical and too long. Is it really a two-way data exchange?"

We have simplified the sentence by replacing "integrated forecasting system (IFS) model" with "IFS model". Yes, we do describe the two-way exchange between TM5 and IFS in this paper.

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"The aim of this model is never explained neither in the abstract nor is suggested by the intriduction but never clearly exposed. However, the importance of bias can only be apreiciated in regard with the use/applications of the model. Two versions of the model are compared and The last 3 sentences should be in the reverse order."

It is mentioned in the Abstract that the work presented contributes to the development of a climate model, and is a first step in the development of an Earth system model with fully interactive atmospheric chemistry and aerosols. EC-Earth has various applications ranging from seasonal to decadal predictions to long-term climate simulations (see Hazeleger et al., 2012). We have added a sentence in the Introduction in which this is mentioned.

The last sentences of the Abstract follow the structure of the paper. The first sentence concerns the oxidizing capacity and the methane lifetime, for which no direct observations exist. Then we mention the fields that have been compared with observations. We have included the word "further" to make the connection between the two sentences more clear. We want to finish with a general statement putting the work into the broader context of ongoing developments.

1. Introduction

"p1936, l3-5, please shorten/clarify the sentence"

The sentence refers to the well-known difference between the abundance-based and emission-based view of radiative forcing of climate. We believe this is an important point to make, but to explain why would be too much for this Introduction. Details can be found in the references given in the sentence. We don't think the sentence is too long or unclear.

"p1937 l 22, 'crucial' is a too emphatic term"

We have replaced "crucial" with "important".

"l28, remove 'fully'"

OK, we have removed "fully".

"p1938, l1, 'fully' should be removed"

OK, we have removed "fully".

2. Model Description

"P1939, l5, '31r1' should be '(version 31r1)'"

We believe there shouldn't be any parentheses here. For the IFS model we prefer to stick to the ECMWF terminology and refer to model versions as "cycles".

"L20, the aerosol forcings is mentioned whereas the version presented is one way coupled."

The one-way coupling implies that aerosol forcings are prescribed as in EC-Earth v2.3. In this sentence it is mentioned that the aerosol forcings used in EC-Earth v2.3 are consistent with the CMIP5 recommendations.

"P1940 , l15 and 22 : Please explain how the horizontal resolution of surface deposition and emissions can be more precise than the horizontal grid of the model ?"

This is explained in the sentence above l22: "Dry deposition velocities and surface emission fluxes that depend on local meteorological conditions and/or other surface variables are calculated on a higher-resolution surface grid and subsequently coarsened to the atmospheric grid." This can be done because the surface meteorological fields and other properties of the surface are available at the resolution of the surface grid.

"L26, please precise the altitude of the top of the model."

We now mention that the top of the model is at 5 hPa.

"P1943, l 11-14 : 'is designed to simulate. . .', the fact that is allows to take into account emission at the surface and in altitude should be mentioned"

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This sentence merely introduces the TM5 model version applied in this study. The emissions applied in the model are described later on (Sect. 2.2.8). There it is mentioned that, depending on the emission source, the emissions are applied at the surface and/or at higher altitudes.

"P1944 I1-2 and I8-9 : the future development of the model should be merged in a dedicated section."

We have removed these sentences. Future developments of the model are now discussed in a separate paragraph at the end of the "Discussion and conclusions" section.

"P1946, I1-2 : The stratospheric chemistry is not only the photodissociation of O₂ otherwise it should be easy to add to the model. Probably the term 'description of stratospheric chemistry is not included in the model'"

The reviewer is right. We have replaced the two first two sentences of this section with "A detailed description of stratospheric chemistry is not included in the model."

"P16947 I17 'CMIP5 dataset' is only technical jargon (actually it was provided for AC-CMIP, which was afterward used for CMIP5). Maybe the term 'taken from the CMIP5 dataset' could be removed."

We have replaced the sentence with "Emissions from anthropogenic activities and biomass burning are taken from the dataset provided for the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP), which was also used in CMIP5."

3. Simulations

"p1951 I.8 : 'offline' should be 'offline' (maybe due to pdf processing)"

In our document it says "offline".

4. Evaluations

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"General remark on evaluation : in the MACC-II framework, reanalysis of the atmospheric composition are provided combining model and satellite data, why not compare such climatological reanalysis to these model outputs ?"

In the revised version of the manuscript have added a comparison of the simulated contributions of the different aerosol components to the AOD at 550 nm with estimates from the MACC reanalysis. It should be realized that the MACC results on individual AOD contributions are model dependent, because only total AOD is assimilated in the reanalysis. We didn't include a comparison of total AOD with the results from the MACC reanalysis, because we already compared our total AOD fields with MODIS satellite retrievals.

"No satellite data was used to assess the realism of the fields whereas such data are the most suited to evaluate global chemical fields (IASI, MOPITT, GOME, etc) even in a qualitative manner."

We did validate the simulated AOD fields with satellite retrievals from MODIS.

"For many comparisons, the authors mention that the values can be compared to reference in the literature (Spivakovsky for OH, Stevenson or Young for O₃. . .) but never mention explicitly the range of values presented by these authors, it is thus difficult to believe it without doing the comparisons by ourselves."

To make the comparison with the observations-based OH estimates from Spivakovsky et al. more direct, we have added two new panels in Fig. 3, which show their zonal mean OH concentrations reduced by 8%.

When quoting estimates from published references we did give the uncertainty ranges provided by these authors, e.g. for methane lifetime by Prather et al. (p. 1956, l. 11), ozone lifetime, burden, STE flux and burden by Stevenson et al. (p. 1959, l. 2, 11, 18-19, and p. 1960 l.2) and ozone burden by Young et al. (p. 1959, l. 12).

"Please remind the you can only compare 10-year means due to the interannual climate

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variability which can not match the real one in the coupled mode. In the figure's legend, please precise when these are 10-year means."

This is exactly why we only evaluate the 10-year means from the simulations. When comparing the different simulations and the observational datasets we also indicated the standard deviations based on the interannual variability (line plots) and if the results are significantly different at the 5% level (contour plots). This is explained at the beginning of the Evaluation section (Sect. 4).

"P1954, I14 : The role of OH initiation in tropospheric chemistry was not discovered in 2002, 2004 by Lelieveld. For such general knowledge, please refer to original literature."

We have included a reference to the 1971 Science paper by Levy.

"P1957, 1st paragraph, The sentence 'In order words. . . Shindell et al.2006' is confusing since CH4 emissions are not considered in the simulations."

The reviewer is right. To clarify this sentence we have replaced 'effectively lower CH4 emissions' by 'lower effective CH4 emissions'.

"P1957, I6 'This IS likely. . .'"

Thank you. We have corrected it.

"P1958 I 21 : 'Table 6' should be 'Table 5'"

Thank you. We have corrected it.

"P1962 : The difference between the 2 versions of the model and the data should be discussed compared to the multimodel gap for ozone recently dscribed in Stevenson and al. 2013 (ACP)."

We will include a brief comparison with the multimodel gap found by Stevenson et al.

"P1966, I19-25 : the attribution of the underestimation to dust should be illustrated by

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showing the contribution of each type of aerosols to the AOD. This section is really succinct whereas we can imagine that climate issues will be addressed with the ECEarth model."

We have added maps of the vertically integrated burdens of the individual aerosol components (including mineral dust) as well as of their contributions to the AOD (Figures 12 and A1 of the revised manuscript).

"P1968, l14-15 : in fact the scope of the paper is not enough clear to justify no to enter in the details of aerosol distributions. With existing intercomparison projects like AEROCOM, it is quite easy to discuss even briefly how aerosols are simulated."

In addition to the new figures we have also added global budgets, burdens and lifetimes of the different aerosol components (and precursor gases) and compare the results with (multi-model) estimates from ACCMIP and AeroCom, as well as with AOD estimates from the MACC reanalysis. Three new tables have been included in the revised manuscript (Tables 6, A3, and A6).

"P1971, l8-30 : A section dedicated to the future development of IFS and EC-Earth should be created, separated from the discussion."

The discussion on p. 1971, l. 8-30 is about the ongoing, parallel development of C-IFS, and merely discusses the pros and cons of both approaches. A description of plans for the future development of IFS is beyond the scope of this paper.

"Finally, we don't know if the skills of this new model are sufficient enough to address the issues for which it is developed and why it is evaluated now if the development is not finished."

We believe the presented developments justify a proper documentation of the system. Also, we believe it is useful to first present an evaluation of the atmospheric chemistry and aerosol simulation in a one-way coupled configuration with the same climate model as used for CMIP5, before evaluating the two-way coupled system.

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"References P1976, Gillette et al, the year is missing."

Thank you. We have included the year (2003).

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