Response to reviewer 1

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 in italic font below.

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 SO_2 by O_3 and H_2O_2 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 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 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 focused 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.

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 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".

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 ± 2.0 days and 22.2 ± 2.2 da 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.

Response to reviewer 2

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

In response to the reviewer's main comment/concern related to frequency of the coupling (i.e., 6-hours) between EC-Earth and TM5 and how this frequency affects the chemical results, we would like to point out that the internal time step of TM5 is much shorter than the coupling time step, which only sets the data exchange between IFS and TM5. Details are given in our answers to the reviewer's specific comments below.

Abstract, Line 13. Please don't use "which likely reflects an". It either does or doesn't or should be mentioned.

We have removed "likely".

Abstract, Line 18. I would add the value of the CH4 lifetime for EC-Earth, and then discuss the % change. You should state that this is the total atmospheric lifetime (vs that with OH).

We have changed the sentence to "The atmospheric lifetime of methane in EC-Earth is 9.4 years, which is 7% longer than the lifetime obtained with ERA-Interim but remains well within the range reported in the literature."

Page 1937, line 29. Another very good reference is Lamarque, J-F, L. K. Emmons, P. G. Hess, D. E. Kinnison, S. Tilmes, F. Vitt1, C. L. Heald, E. A. Holland, P. H. Lauritzen, J. Neu, J. J. Orlando, P. Rasch, and G. Tyndall, CAM-chem: description and evaluation of interactive atmospheric chemistry in CESM, Geosci. Model Dev., 5, 369-411, doi:10.5194/gmd-5-369-2012. (also in GMD).

We have included this reference.

Page 1938, line 15. "Decadal simulation. . . for present-day conditions". Is this a perpetual 2000 type of simulation, or do you run from 2000 through 2009?

The setup of the simulations is explained in Section 3. The simulations are listed in Table 2.

Page 1939, line 4. "Version 2.3" The abstract denotes the EC-Earth version as "2.4"?

This was indeed a bit confusing. As stated in the abstract, the integration of TM5 is part of EC-Earth version 2.4. However, the atmosphere-ocean GCM used in our study is the same as in version 2.3. For this reason we refer to version 2.3 (the version used for CMIP5) in our description of the atmosphere-ocean GCM (Section 2.1). To clarify that TM5 is part of version 2.4 we have included the following sentence in Sect. 2.2: "The new model configuration with TM5 has been released as part of EC-Earth version 2.4".

Page 1939, line 4. "IFS model cycle" What is a model cycle?

This is the standard terminology used by the ECMWF (see http://old.ecmwf.int/research/ifsdocs/).

Page 1940, line 15. "3x2 degrees, 31 levels" Why such coarse resolution, especially since the EC model is run at 1.125 degrees? Also, is it really necessary to decrease the vertical levels to 31 (from 62 in the IFS)? If this were just about numerical cost, it would be nice to state this. If there is a technical reason why you've decided to have different horizontal and vertical resolutions that also would be important to state.

The reason for the relatively coarse resolution is indeed to reduce the numerical cost. 3x2 degrees is the standard resolution of TM5 for global chemistry applications, and the 31 levels are similar to the standard setup for offline simulations of tropospheric chemistry. We will clarify this in the text.

Page 1941, line 5. "data exchange between TM5 and IFS is set to 6-hours" In most CCMs, the chemistry is inline and the chemical constituents are modified at least every hour (most less than that). Does this mean you calculate photochemistry just 4x per day?

No, the internal time step in TM5 is much smaller than the exchange time. The operator splitting and time stepping applied in the model is the same as in the offline version of TM5 and has been described by Huijnen et al. (2010). A time step of half an hour is used for most processes, including the calculation of the photolysis rates. The chemical differential equations are solved using a time step of 15 minutes.

Page 1942, section 2.2.3 Transport. The TM5 advection routine can use two schemes and it is stated that model simulations presented in this work used the Russell and Lerner (1981) approach. What is not stated is what the advection routine is in ECEarth. One can imagine when the EC-Earth / TM5 is fully coupled (not just one way as shown in the paper), that this could cause issues. In the future, do the authors plan to have the advection handle by the climate model in the fully coupled configuration?

Also in the two-way coupled system, the tracer transport will be done in TM5. Transport in TM5 is mass conserving, while IFS uses a semi-Lagrangian advection scheme, which by itself does not conserve mass. As mentioned in the “Discussion and conclusions” section, this is an advantage of our system compared to C-IFS, in which the transport takes place inside IFS.

Page 1947, line 25. "linear interpolation" I believe you mean you are doing a linear interpolation of the seasonal cycle for the emissions?

Correct. We have now clarified that the RCPs are provided monthly emissions and adapted the text as suggested.

Page 1956, lines 5-12. It would also be nice to report the lifetime with respect to OH (see Prather et al., 2012). In addition, adding the observed OH distribution (based on Spivakovsky et al., 2000) to the figure 3 would be interesting.

We have added the lifetime with respect to OH, and included two panels in the figure showing the OH distribution based on Spivakovsky et al. (2000).

Page 1958, line 21. "Table 6" You mean "Table 5"?

Thank you. The number has been corrected.

General comment on the ozone section: It is a bit on the long side, especially compared to the discussion of OH and CO. You may want to make this section more concise. Also, it would be nice to know why the STE is lower in both model versions (relative to other published studies). Is this a direct result of too little ozone in the lower stratosphere or a dynamics/transport issue?

We tried to carefully description of the differences between the simulations and the observational datasets. For that reason we prefer to keep the section as it is.

As described in Sect. 2.2.7, the vertical profile of ozone in the stratosphere is relaxed to a climatological dataset constructed from sonde and satellite observations. Thus, ozone concentrations are constrained to realistic values at altitudes above ~ 100 hPa in the extratropics. Biases in the lowerlying parts of the stratosphere must therefore be caused by transport issues. A detailed analysis of this is beyond the scope of this paper.

Page 1972, lines 5-6. "photolysis rates" Please be specific in this work how often these rates are updated for the present version and future versions. I.e., are you really only updating photolysis rates every 6 to 3hours?

No, the photolysis rates in the current configuration are updated every 30 minutes, as in the offline version of TM5 at the same resolution. The operator splitting and time stepping applied in the model is described in the paper by Huijnen et al. (2010).

Response to reviewer 3

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 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 out that “the discrepancies between the two configurations of the model should be discussed more by comparisons with recent results of model intercomparison projets 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, NOy and NHx) 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.

Abstract

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

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.

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 apreciated 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

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 mention that, depending on the emission source, the emissions are applied at the surface and/or at higher altitudes.

P1944 l1-2 and l8-9 : the future developpement 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, l1-2 : 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 l17 'CMIP5 dataset' is only technical jargon (actually it was provided for ACCMIP, 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 l.8 : 'offlne' should be 'offline' (maybe due to pdf processing)

In our document it says 'offline'.

4. Evaluations

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 reanalyis, because we already compared our total AOD fields with MODIS satellite retrievals.

No satellite data was used to assess the realism of the fieals 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 litterature (Spivalkowsky for OH, Stevenson or Young for O3. . .) but never mention explicitely 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 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, l14 : The role of OH initiation in troposheric chemistry was not discovered in 2002, 2004 by Lelieveld. For such general knowledge, please refer to original litterature.

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, l6 'This IS likely. . .'

Thank you. We have corrected it.

P1958 l 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, l19-25 : the attribution of the underestimation to dust should be illustrated by 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 amospheric 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.

References P1976, Gilette et al, the year is missing.

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