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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.

Anonymous Referee #3

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This manuscript describes the considerable work performed to develop a coupled model suited to chemistry-climate interaction modelling. The paper aims to evaluate the one-way coupled configuration mainly by comparing the outputs of the new version to an offline simulation performed in a similar framework. The paper is clear and the technical description of the model and simulations is enough detailed. However the precision requested for the future application are not introduced and does not allow to conclude if the EC-Earth model is reliable for its purpose. Furthermore the discrepancies between the two configurations of the model should be more discussed by comparisons with recent results of model intercomparison projets available in the literature and easily accessible on databases.

The small revisions are following:

Abstract The 2nd sentence is technical and too long. Is it really a two-way data echange ? 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 Âń in regard with Âż the use/applications of the model. Two versions of the model are compared and The last 3 sentences should be in the reverse order.

1. Introduction p1936, l3-5, please shorten/clarify the sentence p1937 l 22, 'crucial' is a too emphatic term l28, remove 'fully' p1938, l1, 'fully' should be removed

2. Model Description

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

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

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 ?

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

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.

P1944 l1-2 and l8-9 : the future developpement of the model should be merged in a dedicated 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'

P16947 l17 '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.

3. Simulations p1951 l.8 : 'offlne' should be 'offline' (maybe due to pdf processing)

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 ? 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. 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. 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.

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.

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

P1957, l6 'This IS likely...'

P1958 l 21 : 'Table 6' should be 'Table 5'

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).

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 EC-Earth model.

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.

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

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.

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

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