

Interactive comment on “A new chemistry-climate tropospheric and stratospheric model MOCAGE-Climat: evaluation of the present-day climatology and sensitivity to surface processes” by H. Teyssède et al.

Anonymous Referee #2

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Referee comment: "A new chemistry-climate tropospheric and stratospheric model MOCAGE-Climat: evaluation of present-day climatology and sensitivity to surface processes" by H. Teyssède, M. Michou, H. L. Clark, B. Josse, F. Karcher, D. Olivie, V. -H. Peuch, D. Saint-Martin, D. Cariolle, J. -L. Attie, P. Ricaud, R. J. van der A and F. Cheroux.

This article describes results from a configuration of the MOCAGE model designated MOCAGE-Climat which is designed to study global distributions of chemical species and can be used to study chemistry-climate interactions. A presentation of the details

of the model and a comparison of results from the model and measurements is appropriate for ACPD.

A major concern I have is that in a number of places within the text, including the title, the model is described as a chemistry-climate model and yet all the results shown in the paper are from the model run as an off-line, chemical transport model. I would suggest that the title and description of the model is therefore misleading and could cause confusion for the reader.

I believe it is generally accepted that the term chemistry-climate model refers to models of the kind described in Eyring et al. 2006 where a chemistry scheme is fully coupled to a GCM or at least an off-line chemistry scheme driven by a GCM. You do not present results from a model of this nature in this paper and therefore the references to MOCAGE-climat as a chemistry-climate model should be altered.

To be clear, the model you introduce here should be described as a global tropospheric and stratospheric CTM because this is how the model was run to generate the results you show in the paper. By all means dedicate a paragraph (or more) in the introduction to a discussion of how the same model (MOCAGE-Climat) can be coupled to the ARPEGE-Climat GCM and thus be run as a chemistry-climate model but please do not confuse the results presented here with results from chemistry-climate models like those described in Eyring et al. or chemistry + GCM models.

A clear explanation is required as to why some figures (and related text) refer to the T21 run and others only to the T42 run. For example, figures 3, 8, 10, 11, 13, 14, 15 and 16 show comparisons between observations and the T21 model only. Similarly figures 4, 5, 9 and 12 show only obs and T42 results. Without any explanation, the reader is left wondering why a particular configuration was chosen for some comparisons and not others. It is obviously cumbersome to show all model results for all comparisons but as it stands, without any explanation, the choices seem somewhat arbitrary.

Figure labelling. Many of the figure labels are very small and therefore difficult to read

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either on screen or in hard copy. e.g. pressure (height) and latitude axis labels and axis titles in figure 1. Please make an effort to make all figure labels legible.

Abstract

Line 1. I would argue that the results presented in this paper do not come from a chemistry-climate model but rather a full atmosphere CTM.

line 13 '... stratosphere putting aside the shortcomings associated with the ...'

line 15 'However, because the Brewer-Dobson circulation is too fast, too much ozone is accumulated in the ...'

line 20 '... layer lead to larger ozone differences ...'

line 22 '... hemisphere, which may result from ...'

Introduction

Again more references to chemistry-climate model. (Page 4, line 3; page 3, line 22; page 4, line 15).

Lines 2 - 4 of page 4 you state that the model can be used for a range of scientific applications including the study of chemistry-climate interactions and you cite 3 papers (Cathala, Josse and Pradier). I fail to see where any of the three papers cited discuss chemistry-climate interactions. Two are data assimilation papers and the third describes the transport of Rn222. In my opinion the phrase chemistry-climate interactions implies the study of the fully coupled feedbacks between atmospheric chemistry change and climate change. None of the papers referred to here do this.

Page 3, line 4 'Then, simplified chemistry of ...'

Page 3, line 6 '... McLinden et al. (2000). Large scale ...'

Page 3, line 7 '... of the atmosphere were thus taken ...'

Page 3, line 13 '...Chipperfield et al. 1994). The stratospheric composition ...'

Page 3, line 14 'time-scales. For example the REPROBUS (lefevre et al. 1994) has ...'

Page 3, line 16 '... atmospheric chemistry, including the modelling ...'

Page 3, line 20 '... accounted for, but this substantially ...'

Page 4, line 9 '... Dufour et al. (2004)), and ICARTT/ITOP ...'

2 Model Description

Section 2.1 General Features

Can you please be more clear with your description of the modelling of water vapour (page 6, first paragraph). Water vapour from the ECMWF analyses are used below 380K, Above this, is the water vapour determined by the REPROBUS scheme? If so, why the simple ECMWF parameterisation of water vapour production important?

Page 7, line 11 '... to the surface. Firstly we calculate ...'

Page 7, line 20 '... from simulations both with the full boundary layer chemistry calculations included and approximating the boundary layer as a single bulk layer.

Section 2.2 Surface exchanges

Page 8, line 14 Is the reference year really supposed to be 1895?

Page 9, line 5 '... 7 Tg(N)yr⁻¹). Implimentation ...'

Section 3 Observatons used for the evaluation

Given that this is a long paper I would strongly encourage you to explore the possibility of moving these sections to a supplementary material file. The table (5) summarising the salient features of the data sets and a relevant reference for each data set in the main paper suffices if these sections could be moved to a supplementary material section. This would help reduce the length of the paper.

Page 10, line 6 'The main characteristics of the observational datasets used is pre-

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sented below. Satellite and in-situ observations for ...'

Page 11, line 20 '... technique making daily observations of up to ...'

Page 12, line 24 '... analyzed using version V222 up to ...'

Page 16, line 8 '... 3-month periods to study the seasonal variability ...'

Section 4.1 Description of the simulations performed

Page 17, line 13 'The MOCAGE-Climat model was run using the ... ECMWF (IFS). The model was integrated for six years using the same ...'

Page 17, line 18 '... simulations have been performed, noted in ...'

Page 17, line 23 'This climatology represented conditions of the mid-90s and was adapted for the year 2000 by, for example, reducing the mean chlorine ...'

Page 18, line 15 '... simulations had been completed to evaluate ...'

Section 4.2 Age of air

Page 18, line 24 '... Dobson circulation (cite?), mixing across latitudes and vertical diffusion. Mixing across latitudes is highly ...'

Page 19, line 10 '... the age spectrum. A simple tropospheric tracer continually emitted with a linear growth in time can be used to diagnose the model's mean age of air (Hall and Waugh, 1997).

Page 29, paragraph 2. 'MOCAGE-Climat simulations ...' It is not realistic to describe the age of air only in terms of the Brewer-Dobson circulation. It is well known that cross-latitude transport distorts the idealised picture of the B-D circulation in the real atmosphere and in models. To me, the second paragraph of page 19 suggests that the mixing of air out of the tropics seen in the MOCAGE results is unique rather than something you would expect to see in model results and in the real atmosphere. I think this whole paragraph can be deleted without compromising the discussion of the age

of air.

Page 29, line 18 'The zonal distribution (see figure 1) shows the AOA to ...'

Page 29, line 22, '... altitude, AOA derived from measurements ranges from ...'

Page 29, line 26 '... the transport is significantly too fast ...'

Section 4.3 Numerical stability of simulations

I agree that testing the stability of the global integrated ozone amount is a simple first order test of the model chemistry but as you point out, this type of test is a rather simple and not terribly illuminating with respect to the details of the model chemistry. This is a long paper and so I would think you should consider removing this section and figure 2. A short paragraph discussing on the results of your stability test, perhaps in section 4.1, would be sufficient. Figure 2 with a short explanation could perhaps be added as supplementary material.

Section 4.4 Long-lived species

Page 21, line 27 '... photo-dissociation rates as they determine ...'

Section 4.4.1 Methane (CH₄) and water vapour (H₂O)

Why do you not show stratospheric water vapour results given that (a) water vapour is an important component of the stratospheric system (b) there are good climatologies of measured stratospheric water vapour (c) water vapour comparisons can provide interesting insight into the chemistry, transport and microphysics (dehydration) of a model.

Page 22, line 3 '... on the atmosphere. It interacts ...'

Page 22, line 10 '... that extrapolations into the future are difficult. The ...'

Page 22, line 24 '... observations, with relative differences less than 10% ...'

Section 4.4.2 Carbon monoxide (CO)

Page 23, line 13. You state that CO affects the three most important greenhouse gases. You also state on page 26, line 17 that N₂O is one of the three most important GHGs. Perhaps you mean to say here that CO affects two of the most important GHGs (CO₂ and CH₄)?

Page 23, line 19 '... in the troposphere on a global scale ...'

Page 24, line 6 '... information are shown. The 700 hPa level ...'

Page 24, line 27 'The time versus latitude diagrams (figure 4) at 700 hPa ...'

Page 25, line 1 '... already mentioned. The duration of this feature is also too long. Between ...'

Page 25, line 19 '... of CO emissions. The same emissions were used in this study. It ...'

Page 25, line 23 '... MOCAGE-Climat suggests that emissions ...'

Page 25, line 27 '... are mainly provided by MOZAIC ...'

Page 25, line 28 '... simulations to obtain model output coincident ...'

Page 26, line 11 '... the T21 simulation. This is most likely due to the ...'

Section 4.4.3 N₂O

Features of the analysis of N₂O are similar to CH₄ and water vapour. It would seem to make sense to swap sections 4.4.2 and 4.4.3 so the N₂O section follows immediately after the CH₄ and water vapour section.

In section 4.2 you show that the mean age-of-air is significantly too low in your model runs (both T21 and T42), a problem common to other CTMs driven by ECMWF dynamical fields. In the last paragraph of section 4.4.3 you state that MOCAGE-Climat reproduces the observed N₂O distribution well and that this is determined largely through the Brewer-Dobson circulation but as stated earlier, the Brewer-Dobson circulation is

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significantly too fast. There appears to be an inconsistency here. Given that the B-D circulation is too fast does this mean the destruction of N₂O is also too fast in the model? You say on page 27, line 8 that the photodissociation rate is reasonable.

Page 26, line 19 '... burden has increased from ...'

Page 27, line 9 Smoother is what sense? As a function of latitude?

Page 27, line 11 '... stratosphere exchange sometimes takes ...'

Section 4.4.4 Total nitrogen oxides (NO_y)

You could perhaps more consisely describe the NO_y family as all oxides of nitrogen excluding N₂O.

Comparisons with MOZAIC are constrained to a limited altitude range - an altitude range where the NO_y mixing ratios are relatively low. Have you considered other data sets derived from observations which would allow a greater altitude range to be compared? Park et al. (Park, J. H., M. K. W. Ko, C. H. Jackman, R. A. Plumb, J. A. Kaye, and K. H. Sage (Eds), Models and measurements intercomparison II, NASA/T-1999-209554, Natl. Aeronaut. And Space admin., Washington , D. C., 1999) recommends comparing modelled NO_y with the sum of HNO₃ and sunset NO + NO₂.

Section 4.5.1 Nitrogen oxides (NO_x)

Page 29, line 9 '... mixing ratios lower than a ...'

Page 29, line 13 '... ratio is almost zero due to ...'

Page 29, line 17 '... can be considered to be consistent with ...'

Page 30, line 3 '... 4.6.2, ClO in this region is mainly ...'

Page 30, line 11 '... burning appears similar in both ...'

Page 30, line 19 'This is consistent with the overestimation ...'

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Page 30, line 28 '... simulations lead to large NO₂ column biases during the ...'

Section 4.5.2 Chlorine monoxide (ClO)

Page 32, line 3 '... values at approximately 3 hPa ...'

Section 4.5.3 Hydroxyl radical

Page 32, line 15 '... major misunderstandings about the ...'

Page 32, line 19 The Spivakovsky data set is not an 'observations' data set. I would describe it as a model data set constrained by observations.

Page 33, line 4 use e.g. rather than i.e.

Page 33, line 5 '... and NO₃. Ren et al. (2007) found that ...'

Page 33, line 8 '... reactions with OH were suppressing the ...'

Section 4.6.1 Nitric acid (HNO₃)

Page 34, line 12 '... outputs corresponding to average profiles over the region ...'

Page 34, line 14. The sentence starting 'However, simulations ...' is too long and not well worded.

Page 34, line 23 '... of HNO₃ is consistent with the ...'

Page 34, line 24 '... as HNO₃ is predominantly produced by ...'

Section 4.6.2 Chloridric acid (HCl)

I believe using hydrochloric acid rather than chloridric acid would be more appropriate.

Page 35, line 25 '... occuring on particles ...'

Section 4.7 Ozone (O₃)

Page 36, line 4 '...removed from catalytic cycles by being ...'

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Section 4.7.1 Total ozone column

Page 37. Comparing the results shown here with coupled chemistry-climate models is questionable. The CCMs described in Eyring et al are a very different type of model to the one described here. The age-of-air is underestimated in the majority of the Eyring models but the area and mass deficit of the ozone hole in particular, are also strongly related to the underlying GCMs description of the dynamical vortex e.g. most GCMs produce a vortex that is too small and there can be significant temperature biases at the poles in the GCMs.

Page 37, line 15. Are you suggesting that the bias in the MOCAGE-Climat modelled Antarctic ozone hole (not being deep enough) is related to an overestimation of the total global ozone column? If so, you show in figure 16 that it is in the lower vortex (Antarctic) in October that the model overestimates the ozone mixing ratio by up to 60%. I think this somewhat contradicts the comment above. To me the results for the Antarctic show that the model is underestimating the amount of ozone loss in the Antarctic spring within the vortex (most clearly shown in figure 16). There would seem to be an important point yet there is no comment as to why the model produces this bias, either in section 4.7.1 or 4.7.2.

Page 37, line 22 '... , inducing longitudinal variations ...'

Page 38, line 3 '...than in January and are lower ...'

Section 4.7.2 Stratospheric ozone

Page 39, line 11 A comment on the reason for this difference seems important particularly given the overestimation of ClO in the same region in July shown in figure 10.

Section 4.7.4 Tropospheric ozone

Page 41, line 8 'Wild (2007) explored the variability in current CTMs when simulating the ... budget demonstrated the importance of ...'

Section 4.8 Summary statistics

Page 42 lines 21, 22 and 27 replace 'dots' with 'points'

Page 43, line 8 '... statistical information concerning the model ...'

Page 43, line 10 '... simulations are shown (T21, T42 and T42BL1) as coincident outputs with aircraft observations. The correlation coefficient for NOy ...'

Why do you not show T21BL1 results here?

Page 43, line 14, replace 'dots' with 'points'.

Page 43, line 23 '... very similar to the observed variability and ...'

Page 43, line 25 '... T21 simulation produces slightly better ...'

Page 44, line 2 '... considered and a negative bias ...'

Section 5 Sensitivity to surface processes

Page 44, line 11 '... related to the response in the sink ...'

Page 44, line 15, '... Ganzeveld et al. 1998). We performed and ...'

Page 44, line 17 '... simulation except that the deposition ...'

Page 44, line 20 '... O3) is well defined (Michou et al. 2004). '

Section 5.1 Mixing ratios

Page 45, first line 'A summary of the main differences ... simulations with regard to mixing ratio is as follows; we ...'

Page 45, first paragraph. I find the use of 'relative difference' in place of 'maximum of the relative differences' confusing. On line 5 you say the highest relative difference - do you mean the highest of the maximum relative differences? Why does the average value (17%) on line 6 refer to? The average of the maximum relative differences or the

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average relative difference in the troposphere? It is also difficult to understand what the mean and maxima refer to for other species (NO₂, O₃ and OH).

Section 5.2 Dry deposition fluxes

Page 46, line 20 '... contribute to this unequal partitioning of fluxes between the hemispheres. Deposition ...'

Page 47, line 15 'Lamarque et al (2005) investigated nitrogen deposition using six CTMs. The total deposition ...'

Page 47, line 24 '... are lower than the mean model in Lamarque et al (2005); ...'

Page 48, line 6 '... large-scale models. Finally, Trebs et al (2006) ...'

Page 48, line 12 '... (see figure 22). In the case of HNO₃, ...'

Conclusions

Page 48, first line of the conclusions. 'global troposphere and stratosphere configuration' rather than chemistry-climate

Page 49, line 12 '... highlighted the strong non-linear linkages between the chemical ...'

Page 49, line 14 'A number of the discrepancies ... and the observations are likely to be related to ...'

Page 49, line 17, '... horizontal resolution. This results in too much ozone being accumulated in the lower ...'

Page 50, line 1 '... weak. In the case of inorganic chlorine species, model evaluation ...'

Page 50, line 8 'These results confirm the conclusions of Law et al. (2000) that the stratosphere ...'

Page 50, line 22, '... HNO₃ may also reflect ...'

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Page 50, line 23 '... in the lower troposphere, this would tend to generate too much oxidation in the model. This in turn would lead to a positive ...'

Page 50, last line. This sentence requires a reference.

Page 51, paragraph 2. It would be helpful to add the Stevenson reference on the second line, '... recent model inter-comparison exercises (Stevenson et al. 2006), though ...'

Page 51, lines 11-13. Suggest: '... versus on-line calculations had the greatest impact on HNO₃ and NO₂ in the troposphere; O₃ was impacted up to 800 hPa. Deposition fluxes differed locally by up to 20%.'

References

Bodeker et al. Diff. -> Discuss

Drummond and Mand. The year of publication is missing

Eyring et al. chemistryclimate -> chemistry climate

Table 1, species 8; Hydrochloric acid

Figure 7 caption. State which run of the model is displayed in this figure.

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