

Interactive
Comment

***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 #1

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General comments

The manuscript describes chemical-transport model MOCAGE-Climate, which includes tropospheric and stratospheric chemistry, and its comparison with different observation data. This subject is relevant to the scope of ACP. However, the manuscript is really hard to understand because even with magnifying glass I could not read most of the figures. In general, it makes an impression that there is no substantial contribution to the scientific progress. The manuscript just describes yet another realization of chemistry-transport model driven by too fast (as authors pointed out) ECMWF circula-

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Interactive Discussion

Discussion Paper

tion. The manuscript has many text-book style descriptions (sometimes they are not correct) and the analysis of the results is not convincing. In general, the manuscript rather resembles technical report, it is also too long and hard to follow. It seems that the authors simply had no time to properly analyze the results. I would suggest to resubmit the manuscript as [Technical note](#); after appropriate improvement of the figures and text.

Specific comments

1. Title is misleading. The authors introduced their model as chemistry-climate, but in fact they used chemistry-transport model (CTM) driven by the prescribed meteorology. It means that the two-way interaction between chemistry and climate is fully absent.
2. The review on the progress in chemistry-climate model (CCM) development is limited, many successful efforts were not mentioned at all. It seems that the authors do not properly understand the difference between CCM and CTM. I would add a short review concerning the limitations of the state-of-the-art CCMs in the representation of the tropospheric chemistry, its potential significance for the ozone and climate simulations and introduce the existing CCMs which includes more sophisticated description of the tropospheric chemistry (e.g., Shindell et al., 2006, ACP).
3. The aims of the work as stated in the introduction are not clear. The authors defined 2 main goals: (i) validation of the CTM MOCAGE-Climate aiming on future participation in CCMval activity; (ii) sensitivity of the model to the representation of the lower troposphere. I think it would be reasonable then to split the paper into two parts. It will make paper much more convenient for the reader. The experimental set-up does not fit the first goal. I do not see any reasons to use ECMWF circulation for 2000-2006 because first of all this period is not covered in full by the HALOE data and secondly the ECMWF circulation (as the authors mentioned) is not good for this particular model. I suggest to use the circulation from the appropriate GCM, which will be coupled in the future with MOCAGE-Climate.

4. Which correction scheme is used for the mass-correction in SLS transport. This question is very important for the model performance and the authors should explain what is behind their simple scheme.
5. Page 11301, line 4-10: Radiation is taken into account through the thermodynamical balance of the atmosphere; . What do the authors means? What kind of radiation (solar, terrestrial) do they consider?
6. Page 11301, line 4-10: Tables 1 and 2 presents the chemical species for the RELACS scheme, which species are included in the REPROBUS scheme?
7. Section 3: I think that the description of the well-known satellite data (e.g., NIWA, TOMS, UARS) can be eliminated or substantially reduced.
8. Section 4.2: The obtained ages of air (AOA) looks very low over the middle-to-high latitude area in comparison with Bregman et al.(2006) estimation using similar meteorological fields from ECMWF. It would be very important to explain this difference?
9. Section 4.4.1: The authors mentioned that CH₄ is a good tracer for the transport. Therefore it is not clear how the agreement of measured and observed CH₄ can be reasonably good if the applied circulation is definitely wrong. Some discussion and explanation of this are necessary. The same is true for N₂O. The comparison of the CH₄ and N₂O simulated with T42 version or/and with the GCM meteorology (the cases with different AOA) could be helpful to resolve this issue. I suggest to show also H₂O comparison which is very important for the analysis of the model performance.
10. Section 4.4.2: Figures 4, 5 are too small and unreadable, therefore it is hard to follow the analysis. Why T42 version (with the same vertical resolution) better resolve the tropopause?
11. Section 4.4.3: The ozone sensitivity to the nitrogen oxides was established long before Randeniya et al., 2002. I think, more appropriate reference should be used here. There are other minor sources (e.g., O(1D)+N₂=N₂O) of N₂O in the atmosphere, so

the surface emission is not the only source. I also do not agree with the statement in the last paragraph. I think, that good agreement of N₂O with observations cannot guarantee good agreement of nitrogen oxides. For example, in case of too strong upward transport and overestimated N₂O destruction by O(1D) the correct simulation of N₂O is possible, but the NO_y mixing ratio will be probably overestimated.

12. Section 4.4.4: The correct next step would be comparison of the NO_y in the entire model domain. The comparison of the NO_y near the tropopause (MOSAIC data) is not enough to validate simulated NO_y production. The last paragraph is not clear. From the Figure 3 the overestimation of N₂O in the lower stratosphere is not clearly seen, in May there is even underestimation of the N₂O. I do not see how the results from figure 3 conform the results obtained from the comparison with MOSAIC.

13. Section 4.5.1: I do not agree that NO_x control the ozone in the lower stratosphere (page 11324, line 10). The role of HO_x and ClO_x is very important. Lengthy discussion about NO₃ is not clear and can be eliminated or shortened. The contribution of NO₃ to NO_x at sunset should be small anyway. The author should explain their approach to the comparison of their daily mean with the HALOE NO_x measured at sunset. Probably they should add not only NO₃, but also N₂O₅ (the night reservoir of NO_x). The authors mentioned that the overestimation of the NO_x above the stratopause is due to underestimated ClO amount, but this statement is not convincing. I think, that ClONO₂ cannot be responsible, because its mixing ratio above the stratopause is less than 0.1 ppbv, while the overestimation of NO_x (from the Figure 8) is about 2 ppbv. The comparison of the total vertical NO₂ column with SCIAMACHY does not look reasonable. Probably it makes sense to skip it bearing in mind ongoing activity mentioned by the author. Probably, it is better to compare surface NO_x with in-situ measurements in Europe.

14. Section 4.5.2: I do not think that the role of ClO in the stratospheric chemistry and ozone hole formation was suggested by Farman et al. (1985). I would suggest reading some of the many papers devoted to the discovery and theory of the ozone hole. The last sentence of the first paragraph is wrong, the role of HO_x there is very important.

The authors claimed that the simulated ClO is too low, but I cannot see it in the Figure 10, which shows positive and also negative deviations. By the way, did the authors use equivalent latitudes for the comparison with Randel's data?

15. Section 4.5.3: It should be clarified that Spivakovsky et al. (2000) data have been obtained also from the model.

16. Section 4.6.2: The main source of HCl is the reaction $\text{CH}_4 + \text{Cl} = \text{HCl} + \text{CH}_3$, so HCl is the main chlorine reservoir almost everywhere. As was pointed out by the authors some overestimation of HCl compared to HALOE climatology should be expected due to the trend of organic chlorine in the troposphere. I would advise to use appropriate data or to repeat calculations covering HALOE period using GCM meteorology. I would also advise to compare model Cl_y with available observations as has been done by Eyring et al., 2006. It could help to understand the model deficiencies.

17. Section 4.7: The ozone is not the most abundant trace gas of the stratosphere, CO_2 mixing ratio (for example) is much higher. The photolysis of O_2 is not confined to the upper stratosphere. HOx are also very efficient for the ozone destruction. The last sentence of the first paragraph is not clear, what exactly the authors are trying to convey. I think this introductory paragraph can be easily eliminated.

18. Section 4.7.2: The deviation in the upper mesosphere is rather the result of the ozone diurnal changes. The ozone has a maximum during the night, which is absent in HALOE data. It would be interesting to compare only daily mean values, which should be closer. The chlorine related explanation is probably irrelevant, because the destruction of the ozone is mostly driven by HOx chemistry in the mesosphere.

19. Section 4.7.3: Hard to read because the difference between curves in Figure 17-18 is tiny and not clearly visible. I suggest to plot the difference between simulated and observed values.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11295, 2007.