

Interactive comment on “CAM-chem: description and evaluation of interactive atmospheric chemistry in CESM” by J.-F. Lamarque et al.

Anonymous Referee #1

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General comments

The authors document the implementation and performance of the atmospheric chemistry in the Community Atmosphere Model (CAM). They provide detailed information on parameterizations specific for the chemistry package, as well as the chemical mechanism. Special attention is given to the wet scavenging, where two different schemes are discussed. A key feature of the CAM-chem is that it can run in online and offline mode with respect to the meteorology, which warrants the use for climate studies. Furthermore various offline meteorological data streams can be used. The meteo-treatment is given considerable attention, as it deserves.

In the second part of the manuscript a range of observations (in-situ, aircraft, satellite) are used to assess the system performance and the differences between the runs with

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respect to the various chemistry schemes, wet deposition schemes and meteorological fields.

While the model description part is rather thorough and covering most aspects with sufficient detail, the model evaluation is more diverse and scattered. Although it is inevitable that only a global assessment of the system can be obtained, the authors sometimes give the impression to provide an imbalanced set of evaluations, which leaves open questions to the reader.

For instance, while many details are given on ozone, for CO and aerosols much less information on the various model performances is provided. Also not always results from the three simulation datasets are presented, but the reasons for that are not always clear. For instance, for the aerosol analysis no results obtained with MERRA meteorology are discussed.

No results from the online system are presented for the aircraft observations, as the authors correctly note that such evaluation is sensitive to the actual meteorological circumstances. Contrary, for the assessment of the different wet scavenging parameterizations the authors only compare results from the online scheme to the aircraft observations without further justification. Furthermore, the authors write that the two schemes behave similarly but this not clear from the figures, considering the occasional large differences between results for the two runs.

Also the indirect impact of the different wet scavenging parameterizations on, for instance, ozone, CO and OH would be interesting to see, but is completely omitted.

This makes it difficult to assess the impact of the various meteorological drivers, the different chemistry schemes, and the different wet scavenging parameterizations. For offline runs the authors recommend the use of the GEOS-5 meteorology (pp 2210, line 26), but this is not very clear from the model assessment other than ozone. Therefore a more thorough discussion on the general impact of the various meteorological drivers would be an desirable.

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Concluding I think this manuscript is suitable for publication in GMD, but in a revised version of the manuscript it would be good to see a more structured assessment of the differences between the three model runs.

Specific comments

pp 2209, line 20: What is the standard temporal resolution for the offline meteorological data?

pp 2211, line 25: The authors do not mention aerosol sedimentation. What is used here?

Pp 2214, l3 : Which vertical distribution of emissions is used? Specifically for volcanic SO₂ and biomass burning emissions the choice can be quite important.

pp 2215: Table 6 lists the various runs. The online run is performed from 1991- 2000. But e.g. from figure 5 it appears that data for this run is used up to 2010. Is this a typo in Table 6?

pp 2216, l3: “seems to indicate”: Can this be substantiated? For instance, is this statement in line with the assessment of the online meteorology in CAM?

pp 2216, l14: “. . .issues in emissions or chemistry.” Although this may be true, it is no proof that the meteorology is OK. One could actually claim the opposite: as chemistry and emissions are very similar, the substantial differences that can be seen in Fig. 4 for ozone seasonal cycle at 900 hPa over the SH point at differences in meteorology.

pp2216, l28: Table 8: It would be instructive to see the average ozone budgets separately for the GEOS5 and MERRA meteorology. Considering the general larger ozone concentrations in Fig. 4 and 5 for MERRA, I'm not convinced that those two model versions can simply be averaged. Also this would give an interesting indication of the variation that can be expected when using different meteorology. Furthermore, it is a bit puzzling to see that the ozone burdens in the offline and online runs are practically identical, while the net chemistry and also dry deposition is significantly different.

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It would be good to see the separate numbers for ozone production and loss, as reported, e.g., by Stevenson et al., 2005. How different are they for the various runs?

pp 2217, l14: "... were used to assess that the Neu scheme behaves similarly to the Horowitz scheme". I think this conclusion cannot be drawn from the figures presented in the appendix. From what I can see in the figures it seems that the Neu and Prather scheme leads to generally larger loss of HNO₃. Also H₂O₂ generally appears significantly lower. This all impacts on the tropospheric ozone production. Also I wonder how much the aerosol concentrations are affected by this change. So I think a more thorough, and quantitative evaluation is needed to assess the impact of the change in wet deposition scheme.

Pp 2217, l16: "Therefore we limit our analysis..." I understand that the online run is not most suitable for evaluation against aircraft observations (even though this system was used to assess the Neu and Prather wet scavenging scheme). Still it would be instructive to get an impression how the concentration fields of, say, NO₂, H₂O₂, PAN, HNO₃ in the online run relate to the offline runs, for instance by means of zonal average tropospheric concentration fields for the various runs.

pp 2218, l20: "methane lifetime is 9.3 year". Emmons et al. 2010 present a methane lifetime of 10.5 year. So the lifetime in CAM is significantly lower, resulting in, e.g., lower CO burdens. Again it would be good to see separate numbers of methane lifetime for the three runs. "... CAM-chem behaves very similarly...": I think this statement is too general.

pp 2219, l5: "tropospheric oxidative capacity": Please note that it is also a representation of the CO and its precursor emissions, which still varies substantially between the different estimations presented in recent literature.

pp 2219, l9: "MERRA": It would be interesting to see the CO fields from the GEOS simulation, or at least to provide an indication how CO from GEOS5 relates to the one from the MERRA run.

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pp 2219, l16: The comparison to MOPITT is very interesting to get an indication of the model performance of CO in the free troposphere. Unfortunately only a qualitative comparison is shown in Fig. S5. It would be good to see at least a color scale in this figure, as well as similar figures for the two offline runs. Additionally, a short description on model agreement and discrepancies with respect to the observations would be desirable.

pp 2219, l25: it is interesting to see that OH burdens in the online run are considerably lower in the tropics and over the SH. , and compareable to the offline runs over the NH. This would suggest lower CO burdens in this run, as result of higher CO loss. However, the opposite appears true from figure 7a. Please explain.

pp 2222, l23: “equally well”: please give some more details on the dependency of the model performance to different meteorology. Also the dependency to different wet scavenging parameterizations could be added.

pp 2223, l12: (Lin et al., 2008) : This paper describes results based on the MOZART scheme only, so it is unclear whether the statement can be made for other tropospheric CTM's.

Technical corrections

pp 2200, line 5: ... stratospheric chemistry, dry and wet removal, ...

pp 2205, l.15: Please check the definition of X_i and $X_{i\text{scav}}$

pp 2209, l.4: Sect. 3 should be Sect. 5 ?

pp 2210, l4 : ...each substep. We have ... pp 2213, l 4: the Neu **wet** removal scheme...

pp 2214, l22: This ensures...

pp 2214, l24: Note ...

pp 2218, l15: OH distribution -> OH concentration

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pp 2220, I9: remove “rather”

Fig 9a / Fig 9b: please check the captions

Supplemental material:

Fig. S4: Please adapt the scale on the x-axes to better assess the differences between the runs, e.g. on pp 15.

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