Response to reviewer 1

We would like to thank Reviewer #1 for his/her thorough and useful comments. We have included in this response the original text (in italics) and our answers.

General statement: To answer this and the other reviews, we have considerably changed our figures to be more summarizing. This is in turn enables an easier side-by-side comparison of the various simulations, including the comparison of the two wet removal schemes. Regional aerosol optical depth and surface ozone diagnostics and discussions are also added. A better description of how CAM-chem relates to CAM4 and CESM is also included. Finally, a comparison of some meteorological fields is now in the paper.

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

6 hours. Included.

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

Size-dependent gravitational settling for dust and sea-salt, nothing for the other ones. Included.

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

Only volcanoes are vertically-distributed.

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?

Yes, it is a typo. Fixed.

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.

The whole section on ozonesondes is now rewritten.

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. 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?*

We have modified Table 8 to include all that information.

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.

We have included additional figures to offer a more comprehensive study of the wet scavenging. See revised section 7.2.

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.

We have redone our figure of comparison with observations to more display the information and show differences. All listed species are included.

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.

The methane lifetime is included in the revised Table 8.

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.

True. Such a statement is included.

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.*

It is included in the MOPITT comparison (Figure 12).

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.

Sorry for the missing color scale. This is now added. As requested by another reviewer, this Figure has now moved to the main portion of the paper (Figure 12).

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.

The OH burdens are CH4-weighted and so are not directly applicable to CO.

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.

A summary discussion of these is now included in section 8.

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.

True. Qualifier added.

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

This was not changed as this sentence refers to the set of simulations being discussed. Only wet removal is tested here.

pp 2205, l.15: Please check the definition of Xi and Xiscav

There was a typo that is now fixed.

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

Indeed. Corrected.

pp 2210, l4 : ...each substep. We have ...

Unclear what needs to be changed. No correction was made.

pp 2213, l 4: the Neu wet removal scheme...

No such line on p 2213. No correction was made.

pp 2214, l22: This ensures...

Done.

pp 2214, l24: Note ...

Done.

pp 2218, l15: OH distribution -> OH concentration

Done.

pp 2220, l9: remove "rather"

Done.

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

Sorry. Fixed.

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.

We have included this feature in the summarizing Figure in the main text. We have left Figure S4 unchanged (note that their numbering has changed).