

Response to reviewer 2: W. Collins

We would like to thank W. Collins for his 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 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.

General Comments

This is an important paper documenting the widely used CAM-Chem model and will be a very valuable addition to the literature.

It would be useful if the authors could discuss some of the uses of the CAM-Chem model and how it fits into CESM: providing chemical feedbacks in climate projections, climate impacts of reactive pollutants, air quality impacts of reactive pollutants etc. This would allow the reader to judge the model suitability for the various tasks.

I assume that CAM-chem is the component that supplies aerosols to CAM4. This paper therefore needs to describe the aerosols in more detail. In particular the interaction between aerosols and cloud microphysics is not mentioned.

We realize that there was misunderstanding in how CAM-chem fits with CESM/CAM and that the text in the Introduction was not sufficient. We have therefore included that information in section 2 (retitled) and included a schematic as Figure 1.

There are far too many plots (194 in the main text, 188 in the supplement) to be able to distinguish the wood from the trees when assessing the model performance. The authors should reduce this drastically, particularly in the main text, by picking those that illustrate their main points. Even with 382 plots in total there was no assessment of surface ozone, aerosols outside the US, or aerosol optical depth (crucial if the aerosols are used in climate simulations).

We have fully considered this recommendation and have remade most figures in the main text to be summarizing figures; and moved many of the multi-panel figures to the supplement. We have also added figures for surface ozone (Europe and US), and AOD (4 regions).

Specific comments

Page 2201, A diagram might be useful to illustrate the different coupling process between CAM4, CLM3 and CAM-chem, and how they differ in the 3 separate frameworks.

I'm slightly confused as to what is done in CAM4 and what in CAM-chem.

This is now done in section 2 and Figure 1.

Page 2201, line 1. It is not clear here or elsewhere (section 6) whether the composition

(gas and aerosol) affects the physical evolution of the online model (radiation, cloud physics). This is particularly important for the modelling of stratospheric ozone and temperature.

It does affect (through radiation) the climate in the online version. This is also described in section 2.

Page 2201, line 17. When calculating RF, how is the stratospheric temperature adjustment handled in the specific dynamics configuration? This is usually only treated in offline codes, so the specific dynamics doesn't seem to offer an advantage over a CTM.

We have clarified this by mentioning that this is an instantaneous RF.

Page 2204, line 5. Should cite the CLM technical note here rather than web link.

We have changed the link to directly point to the CLM Technical Note; we have also included a full reference.

Page 2207, line 14. A sentence or two is needed on PSCs, not just the ref to Kinnison.

Done.

Page 2208, lines 16-21. This paragraph was not entirely clear. Is the combined scheme (lookup above 200nm, online below 200nm) used for all configurations (trop and strat)? The first line says only the lookup table is "available at this time". What combinations of lookup and online are used for the experiments trop and trop+strat in section 6?

All configurations use the same photolysis scheme, which is split between online and lookup table. Text was added.

Section 4. I found this a bit unclear. At what frequency are the met fields read in?

6 hours. Added.

Is the dynamics used to advance the model between the input fields the same as the CAM4 dynamics? If so, is it a copy of the CAM4 dynamics in CAM-Chem or does it actually run CAM4?

The CAM4 dynamics is run. This should now be clearer with the addition of the description in section 2.

Is there a jump in the meteorology when the next met field is read in, or is there some smoothing?

There is linear interpolation for timesteps between the reading times. Added.

I didn't quite understand the sub-cycling and the diagram in Lauritzen didn't help. I think the point is that the advection timestep for tracers can be longer than the advection timestep for the dynamical fields. Why is this only the case for the offline version, wouldn't this be true online as well?

It is true for all versions. Added.

In lines 6-7 I assume

"mass flux" refers to the atmospheric mass flux, not the tracer mass flux. I.e. that the atmospheric mass flux is advected in smaller sub-steps?

It is actually the tracer mass flux. Transport is performed for each species separately.

Page 2211. I'm not familiar with GEOS-5 and MERRA. Is GEOS-5 an operational assimilation, and MERRA a reanalysis product using the same model (like ECMWF analyses vs ERA40)? I'm surprised then that they give such different results in section 7.

We have included a short description of differences and a section describing differences in some climate variables (section 7).

Section 5. This section is very short considering it needs to describe both the gasphase and aerosol chemistry. I realise the schemes are described in detail elsewhere, but some description here would be useful.

It is unclear what aspects should be expanded. We have not changed this section.

Page 2212, lines 16-18, I initially misread this as saying that the updated glyoxal production was not needed for long-term trends in the stratospheric composition. Maybe it could be re-phrased. More substantially, it is not the long-term stratospheric trends that are missing in the trop-only chemistry - these are provided as forcing data. It is the short-term variability in stratospheric composition that is not included.

Text was updated accordingly.

Page 2212, lines 24-26, I assume "taken from the WACCM mechanism" means it is the WACCM mechanism (i.e. identical)?

Not quite. WACCM has ion chemistry.

Section 6. Is the ozone coupled to the radiation in the strat-trop model?

Yes. Again, see section 2.

Section 7. There are far too many plots here.

As mentioned earlier, we have considerably reduced the numbers of panels (even though we have increased the number of Figures).

Page 2215, lines 4-6. Some very brief summary of the meteorological performance would be useful here, even if it is just a remark on what process were considered in the references cited. Have the cross-tropopause mass fluxes been assessed. If the strattrop model is coupled to the radiation, the impact on the UT/LS temperatures should be discussed.

We have included in section 7 a description (including temperature) of basic climate variables.

Page 2215, lines 17-19. I don't quite understand how the tropopause location can be wrong with analysed meteorology.

We meant *chemical* tropopause. Qualifier added.

Page 2215, lines 20-21. Why does the reanalysed met perform so much worse than the analysed met?

Possibly because of the difference in assimilation method and datasets (Rienecker et al., 2011). The text was rewritten to include this information.

Page 2215, lines 20-23. I'm not convinced STE is responsible for the positive bias since the online configuration has the lowest STE.

This is true. The Taylor diagrams indicate that all versions are quite similar at 250 hPa but the online version stands out at 500 hPa, with an especially poor representation of the annual cycle correlation. The text was modified accordingly.

Page 2216, lines 10-11, can the mixing of stratospheric air be assessed with an O3S tracer?

Unfortunately, we didn't include O3S in this version.

Page 2216, lines 20-23. I couldn't tell which of the many lines in fig 5 indicated that the variability was better captured in the online version. This "positive role" for consistent transport and chemistry seems to contradict the earlier discussions of figs 3 and 4 where online was worst. I think blue and green have been mixed up in the caption.

This figure was removed and replaced by the Taylor diagrams (correlation with annual cycle is shown by the angle).

Page 2217, line 19. "realistic meteorology"->"meteorology for the observing period"

Changed.

Page 2218, lines 7-10. I think figs 6c and 6d and this discussion can be removed

We have removed those figures and instead are showing the summary plots with all campaigns.

Page 2218, line 20. Is the methane lifetime 9.3 years in all configurations?

This is updated in revised Table 8 (now 9).

Page 2218, lines 25-27. How much of the boundary layer mixing is done in CAM-chem, and how much in CAM4?

As explained in section 2, all physics of CAM-chem is done in CAM4.

Page 2219, line 16. I would suggest bringing the MOPPIT figure from the supplement into the main text.

Done.

Can you include variability (boxes and whiskers) in figure 7?

We didn't as this made the figure too busy.

Page 2219, lines 25-29. What is the reason for the difference in OH. Is it due to the water vapour distributions? This could easily be compared.

This is done in the beginning of section 7. There is indeed a difference in relative humidity between setups and of isoprene emissions (See new Table 9)

Page 2220, line 10. Figures 9a and 9b are swapped round. These would be more informative as pdfs or box and whisker plots rather than scatter plots.

We corrected the labels. We also included NIMBUS-7 data. However we kept it under the same format to easily show that the model has a reasonable representation of the variability.

Section 7.6. This section is missing comparison with non-US sites (such as EMEP) and comparison with AODs such as Aeronet or satellite.

Done.

Page 2223, line 10. There is not enough evidence presented that "stratospheric processes are well described". Replace with "stratospheric composition is acceptable".

Done.

Supplementary figures. The numbering disagrees with the references in the main text.

Done.

