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Geoscientific
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Discussions

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

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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.

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).

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.

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.

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.

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

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

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?

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Section 4. I found this a bit unclear. At what frequency are the met fields read in? 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? Is there a jump in the meteorology when the next met field is read in, or is there some smoothing? 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? 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?

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.

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

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.

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

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

Section 7. There are far too many plots here.

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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 strat-trop model is coupled to the radiation, the impact on the UT/LS temperatures should be discussed.

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

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

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

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

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.

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

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

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

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

Page 2219, line 16. I would suggest bringing the MOPPIT figure from the supplement into the main text. Can you include variability (boxes and whiskers) in figure 7?

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Interactive Comment

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.

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.

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

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

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

Interactive comment on Geosci. Model Dev. Discuss., 4, 2199, 2011.