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**Geoscientific
Model Development
Discussions**

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

Anonymous Referee #3

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General Comments

This paper documents largely the implementation of the Chemistry in the CAM model, which can be driven by on-line or off-line meteorology. The authors propose an evaluation of 3 simulations, one with on-line meteorology and strato-tropo chemistry, the 2 others with off-line meteorology and only tropospheric chemistry. Evaluating the performances of these different versions of the model and hence the respective differences between the ways of driving meteorology is of great interest for the community.

After a well-documented description of the characteristics of the models, many diagnostics are presented to evaluate the model, using observations at different time and space-scales, and for different species, giving a large panel of information and

figures, that can really be helpful for the future model-developers.

However, there are some points that would deserve explanations or precisions before paper can be published:

A great point of interest of this paper is the presentation of 3 different simulations, with 3 different configurations of the model. The difference between on-line and off-line meteorology is clear. But, more details on the differences between the 3 runs, and what we should expect from these differences, are lacking :

- Even if the horizontal resolution is the same, the vertical resolution is quite different between the on-line (26) levels and the 2 off-line simulations (56). It would be very interesting to have the vertical repartition of the levels (PBL, Tropo and Strato). In a more general point of view, the difference between the vertical resolution and its potential impact is never discussed in the paper but should be.
- The treatment of the stratosphere in the off-line simulations is not clear : no chemistry, but is there any transport ? And how is the tropopause defined (chemical or dynamic) ? A small paragraph to detail these points would be very helpful, in particular to better document the strato-tropo exchanges.
- The differences between the 2 off-line simulations seem to lie on 2 key points. The first is of course that the forcing fields are not the same. An explanation of the principal differences between GEOS-5 and MERRA would hence be very helpful. The 2nd point is the different treatment of wet scavenging. A discussion on the impact of this new scavenging scheme would be interesting, in parallel with a brief comment on the differences on diagnosed precipitation.

In conclusion, with the addition of these points, I would highly recommend this paper for publication.

Specific comments

Section 3.3 Wet Deposition :
The sentence with Xi and Xiscav (p2205, line 15-16) is not clear. The presentation of the Horowitz' scheme lets me understand that there is no below-cloud scavenging. Is it right?

Section 3.5 PSC :
The treatment of TCly and TBry would deserve more explanation. How does this conservation impact the other chlorined and bromined species?

Section 4. Offline meteorology :
The time-step of the forcing as well as the horizontal and vertical resolutions of the forcing models would be interesting.

Section 6.1 Emissions :
Is it possible to have an estimation of the impact of the "simple linear interpolation" on the total emission?

Section 7.
General : It is not always obvious to see in the figures presented which simulation better performs. Maybe a numeric summary with simple statistics would be helpful. For instance, the authors could give the mean bias profile for the whole stations in fig 3, or the mean correlation at a specific height for fig 4. This objectiveness would give more weight to the diagnostics.

Section7.1 Ozone sondes

- Profiles : The question of the position of the tropopause is indeed crucial. Once

again, it would be very useful to know how the tropopause (and stratosphere) is treated in the off-line simulations, to better analyse the STE.

- Seasonal cycle : The better performance of the GEOS5 is not obvious. If you compute the mean bias, then GEOS 5 gives always better results than MERRA, at every altitude. But MERRA gives a better average correlation. So what is the main feature to retain?
  For the low bias in the SH Polar region, are you sure that you are not already in the stratosphere at 250hPa, or at least very close to the strong-gradient zone? That would explain why the cycle is so difficult to reproduce, and be coherent with the fig3 for South Pole.
  Do the authors have an idea of what happens in the low layers in North America in fall with the MERRA simulation?

- Long-term change : Once again a numeric evaluation (of the correlation for instance) would help. The Edmonton case should be developed.

- Ozone budget : How do the authors explain this huge difference of ozone net chemistry between online and off-line? Can the difference come from the fact that the chemical limit defined here as under 100ppbv may refer to different levels depending on the model? In addition, the lifetime of ozone for the online simulation is lacking.

Section 7.2 Aircrafts :
The "realistic meteorology" is a too drastic expression. The on-line version does not gives so unrealistic fields. A highlight on HNO3 and the difference between the two scavenging schemes would be valuable here.

Section 7.3 Surface CO :
Can the author comment the exceptionally high values for mean surface CO at approx. 0° and 30° for the online simulation (related to the high value of the cycle at 0°)?

However it is not clear whether the corresponding value for the offline simulation is in the plot.

Section 7.4 Ozone Total column :
The sentence 'Because the online model... only the mean and standard deviation is relevant in that case' is fundamental and true for the whole paper. It deserves to be at the beginning of the analysis. The figure 9 could be replaced by a numerical value of means and standard deviations.

Section 7.6 Aerosols :
Could the authors emphasize why the MERRA simulation is not presented here? For figure 12, once again, numerical value of the correlation would help.

Typos

Pp 2215 line 13 "tropospheric ozone us**ing** ozone sonde measurements averaged o**n** representative regions"
Pp 2218 lines 7 and 10 the figures are no well referenced
Pp 2219 line 18 : expect → except
Pp 2219 line 23 : figure S5 → S3
Fig 5 : there seems to be a mistake in the caption of the corresponding figure, MERRA and GEOS-5 are switched
Fig 12 : the caption is wrong

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