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Geoscientific Model Development Discussions

Interactive comment on "Representation of the Community Earth System Model (CESM1) CAM4-chem within the Chemistry-ClimateModel Initiative (CCMI)" *by* S. Tilmes et al.

Anonymous Referee #3

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Review of: "Representation of the Community Earth System Model (CESM1) CAM4-chem within the Chemistry-Climate Model Initiative (CCMI)" by Tilmes et al.

This paper documents the configuration of CAM4-chem used in the CCMI simulations. It documents updates to CAM4-chem and compares CAM4-chem simulations to measurements in three simulation configurations. It is particularly nice that the paper documents some of the successes of CAM4-chem as well as aspects of the simulations that do not agree with measurements. In and of itself the paper offers model refinements, but does not seem to offer any particularly new model developments or new science not documented elsewhere. The interest of this paper is that it acts as a background for further analysis of the CCMI model runs and thus will be useful to the community at

large in subsequent analysis. It will be particularly useful if other modeling groups post similar papers (hopefully using similar diagnostics). I would recommend publication following minor revisions.

A few general aspects of this paper could be improved (see specific comments below). (i) Some more detail concerning differences in the model simulations should be included. (ii) In a few places the results would benefit from additional analysis. (iii) Some aspects of the paper organization detailing the simulations and model could be improved. (iv) A number of figures are put into the appendix. It is not really obvious why this is done. It just makes it harder for the reader to refer to these figures. The figures in the appendix seem as relevant as those in the main body of the paper. I would suggest including them in the main body of the text.

Comments:

1. It would be useful right in the first paragraph to specify the simulation periods for each of the CCMI simulations (REFC1, REFC1SD and REFC2).

2. P2,L12: "reference CCMI model experiments". It would be worthwhile to emphasize that this is using CAM4-chem in particular – the summarization is not for CCMI models in general.

3. The introduction does not explicitly mention model-measurement evaluation. It seems that it is important to explicitly mention this as a focus of the paper. It may be worthwhile to point out from the beginning that this paper forms the basis for a more in-depth analysis.

4. P2,L22, "The land model". It would be worthwhile stating that the version of the land model used here does not include interactive carbon and nitrogen cycling. 5. P2,L28 and P3,L25: "McFarlanle"

6. Section 2.1. Please state explicitly whether aerosols impact model photolysis.

7. Section 2.1.1. The vertical grid is in hybrid coordinates, transitioning from pure

sigma near the surface to pressure in the stratosphere.

8. P3, L13. Please state which fields are nudged and the time resolution of the input meteorological fields.

9. The QBO (2.1.2). It would be interesting to know if nudging to the QBO impacts the tropospheric chemistry simulation.

10. P5,L8. "simulated atmospheric value". It is unclear to me to what extent atmospheric CO2 is simulated: is it simulated or specified? Also, does ozone feedback onto the atmospheric radiation budget?

11. P5, L14. It doesn't make sense to me to list all these tracers in the text. Most readers will have no idea what they are. Listing in the table should suffice.

12. P6, L3. The description of lightning NOx does not really belong in the section characterizing the chemical mechanism.

13. Section 2.2 . There really is hardly any aerosol description in this section (aerosols are described in 2.1.4 and 2.1.5). Some reorganization here would make sense.

14. Section 2.3. Personally, I would put this section above to give the reader some idea of the simulations before going into details about the model. Much of the information on nudging here seems a repeat (but in more detail) of information above. Please include years of the simulations here (they are included under initial conditions and spinup below). It would be helpful if you could summarize the emission differences between the simulations and possibly put some of the 1995-2010 emission totals in Table 1. The emission differences between the simulations are important for interpreting the results. The emissions for C1SD and C1 are exactly the same, correct? This should be explicitly stated. Emissions in C1 and C1SD show much higher interannual variability than C2 (figure A1). Is this due to biomass burning emissions or something else? The reasons for this should be stated explicitly. Are there mean emission differences between C2 and C1: if so please state what these are. Are the differences between REFC1

and REFC2 over the historical period only due to differences in the emissions, or can differences be attributed to something else in addition? In summary some additional clarity in the differences between these simulations would be valuable.

15. P7, L24, Can you quote measurement estimates of SWCF? Is the REFC1SD outside the measured range?

16. P7, Figure 1. It would be helpful to know the extent that the emissions given in Figure 1 are internally calculated. Section 2.1.6 does not specify which biogenic emissions are calculated. To what extent are the emission differences in the VOCs due to those in the biogenic emissions? Do differences in biogenic emissions account for all the differences between the C1SD and C1 VOC emissions? To what extent do the biogenic emissions account for the differences between C1 and C2?

17. P8, L1, "performance". Please rephrase. I think you mean performance of the simulation, not the chemical variables.

18. P8, L13 "N", Do you mean reactive nitrogen (Nr) including NOx, PAN, N2O etc or . . .?

19. P8, L20-22, "Variations in emissions. . ..". This is a very general statement and could be elaborated. In addition to additional information on differences in emissions between the simulations to what extent can it be expected that the dynamics differ? I would assume dynamics between C1 and C2 would be similar over the historical period except for some differences in aerosol forcing. Is this correct? I am not sure what dynamical metric would be most appropriate to show? I would guess convective mass flux might be sensitive to model dynamics.

20. P8, The differences in ozone are dramatic. The high ozone values (and high OH) are notable in the SD simulation and evidently impact the methane lifetime. Instead of a general statement the authors could dig a bit deeper here – the stratospheric ozone column and lightning NOx do not seem to explain this difference in ozone. Is O3S the

same between the simulations? How about height of convection, or Hadley circulation? It would be helpful if the authors could explain the difference in these simulations more specifically.

21. Some more information on the number of ozonesondes that go into the comparision in Figure 3 would be helpful. What does the caption mean by: "12 observed profiles per year and season"? Is it 12 observed profiles per year or per season?

22. P9, L25, "Large part to differences. . ..". Really? From Table 1 it the STE of O3 is larger in the SD simulation than the online simulations despite the fact that tropospheric O3S is smaller. Thus, the explanation given here doesn't seem to be correct. Have the authors looked at differences in O3 loss or production between the simulations?

23. P10, It is curious that the SD simulation tends to overestimate 250 hPa ozone in the mid and high latitudes but to get about the same STE as the other simulations and to have less O3S in the troposphere. Any explanation?

24. Why is Figure A2 not used in the paper itself? It seems this figure could just as easily be included in the main paper.

25. P10, L23. What is the evidence of a transport problem (see comment 22)

26. P10, Figure 6. I find it noteworthy that the pole to mid-latitude ozone gradients are rather different in the two experiments, with the SD simulations showing a larger southward ozone gradient which seems to be more consistent with the measurements.

27. P10, L32-35. The simulated tropospheric and stratospheric total ozone using 150 hPa as a cut-off is compared to the ozone climatology based on OMI and MLS satellite observations. The authors should address to what extent we might expect an offset (possibly seasonally varying) due to an "apples to oranges" comparison. That is, what is the effect of using the 150 ppb ozone contour as a tropopause in the model versus assumptions made in the measurements? The tropospheric ozone column might be particularly sensitive to assumptions vis-à-vis the tropopause height.

28. P12, L21 "The model reproduces. . .."? Really? This is not all clear from inspecting the figure (which is in a log-scale).

29. P12, L23, "The South-to-North gradient is represented well"? Please be more specific. Do you mean the hemispheric gradient? The aerosol burden is not always larger in the N.H. than the S.H. at all heights and months (e.g., September). This section could in general use a more in-depth and precise analysis of the model-measurement comparison for aerosols.

30. A recent paper (On the capabilities and limitations of GCCM simulations of summertime regional air quality: A diagnostic analysis of ozone and temperature simulations in the US using CESM CAM-Chem, Brown-Steiner, B.; Hess, P.G.; Lin, M.Y. (2015) Atmospheric Environment vol. 101 p. 134-148) seems to find some of the same discrepancies between specified dynamics and free-running simulations as found here.

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