

Second Review of “Implementation and evaluation of the GEOS-Chem chemistry module version 13.1.2 within the Community Earth System Model v2.1” submitted to Geoscientific Model Development by Fritz et al.

The resubmitted draft describing the new implementation of GEOS-Chem within CESM is much improved over the initial submission. I recommend it for publication after my minor comments and technical edits listed below are considered. The line numbers refer to the *marked-up version* that followed after the authors’ comments to the reviewers and not line numbers in the submitted draft as I did not have the new draft when I started my review and was unfortunately without internet at that time.

Line 55: The authors need to check references throughout the manuscript are in alphabetical or chronological order. Often there is a mix. For example, “Community Earth System Model (CESM) (Hurrell et al., 2013; Tilmes et al., 2016; Lamarque et al., 2012; Emmons et al., 2020)”

Line 64 : extra space at the end of sentence before the period.

Line 82: GEOS FP does not have a hyphen (see https://gmao.gsfc.nasa.gov/GMAO_products/NRT_products.php) and this should be corrected throughout the manuscript.

Line 83-85: GEOS FP and MERRA-2 should have references to Lucchesi 2018 and Gelaro et al. 2017 (<https://doi.org/10.1175/JCLI-D-16-0758.1>), respectively. Also, is there an extra space after output and before provides? (“output provides data”)

Lucchesi, R., 2018: File Specification for GEOS FP. GMAO Office Note No. 4 (Version 1.2), 61 pp, available from http://gmao.gsfc.nasa.gov/pubs/office_notes.

Line 87: Should ESM be redefined, or is it assumed folks know it from the CESM definition?

Line 99: The Keller et al. 2017 reference is incomplete. My assumption is it is redundant to the Hu et al reference so the Keller et al. 2017 should be removed and only the Keller et al 2021 reference should be included.

Line 101: Goddard Earth Observing System (GEOS) (Bey et al., 2001; Eastham et al., 2018) is redundant (to text above as GEOS already defined) and confusing (as readers could mistake Bey and Eastham references for GEOS meteorology references). I suggest removing references and “Goddard Earth Observing System”.

Line 105: CESM already defined and references provided.

Line 107: “observed meteorology including from GEOS”, I suggest changing observed to analyzed, unless you can actually nudge towards observations.

Line 119: Earth system models is written out instead of using “ESM”

Line 141: “Sections 4 and 5 present a one-year simulation (not including spin up)”. I know I suggested to make it clear that there was a spin-up vs the year used for evaluation in my initial review but I think this is confusing as it is written, such that one might wonder if no spin-up was included. Instead, maybe something like “Results from the one-year simulation (following the appropriate spin-up) performed for each model configuration 1) CESM with GEOS-Chem, 2) CESM with CAM-chem, and 3) the stand-alone GEOS-Chem CTM are presented with includes model intercomparison in Section 4 and evaluation against surface and satellite measurements in Section 5. Final discussion and conclusions are in Section 6.”. Note, I restructured the sentence and included a final sentence for Section 6.

Line 182: Should HEMCO have a Keller et al. reference? Line 301 redefines HEMCO and provides the Keller et al. 2014 reference.

Line 202: “OH, ozone and NO₃”. OH and NO₃ should be defined. OH is defined later on line 486.

Line 206,229: The unit has a period after gram instead of a space.

Line 249: MAM4 already defined so not necessary to do here.

Line 315: NO_x is defined as NO+NO₂ but then NO is used a few lines later without being first defined as nitric oxide.

Line 322: Sulfur dioxide not defined first before SO₂.

Line 331: Should this say “since iodine chemistry is not explicitly modeled” or do the authors mean only “iodine”

Line 346: “(methane, N₂O, and chlorofluorocarbons)”. When are these used again and if so should they be defined? N₂O is not defined yet. CFCs defined at line 478.

Line 377: The previous paragraph also begins with “Although”. I suggest changing one of these to an alternative word, like “While” (or “Whilst” if you prefer the British English)

Line 387: “Then, we perform a comparison of its output to that generated by two other model configurations (Section 4)”. I suggest adding something to this sentence so it reads to the effect of “the two conventional configurations for CESM-chem and GEOS-chem”. Otherwise, the “other model configurations” is ambiguous as the next sentences states “by comparing these results to C-CC”, it is unclear if the “results” is just the C-GC or C-GC plus these two other model configurations.

Line 398: I suggest changing “data” to “output” as this is from model simulations, and not observations. Does “This section” in Line 399 refer to Section 3 or Section 5?

Line 402: I suggest changing “All simulations cover January 1st to December 31st 2016, with an additional 6 months (S-GC) or 1 year (C-GC/C-CC) of spin up” to “Following a model spin-up period (6 months for S-GC and 1 year for C-GC and C-CC), the 1-year period of January 1 to December 31, 2016 was suitable for multi-model evaluation.”

Line 415: could state this is the stratopause

Line 423: Are the truncated levels the first 56 layers of the GEOS grid and then are they somehow regridded to match the 56 hybrid pressure levels of CESM? Can the authors clarify this?

Line 438: CEDS should be defined and reference provided, especially as a new version of CEDS was recently released.

Line 453: Is there a reference for AeroCom?

Line 506: What is the pressure range in Figure 2? It is hard to know from the y-axis labels. Some of the other vertical plots have the top of the y-axis given in the Figure caption. Check this is given in all captions.

Line 515: Remove “hydroxyl radical” as OH already defined.

Line 531: Should “ozone” be clarified to be tropospheric ozone or UTLS or which levels did the Park et al study look at?

Line 546-557: I am struggling with the structure of these two paragraphs. The bigger differences are in panels b and i, yet this first paragraph focuses on (h) and then the second on (i). I suggest reconsidering the flow of the paragraphs to something like:

There is a clear link between the ozone distributions and water vapor. Outside of the tropics and below the tropopause (Figure 3i), water vapor concentrations are up to 30 % greater in C-GC than in S-GC...<continue as written>...(an indirect sink for ozone). While ozone concentrations are uniformly lower for C-GC than for C-CC (Figure 3c), water vapor concentrations are uniformly greater for C-GC than for C-CC (Figure 3d). This is not surprising since the representation of moist physics in the two models is identical. However...<continue as written>...HNO₃. Differences in ozone related to tropospheric NO_y and halogens will be explained in detail in Section 4.4.

Lines 565-569: Is it differences in moist processes or transport?

Line 576-577: I am struggling to see this link.

Line 590-595: What do the authors mean by “does not show the same hemispheric asymmetry in absolute terms”? The difference over the southern hemisphere is about the same, it is really the northern hemisphere that is different. Is the emission of bromine from sea salt limited to either hemisphere or could it be driving the differences seen in the arctic which looks bluer to me in panel c than panel b. In the northern hemisphere, the C-GC and S-GC are fairly close (2.2 ppbv difference). Are the anthropogenic emissions driving the differences in the northern hemisphere or the southern hemisphere?

Line 602-604: This hot spot is also seen in both C-CC v C-GC plots. So is it almost more remarkable that January S-GC v C-GC does not have it? I wanted to link it to the hot spot over the amazon, but I got my sign wrong. Is there a clear feature in the C-GC minus C-CC in Figure 4 that is consistent all year round that could be linked to this hot spot in the bottom panel of Figure 5 that could partially explain July top panel? (if not, then this falls under the need for future work).

Line 626: reference Figure 7 when claiming the differences are greatest over the oceans, as this cannot be determined in Figure 6.

Line 626-628: The link to OH differences, is that still for below 800 hPa or throughout the troposphere? I do not see a clear link between Figure 3f and Figure 6c regarding the higher sulfate aerosol mass in the southern latitudes, but I can see patterns that match the OH zonal plots elsewhere in the troposphere.

Line 629: Can you quantify the “more closely follows”? I think this is the differences of +/- 25% quoted at the end of the paragraph but I think it should be quoted earlier. Can you give a reason by the C-GC and S-GC differ so much? Is it the difference in the bulk representation vs summing across all aerosol size bins? Oh, based on my latter reading of line 654 saying it is the convective scavenging, I think you may need to check here in line 630 if this should be broken into two sentences or at least properly reference (b) and (c) as I missed here the authors were referring to the ‘differences in the representation of convective scavenging’ was now discussing panel (b) and not still panel (c).

Line 644: In the previous paragraph there was a link to DMS for these greater concentrations and OH. Does that not apply here?

Line 655-657: it is not clear to me why POM would be greater in C-CC if the emissions of POM in C-CC are 29% lower.

Line 674: except the two points just above 50% (as mentioned in line 677). May be best to change your upper bound to match or give the range (looks like about -25% to 55%).

Line 688-691: Is the difference at 10 hPa really significant? At 200 hPa, the 60 and 63% are also only 3% different and read like they are about the same, but the striking difference is the much

greater 78% in C-CC at 200 hPa. Is it fair to open the paragraph with “partitioning between NO_x and HNO₃ differs significantly between the three models”? Is there a point where the difference becomes significant?

Line 693: Can this statement on sulfate aerosol size distribution be linked to Figure 6 at all?

Line 704: At 500 hPa, I cannot see any NO_x in the figures. Given the previous sentences say that from 200-900 hPa HNO₃ and PAN are dominant and the following sentence says NO_x once again becomes significant contributor, is there much NO_x contributing at 500 hPa that it is worth listing it here in the 78%, 85% and 97%? Or is this to be consistent with what is in line 702?

Line 706-707: This sentence is confusing as it opens talking about lower altitudes but then ends referencing 200 to 300 hPa and then the new text again refers to lower altitudes and is possibly redundant. Can the authors revisit these last two sentences to make sure the message is clear.

Line 710: Lightning NO_x emissions are not identical in Table 2 for C-GC and C-CC, but they are calculated using the same online parameterization. This should be clearer in this sentence.

Line 715: Make sure there are not two Table 3 references. May be an error in the marked-up version only.

Line 717-718: Is the percentage of NIT about the same between C-GC and S-GC?

Line 723: Is there a reference you can put for the Neu scheme? It is a bit confusing given the Neu and Prather reference after MAM.

Line 736: What is the scheme used in S-GC? It is not referenced earlier in this paragraph. Can the authors remind the readers?

Line 747-749: This kind of introduction of halogens (referring to both bromine and chlorine) may be better suited for the start of Section 4.4 (Line 660) instead of in the start of the bromine section.

Line 759: It looks to me like at 1000 hPa, the difference is exactly if not slightly less than 100%. Is it correct to say “exceeding ... by 100%”? Add reference to Figure 12b in the sentence. If Figure 12 is not referenced again, this paragraph only focuses on the result of surface concentrations and this could likely be summed up in a table instead.

Line 762: If Figure 12 is kept, reference Fig 12c in this sentence.

Line 765: This sentence is misleading as we do not have information on CH₃Br and OH in Figure 13. If this is a statement of chemistry knowledge, about Br_y increasing with height due to this reaction, it should go with Figure 12. I encourage the authors to reconsider the reference to

Figure 13 for the description of Bry profiles (Line 765-771). Possibly this was a Figure reference that was not updated correctly (line 764) and was intended to go with Figure 12 all along.

Line 800: Could refer back to Figure 12b to quantify this large difference.

Line 804: In the marked-up copy there is a double “mid-tropospheric mid-tropospheric” and then at the end of the sentence another “mid troposphere”. Can the authors reduce it to just one?

Line 812: Could remove “vertical” before “profiles”. Cly has been defined a few times. I am not sure if you need to redefine it here. The authors did not redefine Bry in line 750.

Line 815: Add C-GC to this sentence “total Cly for C-GC follows...”. Is this true, given there are swings in the % difference with values over 100 % near the surface, then negative, then back to over 100%? The distribution is about the same from 100 to 2 hPa (Figure 14b). Also, I do not agree with the “above this pressure, the vertical distribution of C-GC is closer to C-CC”, and this statement does not agree with what is stated in Lines 818-819. The authors need to revisit the discussion of Figure 14. Would it flow better if the authors followed a similar description as for Figure 12 (i.e., first the extreme differences at the surface and then the rest of the profile)?

Line 831: Can the authors quote altitude range here for UTLS HCl (e.g., 400 to 100 hPa)? I am trying to estimate by eye comparing panel c to b and I do not know how far up to the range should be. Does it go over 50 hPa, given the sentence that follows picks this height out for further discussion?

Line 832: While this statement is true, my mind went to Figure 14 looking for the percent difference, not from Figure 15. It would likely be easier on the reader if the 15% came off Figure 14, even if you reference Figure 14a which clearly shows C-CC is greater than C-GC and S-GC. I wanted to get this number off panel b or c, and that is less straightforward. As for the difference in HCl, this is not shown in a figure like panels b and c of Figure 14.

Line 840: Add C-GC to this first sentence. Can the authors link these values to the percent differences shown in Figure 14b and c?

Line 849: MOPITT is on TERRA.

Line 902-903: Does this sentence refer to Figure 4 or another figure (possibly moved to the supplemental) that compares model to observations somehow spatially so the readers could infer the biases over the Mediterranean Sea and Northern Europe. And if there is a high bias, how does that line up with the sentence that follows on 907 that the models are biased low.

Line 909: These numbers could go nicely in Table 9 next to the correlation values. Might help the reader to follow that the greatest bias is found for the C-GC as quoted in line 916.

Line 930-931: Can the authors provide panel references in the text here. I expect this plot is produced by a CAM-Chem post-processing script so it is not something to change as folks familiar with these plots are familiar with this layout, but it is counter-intuitive to me to have the plots nearer the surface at the top and the stratospheric levels in the bottom rows. This figure is very busy; if the 1, 2, 3, 4 key in the bottom left of each plot could be removed except for say in the bottom panels (j, k, l) that would help where there is overlap with some of the plots.

Line 936: Can you refer back to Figure 2 where the authors present the zonal mean ozone. The values at that height all appear to be about the same across the models so it does not surprise me that all three models appear to perform similarly.

Line 947-948: Is this again a familiar comparison for CAM-Chem users to a climatology for 2004-2010? I suggest repeating this reason for using this other reference period if it is the case. Also, could merge these first two sentences together because Figure 20 shows more than just TOC from OMI/MLS. "Figure 20 shows ...<continue as written> ...(Ziemke et al., 2011) (panel a) compared to results from C-GC, C-CC, and S-GC (panels b, d, and f, respectively)".

Line 956: Out of curiosity, is the difference in S-GC driven also by the spring Antarctic? Or is there something fundamentally different about the seasonality of the S-GC vs C-GC runs that leads to the 4.5 DU mean bias difference (7.8 vs 3.3)?

Line 958, Figure 20: The difference color bar labels are a bit odd. Any way to force it so the labels for white are -20 to 20 DU? It does not look like you need all the colors in the color bar. Similarly, Figure 21 difference color bar labels could be improved.

Line 964: Why only April and not the whole year as in Figure 20? Is it possible to have the mean values in the top right title label like in Figure 20 instead of the units mol/cm²?

Line 1053: Is there a reference for MUSICA (publication or website) which could be included here?