Reply of RC1: Review of Lu et al., 2019 "Development of the global atmospheric general circulation-chemistry model BCC-GEOS-Chem v1.0: model description and evaluation"

Reviewer #1

Comment#1-1: This paper describes a framework and evaluation of a novel model framework which incorporates the GEOS-Chem 1-D atmospheric chemistry component into the Beijing Climate Center's Atmospheric GCM (BCC-AGCM). With this combination successfully established, the authors evaluate the model in comparison with a suite of observations, including for tropospheric ozone, OH concentrations, and methane chemical lifetimes. They also compare satellite observations of various important atmospheric measurements, including NO2, CO, SO2, CH2O, and AOD. The work is a significant step forward in the development of Earth System Models in China and is clearly relevant to readers of GMD, and should be accepted pending the authors addressing a few relatively minor points relating to the reproducibility and presentation quality of their manuscript.

Response#1-1: We thank the reviewer for the valuable comments. All of them have been implemented in the revised manuscript. Please see our itemized responses below.

Comment#1-2: Specific Comments: Table 3: it is not clear how oceanic trace gas emissions are parameterized in this work. The work does reference the use of CMIP5 DMS emissions from the ocean in line 204, but Table 3 shows that an oceanic emission of acetone was also included. While this inclusion makes sense given recent research on the subject, it is not clear how this source was decided upon or how it was represented. The representation of the oceanic source of NH3 listed in Table 3 is also unexplained. Additionally, the ocean is a known source of acetaldehyde (Millet et al., 2010; Wang et al., 2019), but this source is not accounted for in Table 3. It is not discussed in the paper why this is the case.

References:

Millet, D. B., Guenther, A., Siegel, D. A., Nelson, N. B., Singh, H. B., de Gouw, J. A., et al. (2010). Global atmospheric budget of acetaldehyde: 3-D model analysis and constraints from in-situ and satellite observations. Atmospheric Chemistry and Physics, 10(7), 3405–3425. https://doi.org/10.5194/acp-10-3405-2010

Wang, S., Hornbrook, R. S., Hills, A., Emmons, L. K., Tilmes, S., Lamarque, J. F., et al. (2019). Atmospheric Acetaldehyde: Importance of Air-Sea Exchange and a Missing Source in the Remote Troposphere. Geophysical Research Letters, 46(10), 5601–5613. https://doi.org/10.1029/2019GL082034

Response#1-2: Thanks for pointing it out. The oceanic emissions of acetone and ammonia (NH3) are obtained from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP) emission inventory (Lamarque et al., 2010). We indeed did not consider the oceanic acetaldehyde emissions, which

should be addressed in the next model version.

We now state in the Section 2.5.1 (Offline emissions): "<u>We also incorporate emissions from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP) inventory (http://accent.aero.jussieu.fr/ACCMIP.php, last access: 14 Jun 2020; Lamarque et al., 2010) and from Wu et al. (2020) for emissions not included in CEDS data set. These mainly apply to oceanic emissions, soil NO_x emissions, and volcanic SO_2 emissions. Several sources (e.g., oceanic acetaldehyde emissions (Millet et al., 2010; Wang et al., 2019)) have not yet been included in this model version.</u>"

Reference added:

Lamarque, J. F., Bond, T. C., Eyring, V., Granier, C., Heil, A., Klimont, Z., Lee, D., Liousse, C., Mieville, A., Owen, B., Schultz, M. G., Shindell, D., Smith, S. J., Stehfest, E., Van Aardenne, J., Cooper, O. R., Kainuma, M., Mahowald, N., McConnell, J. R., Naik, V., Riahi, K., and van Vuuren, D. P.: Historical (1850–2000) gridded anthropogenic and biomass burning emissions of reactive gases and aerosols: methodology and application, Atmos. Chem. Phys., 10, 7017-7039, http://doi.org/10.5194/acp-10-7017-2010, 2010.

Millet, D. B., Guenther, A., Siegel, D. A., Nelson, N. B., Singh, H. B., de Gouw, J. A., Warneke, C., Williams, J., Eerdekens, G., Sinha, V., Karl, T., Flocke, F., Apel, E., Riemer, D. D., Palmer, P. I., and Barkley, M.: Global atmospheric budget of acetaldehyde: 3-D model analysis and constraints from in-situ and satellite observations, Atmos. Chem. Phys., 10, 3405-3425, http://doi.org/10.5194/acp-10-3405-2010, 2010.

Wang, S., Hornbrook, R. S., Hills, A., Emmons, L. K., Tilmes, S., Lamarque, J. F., Jimenez, J. L., Campuzano-Jost, P., Nault, B. A., Crounse, J. D., Wennberg, P. O., Kim, M., Allen, H., Ryerson, T. B., Thompson, C. R., Peischl, J., Moore, F., Nance, D., Hall, B., Elkins, J., Tanner, D., Huey, L. G., Hall, S. R., Ullmann, K., Orlando, J. J., Tyndall, G. S., Flocke, F. M., Ray, E., Hanisco, T. F., Wolfe, G. M., St. Clair, J., Commane, R., Daube, B., Barletta, B., Blake, D. R., Weinzierl, B., Dollner, M., Conley, A., Vitt, F., Wofsy, S. C., Riemer, D. D., and Apel, E. C.: Atmospheric Acetaldehyde: Importance of Air-Sea Exchange and a Missing Source in the Remote Troposphere, Geophys. Res. Lett., 46, 5601-5613, http://doi.org/10.1029/2019gl082034, 2019.

Wu, T., Zhang, F., Zhang, J., Jie, W., Zhang, Y., Wu, F., Li, L., Yan, J., Liu, X., Lu, X., Tan, H., Zhang, L., Wang, J., and Hu, A.: Beijing Climate Center Earth System Model version 1 (BCC-ESM1): model description and evaluation of aerosol simulations, Geoscientific Model Development, 13, 977-1005, http://doi.org/10.5194/gmd-13-977-2020, 2020.

Comment#1-3: Figure 6: The y-Axis is unlabeled and not clearly explained in the caption. I assume this is ozone in ppb, but some label or caption edit is probably in order. More importantly, it is not clear from the discussion of Figure 6 and Figure 3 whether the simulated values are taken from GEOS-Chem over some defined region or simply from the grid boxes corresponding to the observations in Figure 3.

Response#1-3: We have added the label "Ozone mixing ratio [ppbv]" for the y-axis in Figure 6. We now also state in Section 3.1 (Observations used for model

evaluation): "<u>To derive the monthly mean ozone profiles, only sites and months with more than three observations per month are considered, and simulated monthly mean ozone profiles are sampled over the corresponding model grids (Lu et al., 2019b).</u>"

Reference added:

Lu, X., Zhang, L., Zhao, Y., Jacob, D. J., Hu, Y., Hu, L., Gao, M., Liu, X., Petropavlovskikh, I., McClure-Begley, A., and Querel, R.: Surface and tropospheric ozone trends in the Southern Hemisphere since 1990: possible linkages to poleward expansion of the Hadley circulation, Science Bulletin, 64, 400-409, http://doi.org/10.1016/j.scib.2018.12.021, 2019b.

Comment#1-4: I have questions about the choices of regions used in Figures 3 and 6, though I recognize the authors are referencing regions already in the literature. In particular, I do not agree with the classification of the "NH Low Latitude" grouping in the context of this paper: lumping together southern China, Hawaii, Panama, French Guiana, and Nigeria into one coherent region would to elide an enormous amount of difference in chemical regimes, biomass burning, and anthropogenic emissions between the various regions chosen. I believe it makes more sense to separate the points near China and Taiwan into a "Southeast Asia" region, separate from the remaining "NH Low Latitude" grouping. As the "SH midlatitude" region already has an N of 2, this should be similarly acceptable. This choice may have ramifications for the seasonal simulation performance as summarized in Figure 6.

Response#1-4: Thanks for pointing it out. We have followed the reviewer's suggestion and separated the original "NH Low Latitude" group into the "Southeast Asia" and "NH Low Latitude" groups. Figures 3, 5, and 6 are re-plotted and they do not affect our analysis.

Comment#1-5: Figure 7: The choice made to show observations as small circles on a global map is very confusing in this context. Given the density of spatial coverage over the Northeastern US and Europe, I suggest 3 possible fixes. 1) grid the observations to the same resolution as the model output and show them side-by-side with the simulated ozone; 2) show different plots for the US, for Europe, and for the rest of the globe if necessary; or 3) the points should be averaged for display purposes over a larger spatial extents and made larger themselves. The currently displayed global view could be placed in the SI. As of now, Figure 7 serves a mostly pro forma purpose – it is hard for me to glean any useful information from the plot given the display, and true patterns in the observation data (for example the increase in surface ozone in the observations over central Europe) are obscured by the chosen plotting scheme, rather than illustrated. If the authors do not believe that Figure 7 should be changed, perhaps it belongs in the SI in its entirety, as it is not clear to me what the figure adds to the paper in its current state.

Response#1-5: We agree. We have revised Figure 7 as attached below to show a side-by-side comparison of simulated and observed surface ozone at the same model grids. This change does not affect our analysis.

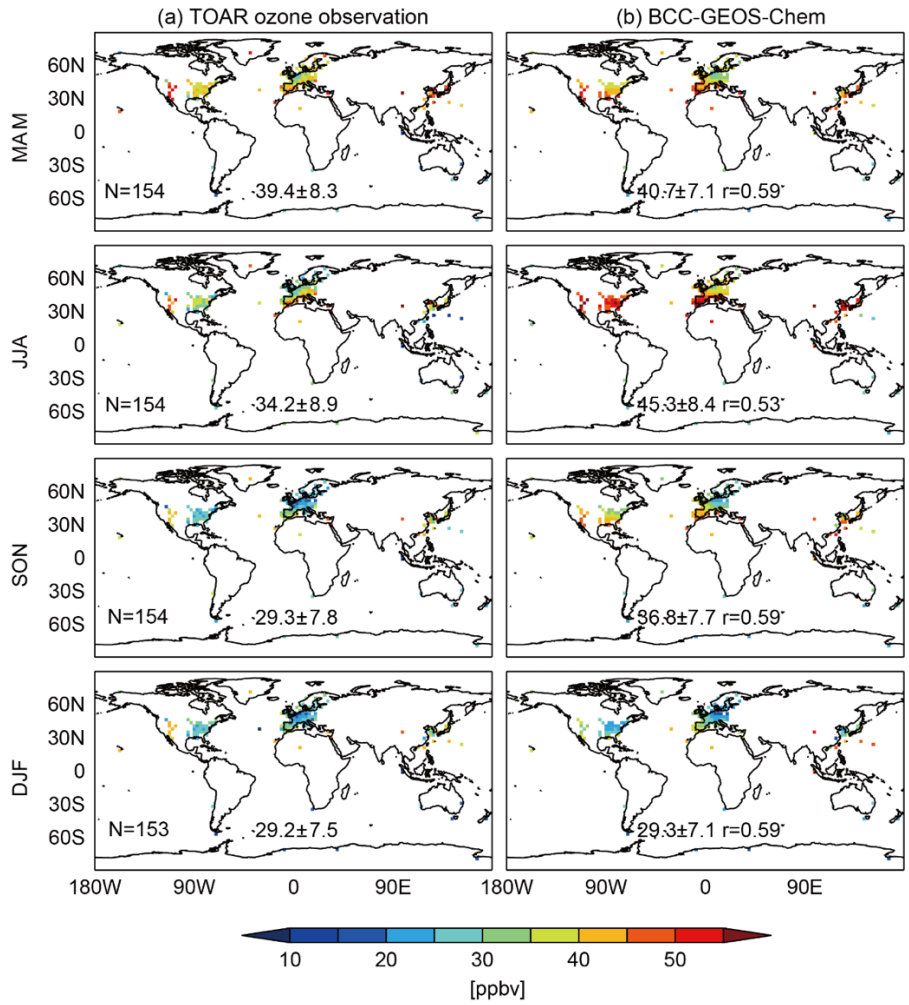

Figure 7. Spatial and seasonal distributions of observed and simulated surface ozone mixing ratios over 2012-2014. The model results (right panels) are compared to observations at rural/remote sites from the TOAR dataset (left panels). Observations are averaged to the same model grid. Seasonal mean values for observations and model results, their spatial correlation coefficients (*r*), and the number of co-sampled grids (N) are shown inset.

Comment#1-6: Figure 8: This is an excellent figure – very clear, with a high information-to-ink ratio. One comment: it seems unlikely that the geographic equator is a meaningful division in this kind of global average, given that the 'meteorological equator' deviates from 0 degrees depending upon season and region. If the comparison datasets support the option, the authors should consider using 5 latitude bins instead of 4 – perhaps 90-50S, 50-20 S, 20 S - 20 N, 20-50 N, and 50-90 N? This has the benefit of treating the tropics, midlatitudes, and polar-latitudes differently.

Response#1-6: Thank you for the nice words on the figure. We agree that using latitude bins of 90-50S, 50-20S, 20S-20N, 20-50N, and 50-90N can be better. However, the results of Young et al. (2013) and Emmons et al. (2010) are presented as the given latitude averages and we followed here to compare with their results.

Reference:

Emmons, L. K., Walters, S., Hess, P. G., Lamarque, J. F., Pfister, G. G., Fillmore, D., Granier, C.,

Guenther, A., Kinnison, D., Laepple, T., Orlando, J., Tie, X., Tyndall, G., Wiedinmyer, C., Baughcum, S. L., and Kloster, S.: Description and evaluation of the Model for Ozone and Related chemical Tracers, version 4 (MOZART-4), Geoscientific Model Development, 3, 43-67, http://doi.org/10.5194/gmd-3-43-2010, 2010.

Young, P. J., Archibald, A. T., Bowman, K. W., Lamarque, J. F., Naik, V., Stevenson, D. S., Tilmes, S., Voulgarakis, A., Wild, O., Bergmann, D., Cameron-Smith, P., Cionni, I., Collins, W. J., Dalsøren, S. B., Doherty, R. M., Eyring, V., Faluvegi, G., Horowitz, L. W., Josse, B., Lee, Y. H., MacKenzie, I. A., Nagashima, T., Plummer, D. A., Righi, M., Rumbold, S. T., Skeie, R. B., Shindell, D. T., Strode, S. A., Sudo, K., Szopa, S., and Zeng, G.: Pre-industrial to end 21st century projections of tropospheric ozone from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP), Atmos. Chem. Phys., 13, 2063-2090, http://doi.org/10.5194/acp-13-2063-2013, 2013.

Comment#1-7: Figure 10: why is the discrepancy between modeled and observed SO2 not discussed further? It is true that the model does broadly reproduce the spatial trend observed over China and India but elsewhere the correlation would appear to be quite poor.

Response#1-7: Thanks for pointing it out. We have partly reduced the discrepancies between the observed and modelled SO_2, by removing OMI measurements with slant columns greater than 5 Dobson Units (1.34×10^{17} molecules cm^{-2}) which are affected by strong eruptive volcanoes (Lee et al., 2009, 2011).

We further state in Section 3.4 (Evaluation of other atmospheric constituents) "We find low biases in the modelled PBL SO_2 especially over the volcanic eruption regions (e.g., Central Africa) but high biases in the industrialized regions such as East Asia, a pattern consistent with previous comparisons between the OMI and GEOS-Chem PBL SO_2 columns, which may reflect inappropriate ship and volcanic emissions in the model (Lee et al., 2009) and/or the model bias in the PBL height."

Reference added:

Lee, C., Martin, R. V., van Donkelaar, A., O'Byrne, G., Krotkov, N., Richter, A., Huey, L. G., and Holloway, J. S.: Retrieval of vertical columns of sulfur dioxide from SCIAMACHY and OMI: Air mass factor algorithm development, validation, and error analysis, J. Geophys. Res., 114, http://doi.org/10.1029/2009jd012123, 2009.

Lee, C., Martin, R. V., van Donkelaar, A., Lee, H., Dickerson, R. R., Hains, J. C., Krotkov, N., Richter, A., Vinnikov, K., and Schwab, J. J.: SO2emissions and lifetimes: Estimates from inverse modeling using in situ and global, space-based (SCIAMACHY and OMI) observations, J. Geophys. Res., 116, http://doi.org/10.1029/2010jd014758, 2011.

Comment#1-8: Technical Corrections: Line 448: Citation is misspelled – should be "Kodros and Pierce, 2017". The reference is correctly spelled in the bibliography.

Response#1-8: Corrected.