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Interactive comment on "Development of the global atmospheric general circulation-chemistry model BCC-GEOS-Chem v1.0: model description and evaluation" *by* Xiao Lu et al.

Anonymous Referee #1

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This paper describes a framework and evaluation of a novel model framework which incorporates the GEOS-Chem 1-D atmospheric chemistry component into a 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

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Discussion paper

manuscript.

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: 3âĂŘD model analysis and constraints from inâĂŘsitu 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.âĂŘF., et al. (2019). Atmospheric acetaldehyde: Importance of airâĂŘsea exchange and a missing source in the remote troposphere. Geophysical Research Letters, 46(10), 5601–5613. https://doi.org/10.1029/2019GL082034

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.

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.

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.

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 90-50 S, 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.

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.

Technical Corrections: Line 448: Citation is misspelled – should be “Kodros and Pierce, 2017”. The reference is correctly spelled in the bibliography.

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