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Geoscientific Model Development Discussions

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 #2

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The manuscript presents an overview and assessment of a newly constructed chemistry-climate model that has resulted from the linking of GEOS-Chem with the Beijing Climate Center AGCM. The general features of the model are presented and a fairly extensive comparison against observations for ozone are presented. In addition, other aspects of the model chemical climate are presented, including the global distribution of OH, ozone budget terms, and some limited comparisons for aerosol quantities against observations including AOD and speciated aerosol concentrations over the US.

The manuscript is very clearly written and presented, providing a fairly complete overview of the model components and an idea of how the chemical climate of BCC-

GEOS-Chem compares with GEOS-Chem itself and with other chemistry climate models. My only significant criticism is focused on Figure 10, comparing column amounts of NO2, CO, SO2 and CH2O. In the discussion of Figure 10, lines 394 – 404, the authors say that the averaging kernels for the satellite observations were not applied to the model concentrations when calculating the column amounts and that the comparisons 'mainly focus ... on spatial variations rather than absolute magnitudes.' In this case the comparison is nothing more than a test to make sure the specified emissions are being put into the model in the correct locations. Global models have a long-standing low bias for CO in the northern hemisphere that appears to be related to emissions and the hydroxy radical, but Figure 10 shows that BCC-GEOS-Chem has too high CO in the Northern Hemisphere. Due to the lack of a quantitative comparison with the satellite data by application of the averaging kernel it is impossible to judge whether the differences signify anything. While CH2O is not predominantly due to direct emission, the spatial distribution is tightly coupled to the emissions of biogenic hydrocarbons, so the comparison will also be largely driven by having regions of high biogenic emissions in the correct place. The differences in the magnitude of CH2O between the satellite and model is quite large and it would be interesting to have a more quantitative comparison with the satellite observations as the qualitative comparison focused on the spatial distribution is not informative at all. I would strongly urge the authors to revise the comparison of the column amounts to be more quantitative by application of the appropriate averaging kernel.

My other comments are all minor in nature and are given below.

Lines 80 - 83: Here the authors state 'Integration of GEOS-Chem chemical module into CSMs has been enabled by separating the module (which simulates all local processes including chemistry, deposition, and emission) from the simulation of transport, and making it operate on 1-D (vertical) columns in a grid-independent manner (Long et al., 2015; Eastham et al., 2018).' How is the 1-D column version of GEOS-Chem integrated with a 3-D CSM for processes that typically occur in the physics of the model such as

vertical turbulent diffusion and transport by deep convection? (I do find a description of deep convection and wet deposition around line 190, but no mention of how vertical diffusion is performed.)

Lines 181 – 183: The dry deposition uses the general characteristics of the land surface as given by the CSM land module BCC-AVIM. Are there also links to the land surface scheme for more short-term variables such as stomatal resistance, that would allow for effects such as drought on dry deposition?

Line 234: Minor typo in 'The model estimates t global annual ...'

Lines 311 – 314: Somewhere, either in the discussion of Figure 5 or the caption, there should be mention that the comparison is for annual average ozone.

Lines 314 – 321: I was a bit curious about why the vertical profile of ozone for the Japanese stations shows such a different vertical structure between the observations and model in Figure 5. Looking at Figure 6, the 300 hPa doesn't show that big of a difference. If 300 hPa is somewhere around 10 – 11 km, shouldn't the annual average in the observations be over 120 ppbv, though it is listed as 90 ppbv on Figure 6?

Line 364: Discussing the discrepancy in OH in the tropics between the Spivakovsky climatology the authors state 'This discrepancy appears to be mainly driven by the high bias in ozone levels in this region.' Attempts to understand the reasons for differences in OH between models has shown how many different factors play a role – see, for example, Nicely et al. Atmos. Chem. Phys. 20, doi:10.5194/acp-20-1341-2020, 2020. Do the authors have some reason to believe that the ozone and hydroxyl biases are related and, if not I would suggest removing this statement.

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