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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone and its precursors at Summit, Greenland: comparison between observations and model simulations" *by* Yaoxian Huang et al.

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This paper presents a GEOS-Chem model analysis of surface ozone and its precursors (NOx, PAN, C2H6, C3H8, CO) observed at Summit, Greenland during the period of July 2008 - June 2010, with a focus on their concentrations and seasonal variations. Modeling tropospheric ozone in the Arctic has been challenging, and it is very interesting to use a state-of-the art chemical transport model to test and improve our understanding of its sources and variability. The authors identified the discrepancies between the GEOS-Chem simulations and observations, which were then examined using various model perturbation experiments. The results are original, and the paper

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Discussion paper

is concise and very well written. I recommend its publication on ACP with some minor modifications, as itemized below.

1). Title - Using "tropospheric ozone" in the title is a bit misleading. Although this study also compared the model vertical profiles of ozone and specific humidity with ozonesonde observations, the main scope of this paper is "surface ozone".

2). Section 2: It is not clear which version of the GEOS-5 meteorological data archive was used. Is it GEOS-5.1.0 or GEOS-5.2.0? See e.g., "http://wiki.seas.harvard.edu/geos-chem/index.php/GEOS-5_met_field_reprocessing" and "http://wiki.seas.harvard.edu/geos-chem/index.php/List_of_GEOS-5_met_fields".

3). Section 2, 2nd paragraph: "Time series data were archived with 3-hr temporal resolution at the Summit grid box" — I think you meant "grid column". Moreover, it is not clear how the model output was sampled in the vertical. The elevation of Summit is 3212m asl. Did you sample the model bottom layer, or the model vertical layer that is about 3212m above the sea level? The latter may very well be different than the former because the topography is not well resolved at coarse resolution. Would the results about model overestimates or underestimates found in this paper be different if the alternative way of model sampling is used (e.g., lines 206-207)?

Minor editorial comments:

Line 43: change the "and" before "volatile organic compounds" to comma.

Line 56: the ARCTAS mission

Line 66: What do you mean "O3 mixing ratios below the boundary layer"? Within the boundary layer?

Line 77: "....used to be the global default anthropogenic C2H6 emission inventory" - Do you mean "default" in GEOS-Chem or any other models?

Line 228: add "over Greenland" after "PAN".

Line2 268-270: "relative to NEI11_MIX" – isn't this relative to NEI11? Remove it?

Line2 279-281: The annual mean agrees quite well with observations, but the simulation is worse in summer.

Lines 283-288: Good point, but this long sentence needs a break.

Line 325: "Unfortunately, ..." – "However, ..."

Lines 339-340: "..., which implies that GEOS-Chem possibly underestimates STE for O3 over Summit" – This is interesting and appears consistent with Choi et al., ACP 2017 (https://www.atmos-chem-phys.net/17/8429/2017/ , see their Fig. 6) , where the GMI CTM driven by MERRA (GEOS-5.2.0) underestimates ozonesonde-observed ozone in the Northern Hemisphere high-latitude upper troposphere.

Lines 358-359: Summit, Greenland; surface ozone

Figures 2, 3,5, S1: In the caption, state briefly what the perturbation simulations are and refer the reader to the text for details.

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