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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of ACCMIP ozone simulations using a multi-constituent chemical reanalysis" *by* Kazuyuki Miyazaki and Kevin Bowman

Anonymous Referee #2

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Review of Evaluation of ACCMIP ozone simulations using a mulch-constituent chemical analysis by Miyazaki and Bowman for ACP

In this work, the authors evaluate tropospheric ozone in the 2000 time-slice simulations performed by global chemistry-climate models in support of the Atmospheric Chemistry-Climate Model Intercomparison Project using a chemical reanalysis product that assimilates data from multiple satellites. The chemical reanalysis for 2005-2009 is first evaluated against global ozonesonde data and found to be in good agreement with measurements. In comparison to the reanalysis, the model ensemble mean is found to underestimate tropospheric ozone spatial and temporal variability. The paper is mostly well-written and is within the scope of the journal.

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Discussion paper

Evaluation against observation is a necessary step for building confidence in the global model simulation of tropospheric ozone. Limited observational constraints restrict our ability to thoroughly evaluate models. The use of chemical reanalysis discussed in this work is a promising method of model evaluation. However, a major aspect of the evaluation presented in this work need to be addressed before I can recommend publication. Reanalysis over four years (2005-2009) is used to evaluate time-slice simulations characteristic of the year 2000. The ACCMIP simulations were designed to eliminate interannual variability whereas the reanalysis product includes interannual variability. The use of 4-year product for evaluating climatological mean model simulations needs to be strongly justified.

Below are additional specific comments to help improve the manuscript.

Specific Comments:

Abstract: Please mention that a 4-year reanalysis data is used to compare ACCMIP time-slice simulations, and that the evaluation itself can be biased because of this inconsistency.

P1L17: Insert anthropogenic between "...important greenhouse.."

P2L1: There are several studies highlighting the use of CTMs/CCMs to assess the radiative impacts of tropospheric ozone prior to Bowman et al (2013). Please acknowledge those.

P2: 2nd and 3rd paragraphs discuss uncertainties in measurements for evaluating chemistry-climate models. I think they can be combined and modified for clarity.

P3L12: Please provide motivation for evaluating only tropospheric ozone and not its precursors (e.g., NO2, CO). Presumably biases in ozone are driven by biases in its precursors.

P4. How is photolysis calculated in the forecast model? Does the model represent methane - prescribed concentrations or emissions? Since the ACCMIP models used

different emissions inventory for ozone precursors compared to what is used in this reanalysis product, how would the comparison be affected by this difference?

P4L13: What is the convection scheme of MIROC-AGCM? Please describe it in a sentence.

P5L7: What is DOMINO data? What does the acronym stand for?

P5L21: According to Lamarque et al (2013), models were run for several years (up to 10 years) for each time slice. How are the model results for 2000 time-slice handled for comparison with the reanalysis?

P5L29: Young et al. (2013) note that although the models used the same anthropogenic and biomass burning emissions, model-to-model diversity in the implemented chemical scheme resulted in differences in precursor (especially VOCs) emissions across the models. So, the statement "same emissions were used in all the models" is not accurate. Please modify.

P6L7: Please provide details on how the ACCMIP model monthly ozone concentrations were interpolated to 2 hour temporal resolution? What diagnostics from the reanalysis were used to compare with observations - monthly or hourly ozone? Some clarification is needed here.

P6L29: Can you please elaborate on the model setting that caused this significant degradation of the representation of ozone in the UTLS?

P7L3: The terminology to refer to model output is somewhat confusing making it difficult to keep track of observations versus model output. Suggest referring to output from the forecast model and ACCMIP as "forecast model output" and "ACCMIP model output", respectively, and observations as "data".

P7L22-24: The simulation of the "wave-1" pattern by ACCMIP models has already been highlighted by Young et al. (2013). This reference needs to be cited.

P7L32: It is not clear what “common reported” means here. Please clarify.

P8L10: Which region is “In this region. . .” referring to?

P8L14-15: These results are consistent with those discussed in Young et al. (2013).

P8L24: The differences could also be associated with the way biomass burning emissions are handled across models - whether they are emitted at the surface layer or distributed vertically in the model, as a larger role for biomass burning in the tropical mid to upper troposphere has been suggested recently (Anderson et al., 2016).

P8L33-34: Why are results from a specific model highlighted? Is it because model 8 is driving the large model diversity at 200hPa? Please provide a figure to support this statement if this is indeed the case.

P9L3: It took me a while to understand the meaning of “reanalysis concentrations from the ozonesonde sampling”. Please rephrase this to indicate that reanalysis output is sampled at ozonesonde sites instead of averaging the reanalysis at all grid cells.

P929-31: Here again the results are consistent with Young et al. (2013) - see their Figure 4 and its discussion.

P10L23-25: Please clarify what is meant by “radiative heating distribution in chemistry-climate simulations are largely uncertain. . .” Are you referring to the radiative heating due to tropospheric ozone? Please elaborate on how the O3 NH/SH ratio provides information on the radiative heating distribution.

P11L17: replace “completion” with compilation.

P11L19: By coarse resolution, do you mean the coarse horizontal resolution?

P11L29: Did you mean - “...less variabilities in the SH than in the NH”?

P12L9-11: Please quantify large in “Large negative model biases. . .” and larger in “...errors are larger than those. . .”

Section 5.1 Please elaborate on how the comparisons discussed here may be influenced by inconsistency in the time period of the reanalysis and the ACCMIP simulations. The reanalysis output is for the 2005-2009 whereas the ACCMIP simulations are representative of 2000. Additionally, the precursor emissions used in ACCMIP simulations were decadal means and not specific to the year of simulation. For example, year 2000 biomass burning emissions were calculated as average over 1997–2006 (Lamarque et al., 2010), so they encompass the high emissions over Southeast Asia in 1998, an El Nino year. One would expect that biases due to sampling in time would occur similar to the biases due to sampling in space.How significant are the biases due to spatial sampling errors (highlighted here) compared to temporal sampling errors. The issue of temporal and spatial sampling was recently highlighted by Lin et al. (2016) in the context of tropospheric ozone trends.

P14L24: Replace biogenetic with biogenic.

P15L6: I am not sure if the 2005-2009 can be considered a "long-record" Are the authors referring to the possibility of a long-record reanalysis sometime in the future when observations have accumulated in time.

P15L7-9: From Lamarque et al. (2013): "This averaging was designed to reduce the effect of interannual variability and therefore provide optimal conditions from which average composition changes and associated forcings can be more readily computed." The ACCMIP simulations were designed to remove interannual variability, therefore, it is unjustified to state that "the influence of ENSO was not well-simulated in ACCMIP. . ."

P33 Figure 5. Please provide statistics such as mean bias and correlation for the comparisons here.

References:

Anderson et al., A pervasive role for biomass burning in tropical high ozone/low water structures, Nature Communications 7, Article number: 10267 (2016)

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Lin, M., L. W. Horowitz, O. R. Cooper, D. Tarasick, S. Conley, L. T. Iraci, B. Johnson, T. Leblanc, I. Petropavlovskikh, and E. L. Yates (2015), Revisiting the evidence of increasing springtime ozone mixing ratios in the free troposphere over western North America, Geophys. Res. Lett., 42, 8719–8728, doi:10.1002/2015GL065311.

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