**Atmospheric
Chemistry
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Discussions

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# *Interactive comment on* "Multi-model study of chemical and physical controls on transport of anthropogenic and biomass burning pollution to the Arctic" *by* S. A. Monks et al.

## Anonymous Referee #2

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This manuscript presents a comprehensive multi-model analysis of CO and O3 in the Northern Hemisphere with focus in the Arctic region, in the frame of the POLMIP project. Potential sources of model discrepancies and variability have been investigated in great details through analysis of simulated CO, O3 and OH, as well as CO-like tracers throughout the Arctic troposphere using numerous atmospheric models (global and regional) which have been first evaluated against Arctic surface, aircraft and satellite measurements.

This paper is an interesting and important work which covers many aspects (atmospheric transport and chemical processes) related to model deficiencies in tropo-

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spheric ozone and its precursors in the Arctic. The manuscript is well written and the topic is suitable for ACP.

I have one major comment related to the impact of using different model resolutions and meteorological data for evaluating model performance and inter-model differences, which should be addressed before final publication.

The authors argue to evaluate the chemical and physical controls on transport of pollution to the Arctic through a multi-model analysis. However from literature reviews it is known that model resolutions may be influencing ozone and CO concentrations. This could, amongst others, explain the inter-model differences in the underrepresentation of ozone transported from mid-latitudes. Similarly, underrepresentation of ozone transported from stratosphere may also be influenced by vertical resolution between models, especially at the tropopause level. The different vertical and horizontal resolutions in models as well as the different meteorological datasets are mentioned in Table 1 but their impact in both model performances and inter-model comparison results is not discussed later.

I understand that modelers are used to perform model runs with specific resolution and offline meteorology, and the complexity for running consistent models, but I do not think that only considering the same emission dataset is enough for arguing models as "directly comparable" and discussing performances and inter-comparison results without mentioning other probable causes of inter-model differences. I would suggest to better discuss your results in line with the differences in both resolutions and meteorological datasets. Even though the authors shortly mention the influence of the resolution by running WRF-Chem at two horizontal resolutions (100 km and 50 km), both of them are high-resolution simulations and they do not allow to evaluate the effect of coarse global model resolutions on multi-model vs. observations comparisons.

Specific comments: p. 25290, l.30: I do not understand what do you mean with this sentence. Please explain or reformulate. Do you mean that in terms of available days

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when comparing with observations?

p.25290, ll.8-11: It is not clear to me what do you mean by "These tracers therefore had the same transport ..." I guess you mean it by comparing, for each tracer, each TOMCAT run performed with monthly mean OH concentration fields taken from each POLMIP model? Could you please clarify? For this sensitivity study, why did you choose the TOMCAT model which uses lower spatial and vertical resolutions than most of the POLMIP models?

p. 25290, ll.16-18: Why using monthly means to evaluate your model simulations and not daily means since daily observations and hourly model output are available?

p. 25290, ll.18-20: Could you please indicate the accuracy associated with the Arctic surface CO and O3 measurements?

p. 25291, ll. 10-12: What are the main differences between older versions of MOPITT and version 6? Is it possible to use the bias previously calculated by Deeter et al. (2010) with in situ estimates to version 6 of MOPITT, which could be used later in the analysis?

p.25292, ll. 5-10: Could you please indicate the uncertainties for CO and O3 measured by the POLARCAT aircrafts?

p. 25292, ll.19-22: It is not correct here to indicate that NMGE "gives the mean model bias over the vertical column", in fact it corresponds to specific altitude ranges sounded by instruments or to two specific pressure levels (300 and 700hPa) when comparing models with MOPITT. Please reformulate.

p. 25293, ll.3-5, Figure 2: Since daily observations for surface measurements are available, you could add in the figure error bars corresponding to the observed variability ($3*\sigma$) associated with monthly means. Similarly, you could add on the $3*\sigma$ model variability associated with monthly means for each run using the hourly instantaneous output files for model simulations. This should help to better evaluate the seasonal

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variations and observations-models differences in your Section 4.1.1. Models can capture the monthly means but misrepresent the amplitude of the associated variability. It would be also worth to indicate and discuss here that observations – multi-model mean differences are larger than inter-model differences.

p. 25293, l.8, Figure 3: This figure is not fully described in the text. I do not understand what do the scatter points represent? I guess that they should correspond to each monthly means, but then, why only 4 scatter points by model run are represented for the CO Taylor diagrams against only 2 scatter points by model run for O3 and not 12 points for both CO and O3? Please explain the figure and describe better the results.

p. 25293, ll. 15-17: I do not understand this sentence. It would mean that even if large monthly deviations from the annual mean are real and that models capture them, you will get a high RMSD instead of a value close to 0. This would be nonsense. Do you rather mean that the statistic error is weighted by monthly deviations from the observed monthly means? Could you please clarify and reformulate?

p. 25293, ll. 17-18: I think that figure 4 is not really useful here since you do not discuss the values of the min., max., 25th and 75th percentile except the median biases. Similarly to figure 6 you could add on figure 2 the monthly mean percent bias. Figure 4 should appear later in Section 5.4.1. or reorganize these sections.

p. 25294, ll. 26-29: The better agreement of GEOS-Chem with observations at Barrow in March could be an artifact. In contrast with observations, no decrease in ozone in March is simulated by GEOS-Chem. The monthly mean values are underestimated over the year making the increasing simulated concentrations better matching with the observations in March. Such an underestimation is as well observed at Summit. I suggest to rewrite this sentence and to clarify the role of the halogen chemistry in the GEOS-Chem simulations.

p. 25295, ll. 16-19: Which model has been excluded from the MOPITT – multi-models comparison? Why?

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p. 25296, ll. 26-28: What does the shadow grey area on Figure 6 represent?

p. 25297, ll.23-26: If comparison between the Arctic and the source regions is difficult during winter, for the rest of the year, MOPITT data over the Arctic are characterized by around one full level of information allowing comparisons between total columns measured in the Arctic with source regions. Because of the lack of MOPITT vertical sensitivity in the Arctic, showing the monthly mean MOPITT CO at 700 and 300hPa in figure 6 does not make sense. You should better evaluate total column model performance over main source regions relative to the Arctic.

p. 25299, ll. 16-21: It would be worth here to comment and to point probable reasons explaining why all models with lower and higher OH underestimate OH in the lower and the middle troposphere.

p. 25300, ll. 22-25: It is not possible to distinguish the two WRF-Chem simulations, same color is used.

p. 25305, ll. 9-10, Figure 11: Why didn't you show the zonal mean OH from MATCH?

p. 25306, ll. 11-15, Figure 12: Why is LMDZ-INCA not shown in the correlation plots? For panels c) and d), I would suggest to plot annual mass-weighted tropospheric mean OH concentration in the northern hemisphere excluding the Arctic instead of global mean to investigate drivers in controlling inter-model OH differences over the Arctic and over the source regions.

p. 25307, ll. 20-25, Figure 13: Why didn't you plot seasonal mean differences for GEOS-Chem and TOMCAT?

p. 25308, ll. 10-13: Apart from vertical convection, what about the role of vertical resolution in the POLMIP models for explaining a part of the discrepancies in the magnitude of the tracer?

p. 25308, ll. 20-25, Figures 14 and 15: I do not see contributions from MATCH and GEOS-Chem. Which models do include the six regional tracers and which do not?

Figure 13 shows that MATCH includes them all and TOMCAT does not and Figure 14 and 15 shows the contrary, this is confusing. Please correct or clarify. In Figure 15, since all tracers are included within the TOMCAT model, even if GEOS-Chem does not include the regional tracers, you could use its OH field within TOMCAT. Did you exclude GEOS-Chem from the multi-model analysis because of different anthropogenic emission dataset? Please clarify?

Table 1: What is the number of vertical levels used in WRF-Chem? Does the model use 27 vertical levels from the surface to 50 hPa similarly to Thomas et al. (2013)? Please insert that in the Table?

Technical corrections: p. 25283, l.26: "at the Barrow." → "at Barrow."

P.25295, l.3: "with Shindell et al. (2008)" → "with Shindell et al. (2008)."

p. 25296, l.14: "Shindell et al. (2006)," → "Shindell et al. (2006)"

p. 25306, l. 11: "have the the highest" → "have the highest"

p. 25336, Figure 3: Specify that the diagrams represent Arctic surface comparisons "at Barrow and Zeppelin". It seems that the RMSD contours are plotted in blue dotted contours, not black.

p. 25339, Figure 6: The grey area is not mentioned in the caption/text.

p. 25340, Figures 7 and 8: Please, use different colors for WRF-Chem_100 and _50 km resolution simulations.

p. 25345, Figure 12: I do not see the OLS regression black lines in panels a), b) and d). Figure caption/text is not clear and too long: no differences between description of panel a) and panel c), and most of the explanation should be indicated in Section 5.1 instead of including it in the caption.

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