

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 #1

Received and published: 1 November 2014

The authors examine the performance of eleven state-of-art models participating in the POLARCAT Model Inter-comparison Project over Arctic and lower latitude emission regions by evaluating their simulated key atmospheric components (CO, O₃, OH, and H₂O), using observations from aircraft, surface stations, and satellite. They further investigate the contributions of dynamic transport and OH chemistry to the diversity of simulated Arctic pollution using two designed tracers. This is an interesting and valuable study. The paper is well written. I recommend publishing the paper after the authors address the following minor comments.

Specific Comments Page 25291 lines 14-20 and Page 25292 lines 1-13: It might be
C8733

better to give a table summarizing the observed atmospheric components, the used technologies, and the measured accuracies from the aircraft campaigns of MOZAIC, ARCTAS-A, ARCTAS-B, POLARCAT-GRACE, and POLARCAT-France.

Page 25293 – Page 25298: Reorganize sections 4.1, which addresses tracer seasonality, and 4.2, which addresses tracer vertical distribution. Aircraft measurements discussed in section 4.2 contain seasonal information, while satellite MOPITT data discussed in section 4.1.2 give CO vertical information.

Page 25294 lines 23-30: I am confused by the authors' explanation here for GEOS-Chem performance over Barrow. If including halogen chemistry is the reason for GEOS-Chem's better performance during March, why does this model still significantly underestimate O₃ year-around? Meanwhile, the authors indicate that GEOS-Chem model is also special in its accounting for transition metal catalyzed HO₂ uptake onto aerosol (page 25316 lines 1-5) and this new implementation might help GEOS-Chem performance during springtime. The paper would be more valuable if the authors could explore the contributions of these two chemical mechanisms more thoroughly.

Page 25297 lines 15-26: The argument of vertical transport resulting in the vertical response of NMGE over North America is not convincing. If this hypothesis is true, we would expect a similar pattern of NMGE over Asia since vertical transport is typically more vigorous there. Meanwhile, the NMGE vertical pattern over North America is not supported by CO vertical profiles shown in Fig. 9.

Page 25300 lines 23-26: Why is the simulated CO by WRF-Chem 100 km larger than that by WRF-Chem 50 km?

Page 25302 line 23: Please clarify what “low model bias” means. Does it imply the model's error is small or the model underestimates the observation?

Page 25303 lines 10-12: This is an interesting issue that merits further discussion. It might indicate that some fundamental model processes, e.g. vertical convection, need

to be improved.

Page 25308 line 16: Please add “and biomass burning” after “anthropogenic”.

Page 25317 line 1: Please add “Bian et al., 2013” before “Law et al., 2014”.

Page 25317 lines 4-7: If over diffusion during long-range transport is an issue, it should apply to both O₃ and CO coming from mid-latitude sources.

Figures 3: How is the value of RMSD from the figure supposed to be read?

Technical corrections 1. Page 25298 line 9: Change DC8 to ARCTAS. 2. Page 25305 lines 6-7: Delete the sentence “Firstly, it is ...”. 3. Page 25306 line 11: Delete one of “the” in “the the”. 4. Page 25314 line 15: Delete “in the”. 5. Page 25316 line 4: Change “OF” to “of”. 6. Figure 10: add “ATR = ...” after “FAL = Falcon”.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25281, 2014.