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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Pan-Arctic surface ozone: modelling vs measurements" *by* Xin Yang et al.

Anonymous Referee #2

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This study uses a suite of surface observations, ozone sonde measurements and satellite observations to evaluate pan-Arctic surface ozone in two global models (a chemical transport model (CTM) and a coupled chemistry-climate model) and the impacts of halogen chemistry on simulated seasonal cycles and modelled ozone during springtime ozone depletion events. The study serves as a useful benchmark for demonstrating the importance of inclusion of bromide chemistry in models for the simulation of Arctic surface ozone, and when and where this might lead to model improvement (as well as degradation). The assessment is novel, and it is a nice synthesis of surface data, satellite data and modelling. The paper is well written with clear figures. In general, the paper is worthy of publication in ACP, but I would recommend that the following points are addressed before acceptance.

A weak aspect is the exploitation of the two different models. It seems a lot of effort to run both models for the analysis, yet not very much is made of comparing their performance and discussing possible reasons for their different performance or what can be learned from this. In many sections and figures, only one of the two models is shown (different single models for different sections), which is not well justified.

Specific points:

Page 3, paragraph 2: There have in fact been multi-model assessments of Arctic surface ozone in global CTMs. See Monks et al., (2015), Emmons et al., (2015), and an older study by Shindell et al., (2008). Monks et al and Shindell et al both show overprediction of surface ozone at Barrow in spring, likely as a result of missing halogen chemistry. However, Emmons et al show a general model *underprediction* over the depth of the Arctic troposphere in April compared with ozone sondes, suggesting that the halogen-induced bias may not be pervasive in the Arctic troposphere. It would be helpful to see these previous studies highlighted in the text for context.

Page 5, line 3: "The retrievals were performed on a 0-4 km grid with 0.2 km resolution." Not clear what this means. What is a 0-4km grid?

Page 5, line7: Both models are driven by ERA-Interim data. For UKCA, please briefly explain what this means for the climate model. i.e. does this imply nudging with a certain degree of relaxation? Over what altitude range? It is important to recognise that this is different from a purely offline model (such as pTOMCAT). What else is prescribed / free-running between the models? Clouds? Surface fluxes?

Page 5, line 23: Is the Law et al., (2000) study the most up-to-date reference for the model chemistry scheme? How up-to-date is the kinetic data used? How do these data compare with that used in the UKCA model for the same tropospheric reactions? Does p-TOMCAT include non-halogen related heterogeneous chemistry (it seems that UKCA does)- e.g. N2O5 hydrolysis on aerosol, which is likely important for winter / early spring ozone and NOy in the Arctic. Given the focus on comparing ozone performance

between the models, it is important to acknowledge any important differences in the chemical schemes of the models.

Page 7, line 15: Care needs to be taken in over-interpreting the reason for differences between the models and assigning this to mainly physical parameters (and I agree that such differences between a climate model and CTM would be expected - although see also my comment on nudging), as it may be that there are important differences between the model chemical schemes (see point above). This is why it would be useful to point out more about these potential differences. I am not sure it is the case that only or a dominance of physical factors can be assumed.

Page 7, line 32: Effect of dry deposition on ozone abundances during long-range transport into the Arctic. A key references here showing suppressed high latitude ozone due to deposition loss to vegetation in Siberia is Stjernberg et al., (2012).

Section 4.1: Discussion of model and observed seasonal cycles. I think it is worth explicitly pointing out that inclusion of the halogen chemistry the control pTOMCAT model leads to severe under-estimation of spring ozone at Summit and Pallas.

It does not seem obvious to me why in presentation of the results in Section 4.3.1 model output switches from using p-TOMCAT to UKCA. Perhaps I have missed something in the applicability of the simulations to different periods. In general, only one model is shown for each part of the results comparing with observations. Would it be more informative to show both models where possible?

Throughout: I find the use of the labels "pTOMCAT_SI_OO_VSLS" and "UKCA_SI_OO_VSLS" to name the two control runs overly complicated and distracting when reading. It is better just to call these "pTOMCAT_control" and "UKCA_control" in the text. The names of the other experiments are then enough to highlight what is missing/included for the other runs.

I would recommend a more explicit short "Summary" or "Conclusions" section to defini-

tively set out the key findings of the study and their context in the wider picture. At the moment, the final paragraph is a bit brief and needs to be separated from the main discussion.

Figure 2 caption: "in various experiments" - please clarify in the caption from which model.

Figure 6 caption, please correct date "(May 201)"

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