

Interactive comment on “Denitrification, dehydration and ozone loss during the Arctic winter 2015/2016” by Farahnaz Khosrawi et al.

Anonymous Referee #2

Received and published: 31 July 2017

Khosrawi et al. present a detailed analysis of polar processes occurring at high northern latitudes during the Arctic winter 2015/16. In particular, they compare simulations carried out with a nudged version of the EMAC CCM with a range of satellite and aircraft observations. The analysis presented in the paper is of high standard and explores an important and relevant topic within the scope of ACP and as such merits publication following revision. I have several comments the authors should address before publication:

General Comments:

P5L25 The authors present their analysis averaged over a fixed latitude range (70–90N) rather than using a vortex following coordinate (e.g. by defining the edge of the vortex following Nash et al., 1996). Figure 12 in the manuscript shows the large zonal

variation in temperature and chemical fields, and highlights that the vortex is neither centred on the pole nor circular. I wonder what effect using a fairly large area average has on the results compared to averaging only within the vortex. While I do not feel it necessary to redo the analysis in any way, I would like to see a discussion on how using a fixed latitudinal average may affect the results of the paper compared to only considering airmasses within the vortex.

P3L22 While the authors have reference all the appropriate literature on the model configuration and description, and a detailed description of the EMAC model is not required, I would like to see further information on those parts of the model key to this paper. For example, section 2 should, in my mind, include a description of which PSC and aerosol types are included in the model, how sedimentation velocities are calculated, which heterogeneous reactions occur on aerosol surfaces, do uptake coefficients include temperature dependencies, etc. I feel this would significantly aid those not familiar with the EMAC CCM configuration.

Specific Comments:

P1L3 There is no need to capitalize polar stratospheric clouds here, and it should appear instead as it does in the Introduction (P2L10). However, in the Introduction it should read PCSs within the brackets.

P1L18 This is at odds with P7L32, where the authors state maximum ozone loss is 120 DU. While 2 ppmv is the maximum mixing ratio difference, 100 DU is more representative of the average loss over mid March, and does not represent the maximum column loss. This also applies to the conclusions (P11L19). I feel as well that it would be good to combine figures 7 and 8 so that total column differences appear below the deltaO3 plot in a single panel and the reader can compare the column loss with the altitudes at which this is occurring.

P3L4 I feel that having defined TNAT and PSC, these should be used consistently throughout the manuscript in place of NAT existence temperature and polar strato-

[Printer-friendly version](#)[Discussion paper](#)

spheric clouds.

P7L12I feel ΔH_2O should be defined in the text as ΔNO_y and ΔO_3 are. In fact, I feel each should be specifically defined in the text and figure captions (i.e. state $\Delta O_3 = O_3 - O_3^*$).

P9L3 Is the Khosrawi et al. (2017) paper in prep, which it is in the reference list, or now published? If so this should be stated in the text. Further, if the paper is not yet available I do not feel that the reference should be included in this manuscript and reference to it removed (i.e. removed the sentence on P9L2-4. This also applies to the papers referenced on P10L27-28. Certainly they should say they are in prep if they are not yet published, and further if the findings of those studies are not key to this paper I do not feel they should be included.

P9L14 The simulations presented in the study are described as nudged in section 2. Therefore, surely any difference in temperature between the model and observations is a result of the nudged dataset and not the model. I feel saying ‘temperatures as simulated with EMAC tend to be slightly warmer than measured outside the polar vortex’ is misleading, as the temperature field is not being simulated freely. Presumably, in a free-running model the temperature biases would be significantly different.

P9L28 Without providing further information this a difficult conclusion to follow. Can the authors be sure that chlorine activation is not just too weak? The assertion in the manuscript reads as though the chlorine activation is correct, but petitioning between other active chlorine species is the cause of the low ClO values, indicating too high Cl, Cl₂O₂ etc. Can this be demonstrated by showing that ClONO₂ and HCl are well simulated? Looking at these species should highlight the ability of the model to capture chlorine activation. Here also ClO_x should be defined.

P11L4 The model simulations are nudged, and so is it still true that the EMAC model has weak downwards transport in this configuration? I would have thought that nudging the model ruled out dynamical factors as likely causes of any biases in chemical fields

when compared with observations.

P11L9 A further complication here is surely that if the fine-scale features are not present in the ECMWF dataset used for nudging then the model could never accurately capture these features. Perhaps a discussion on this and to what extent will this limit the ability of your future T255 model to reproduce this structure is warranted.

P12L1-3 This is true only for nudged configurations where the dynamics is accurately captured, and would not be true of free-running models. I feel this is an important point which should be made to caveat the conclusion.

Technical Corrections:

P11L29 ClO_x should have a subscript x. Similarly subscripts should be used for NO_y in Figure 4

Figure 1 I feel contours should be used consistently alongside the shading in the figures to aid with clarity, as is done in the top panel in Figure 1. This could be applied to all the pressure vs time plots.

Figure 13 It looks like there are zeros used for multiple contours in the top panels (ClO) in Figure 13, indicating the contour label does not have enough decimal places. This should be corrected. In a number of locations the grammar and sentence structure could be improved – I would encourage the authors to undertake another proof-read of the manuscript. The sentence on P9L30-32 should certainly be edited for clarity.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-503>, 2017.

Printer-friendly version

Discussion paper

