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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of ECHAM5/MESSy Atmospheric Chemistry (EMAC) Simulations of the Arctic winter 2009/2010 and 2010/2011 with Envisat/MIPAS and Aura/MLS Observations" *by* Farahnaz Khosrawi et al.

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We thank reviewer 2 for the constructive, helpful criticism and the suggestions for revision. We revised the manuscript according to the suggestions made. Additional to this reply, we provide a supplement with two figures, one showing the daily maps from Aura/MLS and the other one showing time series from Aura/MLS compared to Envisat/MIPAS for the Arctic winter 2010/2011.

The authors present a detailed comparison of the Arctic atmosphere using the EMAC

model system and the satellite instruments MIPAS on Envisat and MLS on EOS-Aura. The authors chose for the comparison two winters 2009/10 and 2010/11 on the grounds that both were extreme but showing different dynamic features. The publication is a description of the performance of the EMAC model in these two winters in the Arctic. The authors discuss differences in HNO_3 in the comparison, but do only speculate about the causes, or cite other work which explains the deficiencies.

The intention of this study is to evaluate the model performance concerning PSC formation and redistribution of HNO_3. Thus, this study is a quality assessment of the model performance and deficiencies in the model performance have been detected and discussed. Of course we try to explain these, but to solve the problems and improve the model performance within the frame of this study is beyond the scope and should thus be done in the frame of future studies.

Therefore I wonder, if this publication is in the focus of ACP. From the 'Aims and Scope' section, it seems to fit more a journal like 'Geoscientific Model Development'.

Before the submission of our manuscript we have checked the "Aims and Scope" of both journals and came to the conclusion that our study definitely fits better into the "Aims and Scope" of ACP since our study does not focus solely on the model comparison (evaluation), but also on the characteristics of the two Arctic winters considered.

The manuscript is generally well written. However, sometimes the wording is a bit sloppy. An example is the sentence: 'The Arctic winter 2010/2011 was one of the most persistently cold winters on record.' I guess, the authors want to emphasize that the stratosphere has been exceptionally cold, not the whole atmosphere or the troposphere alone. The manuscript should be published, after the following minor issues have been addressed:

It is correct that we wanted to emphasize that the stratosphere was exceptionally cold. We agree that the wording in this specific example is not precise on which part of the atmosphere is meant. We changed the sentence as follows:

The Arctic winter 2010/2011 was one of the most persistently cold stratospheric winters on record, leading to the strongest depletion of ozone measured in the Arctic (Manney et al., 2011).

Please use a more stringent language. One example has been given above, but there are more throughout the manuscript.
We have thoroughly checked the manuscript and made improvements in the text accordingly.

Page 17 line 18-20
I cannot follow the argumentation why for the maps the 34 hPa level have been used and for the time series comparison the 50 hPa level. Also, for the two levels, different satellite instruments have been used. Given this information I cannot help wondering, why no MLS maps and no MIPAS time series have been shown. The authors mentioned, that maps from MLS compare similar to EMAC, but I would suggest to put those figures in the supplement.
We agree that the sentence is not precisely formulated. Gas phase depletion is pronounced over a certain pressure range and we just meant that in the chosen presentation forms (daily polar stratospheric maps and time series based on a 70-90°N latitude average) the gas phase depletion was most pronounced visible in the respective levels. Therefore, we changed the sentence as follows: *Comparisons of temperature, HNO_3, and PSC volume densities are shown over the full vertical range (200-6 hPa) in time-pressure cross sections. In addition, for HNO_3 we have chosen to highlight the 34 hPa level, where daily maps show the most pronounced gas-phase depletion features, and 50 hPa, where such signatures are largest in the time series of polar-cap averaged HNO_3 mixing ratios.*
In an earlier version of the paper we had the time series of both satellite instruments included in the figure, but decided in the end against it since it made the discussion of the results more complicated. Although the time series from Aura/MLS and Envisat/MIPAS are quite similar they are not the same. Using both satellites in the time series plots puts us in the need to discuss also the differences between the satellite

instruments which is, however, not the focus of this paper and distracts from what we actually want to discuss. The same holds for the daily maps. Showing both makes the discussion more complicated. By using MIPAS for the daily maps, both satellites for the contour plots and MLS in the time series resulted in a fair employment of the respective satellite data sets used in this study. We added the daily maps for Aura/MLS and a comparison of MLS and MIPAS time series for the Arctic winter 2010/2011 as a supplement to this reply (Figures 1 and 2), but think that for the final version of the paper such a supplement is not necessary.

Figure 3 and 9 From figures 3 and 9, MLS and MIPAS show differences in the HNO_3 time series, but I would like to see, if it is mainly scaling or if there are different features. Although the authors put forward the argument that MIPAS is not sensitive to gasphase HNO_3 (page 8 line 15) in the presence of PSC's, the MIPAS measurements seem always higher than the MLS measurements.
It is correct that the MIPAS HNO_3 mixing ratios are generally somewhat higher than the ones from Aura/MLS. We see that e.g. in the time series when we plot both satellite data sets (see Figure 2 in the supplement). Further, the difference in the HNO_3 mixing ratios between MLS and MIPAS is not solely related to the sampling bias of MIPAS in the presence of PSCs. Differences between these instruments are found during the entire course of the winter, but the differences increase when PSCs are present. The differences in HNO_3 between MIPAS and MLS have been documented in the literature. The paper by Sheese et al. (2017) discusses comparisons of ACE-FTS, MLS, and MIPAS profiles of several species. In all their figures that showed relative differences of HNO_3 profiles they found that MIPAS had a more positive bias than MLS in the altitude range 18–27 km, indicating that MIPAS provides higher HNO_3 values in general (their Figs. 7, second row, third panel from the right; 9, bottom right panel; 11, bottom right panel). We changed the the sentence at P8, L14 as follows: *The patterns of the temporal evolution of HNO_3 derived from Envisat/MIPAS and Aura/MLS are generally similar, however, Envisat/MIPAS provides somewhat higher HNO_3 abundances*

than Aura/MLS (see e.g. Sheese et al. 2017 for a throrough comparison and discussion of the differences between Aura/MLS and Envisat/MIPAS).

The authors did not mention if the models have been convoluted with the AVK of the measurements. From the AURA/MLS quality document, the maximum AVK peak for HNO_3 is 0.8. Does this make a difference to the comparison in figures 3 and 9?
We have not convolved the model data with the AVKs of the measurements. Although there are differences in the vertical resolution (EMAC: 1 km, MLS: 3-4 km, MIPAS: 3 km) we would nevertheless expect that differences between the model data and the model data convolved with the averaging kernels are small since we use averaged data (daily zonal means) which results already in some smoothing of the data. Further, we would rather expect that the usage of the averaging kernels from e.g. MLS would result in even lower HNO_3 mixing ratios and thus would increase the differences.

Figure 6 and 12.
The time series of HNO_3 is discussed with frequent reference to the PSC occurrence in figures 4,5 and 10,11. However, given the different scaling and labeling of the x-axis of those figures, it is difficult to judge the authors conclusion about the differences in the model versus measurement comparison. I would suggest to align the scaling of the figures 4,5,6 and figures 10,11,12 and to put grid lines on the figures. This would make the reading much easier.
On Figures 6 and 12 we have already grid lines in the upper panels and included now also grid lines to the lower panels. Putting grid lines on Figure 4, 5 and 10, 11, however, will not work. If we put grid lines on these plots these will not be clearly visible if these are in black. If we use white grid lines instead these will be too strong and make seeing the volume density distribution difficult. Adjusting the x-scaling for figures 6 and 12, however, is no problem. We have done this.

Page 8 line 18 In page 8, line 18 the authors write, that the concentration of HNO_3 seems generally low by 2 ppbv throughout the winter, as can be seen in Figure 6 and 12. I find

this difficult to see. In figure 6, EMAC and MLS do not differ anymore from mid-January till end of the time series. Later page 10, line 9 to 11, they restrict the difference to the PSC season. Please align the statements.
Thanks a lot for pointing this inconsistency out. We changed the sentence at page 8, line 18 as follows: *However, this comparison shows that EMAC HNO_3 seems generally 1-2 ppbv too low in early winter and throughout the PSC season.*

References:
Sheese, P. E., Walker, K. A., Boone, C. D., Bernath, P. F., Froidevaux, L., Funke, B., Raspollini, P., and von Clarmann, T., ACE-FTS ozone, water vapour, nitrous oxide, nitric acid, and carbon monoxide profile comparisons with MIPAS and MLS, J. Quant. Spectrosc. Radiat. Transfer, 186, 63-80, 2017.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-1190/acp-2017-1190-AC2-supplement.pdf

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