

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.

Anonymous Referee #2

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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 HNO₃ in the comparison, but do only speculate about the causes, or cite other work which explains the deficiencies.

Therefore I wonder, if this publication is in the focus of ACP. From the 'Aims and Scope'

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section, it seems to fit more a journal like 'Geoscientific Model Development'.

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:

Please use a more stringent language. One example has been given above, but there are more throughout the manuscript.

Page 7 line 18-20

I cannot follow the argumentation why for the maps the 34hPa 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.

Figure 3 and 9 From figures 3 and 9, MLS and MIPAS show differences in the HNO₃ 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 HNO₃ (page 8 line 15) in the presence of PSC's, the MIPAS measurements seem always higher than the MLS measurements.

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 HNO₃ is 0.8. Does this make a difference to the comparison in figures 3 and 9?

Figure 6 and 12.

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The time series of HNO₃ 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.

Page 8 line 18 In page 8, line 18 the authors write, that the concentration of HNO₃ 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.

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