

Interactive comment on “On the discrepancy of HCl processing in the dark polar vortices” by Jens-Uwe Grooß et al.

Jens-Uwe Grooß et al.

j.-u.grooss@fz-juelich.de

Received and published: 22 May 2018

We thank Reviewer #2 for the constructive review. The review comments are repeated below indented and *in italic letters* followed by our answers.

Specific comments

- 1.** *Introduction has to be elaborated with previous modeling studies in the polar stratosphere and HCl comparisons (e.g. Feng et al., Wohltmann et al., and Kuttipurath et al. articles on polar processing and ozone loss studies)*

In the revised version, we mention and discuss also the publications of the other models

that show HCl. For further details, see also our answer to comment 1 of reviewer #1 (Ingo Wohltmann).

2. *As stated in the introduction, the main idea was to check the impact of HCl discrepancy on ozone loss or polar ozone loss chemistry. However, that section is too short and limited to the description of the impact of change in ozone with respect to different model experiments. I would suggest you to calculate the ozone loss (profiles too) and compare with the published results (even for similar winters in the past). This would also give an idea about the model performance in comparison to other models.*

This is a good point. We enhance the discussion of ozone loss by adding the new Figure 14 with a vortex average ozone profile on 1 October when the ozone hole is fully developed. This is done for all three models in comparison with MLS observations. This figure shows that all models are capable of reproducing the ozone hole. It also shows the small effect of the hypothetical simulation “NAT decomp”. We also realised that the column ozone depletion for the Arctic winter shown in the old Fig. 14 were based on an earlier model run and were not consistent with the simulations shown in Fig. 4. This was now corrected. However, it does not seem necessary to us to add simulations for other years with the same model setup, as this would be excessive work and more or less a new study.

3. *I think that you missed ClO comparisons in this study, although you have a comparison with ClONO₂. You have described a lot about the chlorine partitioning and chemical polar processing (e.g. page 12, line 28–29). Therefore, I think it is important to compare the simulated ClO (from different experiments) with measurements (e.g. from MLS).*

This is correct. The comparison is however somewhat more complicated, as one needs to simulate the diurnal cycle of ClO. For that, we calculated the chemical composition

Printer-friendly version

Discussion paper



for the time and location of the MLS data using the chemistry-box-model mode initialised from the CLaMS 3-D simulation on the previous day. Figures 1 and 2 show the comparison on the 500K level for 20 June and 20 July, respectively. From this comparison, it is evident that in the considered vortex core region ($\Phi_e > 75^\circ\text{S}$) the ClO mixing ratios are near zero within the measurement uncertainty. Therefore we cannot gain much knowledge regarding the potentially missing process. Further it is evident, that the hypothetical process included in the simulation "NAT decomp" induces almost no change to the ClO mixing ratios. Nevertheless, we now also mention this aspect regarding the ClO comparison in the revised version of the manuscript.

4. *Page 8, Para 2: You stated that the numerical diffusion masks the HCl differences in Eulerian models. However, still the HCl discrepancy is very much apparent in those models/simulations, as demonstrated in this manuscript? So how much is the contribution from numerical diffusion?*

Besides the HCl issue, we think the effect of numerical diffusion itself is an interesting aspect of this study. An exact estimation of the impact of numerical diffusion is however difficult. We show the sensitivity run, in which every 24 h an Eulerian averaging event was triggered in CLaMS. The HCl change in this sensitivity run is on the order of about half of the HCl discrepancy. As the model is set up, it would be technically challenging to do such an Eulerian averaging event at every timestep. A comparison like this would have to be done in a model with otherwise identical setup to quantify the numerical diffusion effect. For the current study, we think that it is enough to point out numerical diffusion as one reason for the inter-model differences without being able to quantify the effect exactly.

5. *You have used three different models for this study, which is also the strength of this study. However, a discussion on the ability of synergetic use of the models to be*

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

applied for such studies is missing here. Only different test simulations are given. Please include a brief discussion in Section 6, and add few lines in conclusions too.

Thank you. We followed this suggestion in the revised version.

Technical

The typographical and grammatical issues will be updated as suggested.

Page 5, Line 17: You did not use CIO data?

We did not use the CIO data for initialisation. The initialisation time is before the period of chlorine activation. Further, it is difficult to derive the partitioning of chlorine from CIO only. However, in the revised version we mention the comparison with MLS CIO as indicated above.

Page 8, Line 32: Is there any reasons for taking 500 K altitude for this comparison?

Yes. From Figs. 2 and 3 it is evident that the largest difference between model and data is present in the “tongue” at this level. As here the HCl depletion is still ongoing in the observations, we speculate that this is the location where the possible missing process has the strongest impact. This point will be clarified in the revised version.

Page 13, Line 9: Numerical diffusion! Then how can we use these models even for this study (e.g. HCl differences)?

Yes, of course there is a conceptual difference between Lagrangian and Eulerian models. But we think, we can and even should use these different models in this study. While using the Eulerian models for this comparison, we must be aware that the numerical diffusion may cause problems. This is our interpretation for the inter-model

differences. Note that both WACCM and TOMCAT-SLIMCAT are well established used for studying a variety of questions regarding stratospheric ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-202>, 2018.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper



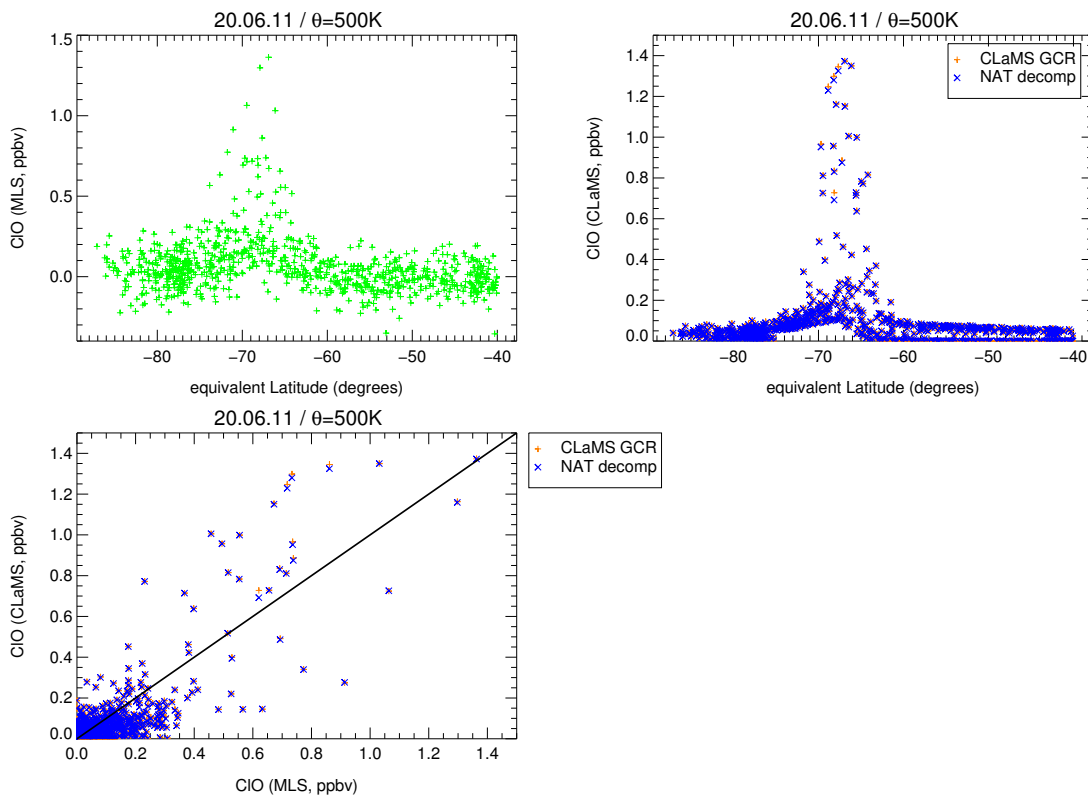


Fig. 1. Comparison of MLS CIO observations with CLaMS for 20.06.2011 at 500K potential temperature pole-ward of 40S: (a) MLS data vs equivalent latitude, (b) corresponding CLaMS results, (c) CLaMS vs MLS.

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

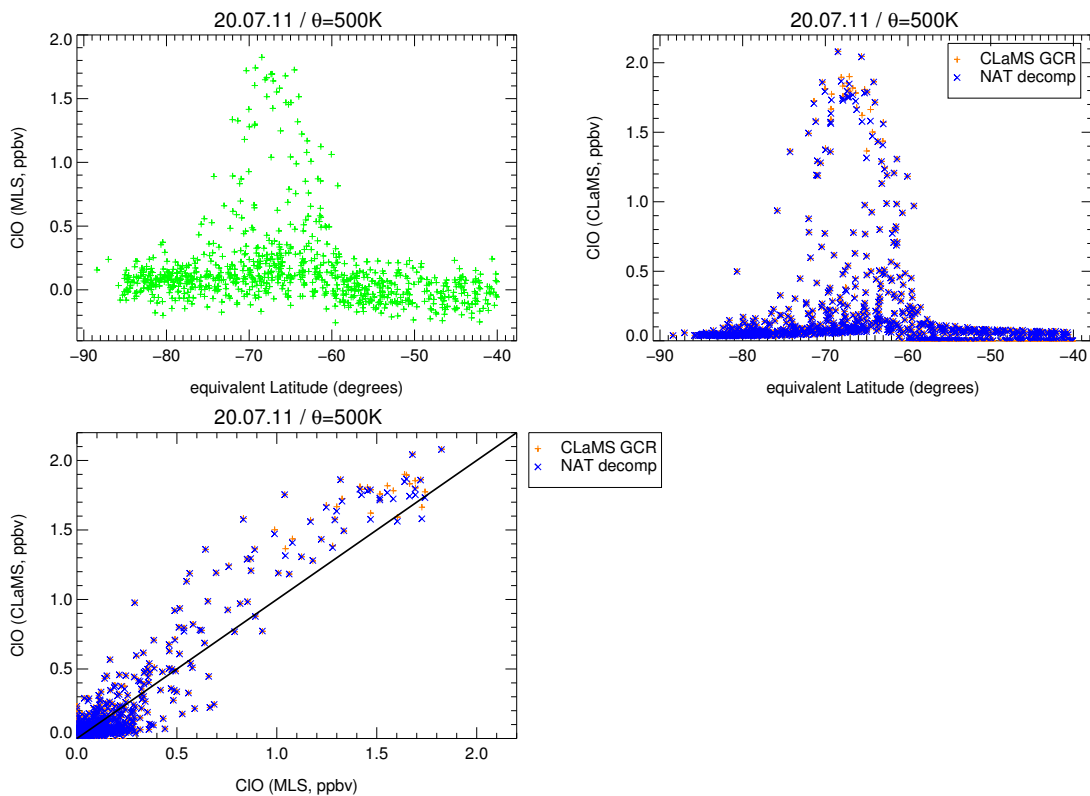


Fig. 2. As Figure 1 but for 20.07.2011.

Printer-friendly version

Discussion paper

