

On the discrepancy of HCl processing in the dark polar vortices

By Grooss *et al.*

General:

The difference between measured and simulated HCl in the polar stratosphere has been a problem for modelers and also for chlorine partitioning studies. The authors address this issue in this manuscript and it merits a discussion. However, more background information on previous modeling works on this subject and HCl/ClO comparisons should be presented in detail. The manuscript is well written at parts and hence, a language editing is also needed. Please find my specific comments below. The manuscript can be accepted for a publication after this minor revision.

Specific comments:

1. Introduction has to be elaborated with previous modeling studies in the polar stratosphere and HCl comparisons (e.g. Feng *et al.*, Wholtmann *et al.*, and Kuttippurath *et al.* articles on polar processing and ozone loss studies)
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.
3. I think that you missed ClO comparisons in this study, although you have a comparison with ClNO₂. 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).
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?
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 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.

Technical:

Page 1

Line 8: and, to date, to varying

Line 22: rates are small

Page 2

Line 12: a major role

Page 3

Line 6: data from the

Line 8: the model results and comparison

Line 9: mixing process related to HCl, and

Line 9: “ show”

Page 5

Line 2: resolution were

Line 16: we use the MLS

Line 17: You did not use ClO data?

Line 24: Use “ However ,” instead of “ unfortunately”

Line 26: delete “unfortunately”

Page 7

Line 11: comparison to other models is absent in this section (e.g. MIMOSA-CHIM, REPROBUS, ATLAS, etc.)

Line 23: latitude calculated from the ERA-interim

Line 27: However, the ClONO₂ observations

Line 31: underestimate”

Line 32: all simulations? From all models?

Page 8,

Line 7: data in mixed polar”

Line 7: differences

Line 8: not likely” , use “are unlikely.....”

Line 8: model differences

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

Page 9

Line 6: show any significant

Line 10: Delete “ Therefore”

Line 30: CALIOP observations, which

Page 11

Line 4: Evidence

Line 14: overestimation

Line 23 and 24: "cross-sections" would be better in this context

Page 12

Line 26: This must be section 7

Line 33: the same model setup

Page 13:

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

Line 18: How much is this minor? Significant?
