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Comment on acp-2021-574

Anonymous Referee #1

Referee comment on "Challenge of modelling GLORIA observations of UT/LMS trace gas and cloud distributions at high latitudes: a case study with state-of-the-art models" by Florian Haenel et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-574-RC1, 2021

The paper of Haenel et al. presents a comparison of observed 2D distributions of species from the GLORIA instrument with model simulations. The authors use one particular flight from winter 2015/2016 at high northern latitudes (POLSTRACC) to compare the capabilities of ICON-ART and EMAC to simulate H_2O, O_3 and HNO_3 as well as cloud occurrence in the UTLS region. The selected flight comprises very different meteorological situations which allows to evaluate different aspects of the relevant model parametrisations.

The ICON-ART data are based on a R2B6 global simulation with a R2B7 nest in the region of interest, the latter corresponding to 20 km horizontal spacing. EMAC data are available at T106 spectral resolution corresponding to a grid spacing of approx. 40 km at 70N. Data are interpolated at the tangent points of the observations and vertical cross section of relevant species are analysed.

Discrepancies are found for cloud occurrence in ICON-ART. Stratospheric water vapour is simulated too high for EMAC not too surprisingly underestimating the vertical gradients. Contrary, ozone is represented well in EMAC while ICON-ART ozone data suffer from the modified LINOZ-scheme.

The authors put a strong focus on the potential reasons for the misrepresentation of clouds in the high resolution simulation of ICON-ART and conclude on matching / timing problems. For EMAC the applied cloud mask better fits the observations, which the authors partly attribute to the lower resolution (noting fundamental model and diagnostic differences). Further, based on T42-simulations of EMAC they show, that the model resolution plays a key role for the H_2O gradients and mixing ratios as well as HNO_3 in EMAC. To check the impact of scavenging on HNO_3, which is only provided by EMAC, they conclude, that scavenging is essential to simulate HNO_3 correctly.

The paper is well written, and illustrates some problems of state-of-the-art models to simulate the composition of the challenging UTLS-region governed by strong gradients and often sub-grid processes. However, the central goal of the study is not clear, despite the authors state: " …with the goal to aid model development and improving our understanding of processes in the upper troposphere/lowermost stratosphere…". It leaves the reader with the main key messages: Resolution matters, chemistry matters which are both not too surprising.

Since the fundamental properties of the model systems are very different, but the resolution is one key aspect of the comparison results the authors should provide in addition a comparison of similar grid spacing (e.g. between T106, R2B6 or coarse graining).

Second, the paper shows a bunch of comparisons and sensitivities for different species, processes and models and partly some very nice diagnostics (e.g. the ICON-ART passive water forecast), which are – and partly have to be – model specific (e.g. scavenging and HNO_3 in EMAC), but what are the consequences e.g. for the model developments, which model parametrisations should be improved?

Third, how representative are the findings based just on one individual flight? Does e.g. ozone also show discrepancies for the early winter, or is HNO_3 affected by scavenging during other months, is the cloud mismatch a general problem, etc.

It is an important issue to assess the capabilities of models of different kind to represent the composition of the UTLS and thus merits publication, but the focus of the given study is difficult to find.

Since the paper is not intended to provide novel aspects of atmospheric sciences, but focuses on the capabilities of models to represent tracer fields in complex regimes, a publication in GMD should also be considered.

Major point: Since the two models differ fundamentally in their basic properties it would be desirable to have at least one similar set of resolutions for comparison, especially since the authors emphasize the importance of resolution for their conclusions. The R2B6 simulation would allow for direct comparisons between EMAC T106 and ICON ART or at least coarse graining of the R2B7 data to the approx. T106 grid spacing at 70N would provide more consistency between both data sets. Alternatively one could think to use a high resolution EMAC simulation corresponding to the R2B7 setting (which might, however, be too expensive...). I highly recommend to add at least one comparison at similar resolutions.

Another principle question for the comparison with GLORIA is the use of weighting functions. Since I'm not familiar with GLORIA data, aren't kernels necessary for a quantitative comparison?

Specific points:

p.16., line 15 (also line 23): Why vortex remnant? Couldn't it be just stratospheric air, which descended as part of the stronger downwelling in the high latitude stratosphere outside the vortex?

p.18, line 32,33: Again, if one compares both models at the same coarse grid spacing, how does this affect ICON-ART H_2O gradients?

Also: When only using stratospheric data away from the tropopause (e.g. H_2O for Ozone > 400 ppbv or PV > 8 PVU): How large ist the water vapour bias away from the gradient regions?

p.18 and Fig. 4/8: The enhanced water vapour from GLORIA above the 4PVU implies cross tropopause exchange. This is an interesting case which would be much stronger, if the authors could provide evidence on the process, by e.g. analyzing trajectories or the history of the moisture filaments before the time of flight by comparing e.g. dynamical tropopause altitude, Lagrangian cold points and moisture evolution in both models before the flight. This would also provide a strong case for publication in ACP.

p.22, line 12/13: "This in turn means...": I can't really follow the statement: What is meant with "this region"? Further: Why does trapping with high altitude cirrus affect the lower stratospheric data? Or do the authors refer to the upper troposphere only? Finally "... could play a significant role " for what?

P.22, line 6 ff.: Why does scavenging has an effect up to 1 km above the 4 PVU surface (Fig. 11f) throughout the measurement region? Wouldn't this imply clouds in the stratosphere over the entire region? Even given the sporadic events shown in the appendix I find this puzzling... Is there any other diagnostic confirming this?

Figure 4:

To diagnose the exchange region, add a panel showing the altitude of the PV=2pvu surface. Figure 4 currently does not provide any indication of cross tropopause exchange.

Figure 9:

Since the overplotting of data points may mask some important details of the distributions, I strongly recommend the following: One could easily calculate the mean and standard deviation of each species in bins of e.g. 1 PVU and could overplot this on the Figures 9a)-9e).

Why does Fig 9.e) shows roughly a 1:1 relation for low PV-values (< 4 PVU), but a systematic difference in B1.(i)? (Eventually this discrepancy disappears after considering my previous comment to Fig.9).

Caption Figure 11: Please add "T106 minus T42 resolution"