Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-821-RC1, 2019

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Evaluation of the CAMS global atmospheric trace gas reanalysis 2003–2016 using aircraft campaign observations" *by* Yuting Wang et al.

Anonymous Referee #1

Received and published: 5 November 2019

Wang et al. perform a comprehensive comparison of aircraft observations against three ECMWF reanalyses of atmospheric composition. Their analysis focuses on ozone (O3) and carbon monoxide (CO), but also includes comparisons of other chemical species such as nitrogen oxides, volatile organic compounds (VOCs), and the hydroxyl radical (OH).

Observations from aircraft campaigns constitute a unique resource to evaluate composition models such as the ECMWF reanalysis suite, and the work by Wang et al. offers a meaningful contribution in that regard. Unfortunately, the manuscript contains little interpretation of the results. Rather, it mostly describes the differences between model

and observations, as already shown in the figures. As it stands, it is unclear what the additional insights are compared to e.g. the study by Inness et al. (2019). I recommend adding some high-level discussion to the manuscript in order to explain the results and provide some context. For example, the model seems to generally overpredict OH, which is consistent with an underprediction of CO in the northern hemisphere. Do the authors have an idea why this is the case? Also, model NOx and HNO3 generally seem to be underpredicted in the free troposphere relative to observations, while PAN tends to be higher. Does this suggest that the PAN production rate is too high?

While an in-depth interpretation of all results is out of the scope of this work, highlighting and interpreting some of the main findings from the model-aircraft comparison would go a long way toward making the paper more relevant.

Specific comments:

- Page 2, line 46: the reference for Wagner et al. 2019 is missing in the References section.

- Page 2, line 59: the authors say that the analysis fields for ozone are 'strongly forced by observations', which seems a bit of an odd statement for tropospheric ozone where the constraints provided by the satellites are relatively weak. It would be helpful to expand in a bit more detail how the assimilation impacts tropospheric CO (where the impacts are strong), ozone (some impact), and NO2 (little impact due to the short lifetime).

- Page 4, line 96: the authors use an impressive number of aircraft campaigns for model evaluation. This raises the question how comparable these measurements are? The uncertainties arising from 'mixing and matching' different instruments should be discussed.

- Page 5, line 153: for both the spatial maps and the vertical profiles the authors solely show the mean values. These are useful but don't tell the full story, especially for the here discussed species that exhibit strong temporal (e.g., daily and seasonal) variations. The ability of the model to capture these variabilities is a key performance metric. It can be somewhat deduced from Tables 4 and 5 but should be discussed in more detail in the manuscript. For example, CAMSRA overestimates ozone in the tropics and Arctic by about 30%. Is this a consistent feature or a seasonal effect (i.e., is the overestimation mostly during spring time –when ARCTAS took place)?

- Page 7, line 215: as part of the CO discussion it would be helpful to discuss the treatment of methane CH4 in the various models. Is CH4 prescribed by all models (and to the same value?), or is it a dynamical species with obvious impacts then on OH and thus CO?

- Page 8, line 247: the assimilation of CO seems to degrade the mean bias relative to the aircraft observations, and generally provide little improvement on the other metrics as well. Do the authors have an explanation for this?

- Page 9, line 271: the authors should add a legend to each figure of vertical profiles to make it easier to distinguish between observations, model, and model background. It would also be helpful to show the observed concentration variation at each level, e.g. by showing the standard deviation (or 25%/75% percentiles) of both the observations and the model comparisons.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-821, 2019.