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

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We thank the referee#1 for taking the time to read the manuscript and offer helpful comments and suggestions. The referee's comment is repeated with our response in bold. Responses to those comments are listed below: 1. 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

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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. Response: We agree with the referee that including further interpretation on the results will give more insight on the model development. We have added more explanations to improve the manuscript as the referee recommended, and in particular we added in the paper a new Section of the concentration ratios of chemically interacting species, so that we can check to what extend the photochemical theory is verified. Note, however, that the main point of this paper is to include additional measurements to the routine observations used by Inness et al. (2019) and Wagner et al. (2019) for the evaluation of the new CAMS reanalysis. The aircraft campaigns provide simultaneous profile measurements of many species such as OH and HO2, which are not taken into consideration in the routine evaluation. Page 2, line 46: the reference for Wagner et al. 2019 is missing in the References section. Response: Wagner et al. 2019 has been updated in the reference section. 2. 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). Response: We modified the sentence and added more explanations in the model description section. 3. 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. Response: Although the campaigns used different instruments to measure O3 and CO, the instruments were all calibrated and have relatively small uncertainties in a range of 3-5 ppb for O3 and 2-5 ppb for CO. In some campaigns that measured the same species using several instruments, we compared the simultaneous measurements, and they are in good agreement. We averaged the data to further reduce the uncertainties. We have added one paragraph to address this issue in the campaign description section. 4. 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)? Response: The strong spatial variation only imply that the model's performance varies with space. But we cannot be sure that whether it is a seasonal effect or regional feature due to the sparse measurements (e.g., there are no different campaigns performed in the same region in different seasons). 5. 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? Response: Methane is calculated in the simulation with constant surface concentrations (as opposed to emissions) applied as lower boundary conditions. The destruction of methane results from the presence of OH that is calculated by the model. OH and thus CO are affected by the calculated methane. We have added a sentence at the beginning of Section 2 to specify how methane is calculated. 6. 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? Response: The control run largely overestimates the CO concentrations in the Southern Hemisphere as shown in Figure 5 – 7. The assimilation reduces the positive bias in the Southern Hemisphere thus degrade the mean bias in Table 5. This statement is added in the manuscript. For the all data analysis, the calculated numbers are largely affected by the extreme values that the satellites cannot capture because of the coarser resolution than the aircraft measurements. After filtering out the plumes, the CAMSRA has larger r2 (0.71) and slope (0.78) than the control run (0.66 and 0.75). 7. 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. Response: We added the legend and the standard deviation in the plots.

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