Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-821-RC2, 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 #2

Received and published: 19 November 2019

General comments:

This paper presents an evaluation of several CAMS reanalysis products against a suite of aircraft campaign data for multiple chemical species. Evaluation of reanalyses against independent data is an important activity and the evaluation here is rigorous. While the manuscript does a nice job of evaluating the performance of the CAMS reanalyses, there is only limited explanation or analysis of the causes of mismatches between CAMS and observations, or the reasons why the newer reanalysis out-performs the earlier reanalyses in some regions. An interesting result of this paper is that for some species and regions of the atmosphere, the reanalysis has only a little improve-

ment, or even weaker performance, than the control simulation. It would be helpful to have more analysis of why this is the case. Overall, the paper would be strengthened by a more detailed exploration of the underlying causes of the biases, as this could provide guidance for future improvements and provide greater scientific insight.

Specific comments:

Line 56: What other species are assimilated?

Line 57: Which specific satellite observations are assimilated, and how does the assimilation system account for the vertical sensitivities of different satellites? The coarse vertical resolution of satellite data compared to aircraft campaign data is a likely cause of some of the biases against aircraft observations, so this should be discussed in some detail.

Line 88: What does "consistent in time resolution" mean?

Line 186: Please define "good". Some rather large biases are mentioned later in the paragraph.

Lines 210-212: Is that difference in r2 statistically significant? Also, wouldn't we expect a larger improvement since ozone is being assimilated? Is the limited improvement due to limitations or uncertainties in the observations, or something else?

Line 264: Similar to the comment above, why does the NO2 agreement not improve when NO2 is assimilated?

Line 282: Are the differences small because the species are well buffered against changes in O3, NOx, etc., or because the assimilation doesn't change the O3 and NOx concentrations very much?

Line 302: The larger biases of CAMSRA with altitude seems like a surprising result since satellite observations of ozone are available in the stratosphere and upper troposphere but not the boundary layer. It would be nice to relate the discussion of the

vertical profiles to a description of where observations are available to constrain the reanalysis.

Line 358: What emission inventory is used? Does it have known biases, or is this a new finding?

Summary: Can you end with some directions for future improvements and/or a take-home message for the atmospheric chemistry community?

Editorial comments:

Lines 32-33: It seems, then, that the reanalysis covers the period 2003-3018.

Line 52: misplaced comma

Lines 308-309: confusing sentence, please reword

Figs. 9-12: Please use thicker lines so they are easier to see.

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