Technical note: The CAMS greenhouse gas reanalysis from 2003 to 2020

Reply to reviewer 1

We are very grateful for the insightful comments from the reviewer to improve the documentation of the CAMS greenhouse gas reanalysis and in the manuscript and the suggestions for potential improvements to be considered in future reanalyses. We have addressed the different comments of the reviewer (in black) to clarify the different aspects of the reanalysis including the selection/quality of observations assimilation, the constrain of the annual global growth rates and the quality of the prescribed surface fluxes (see text in blue).

Review of "The CAMS greenhouse gas reanalysis from 2003 to 2020" by Agusti-Panareda et al.

This paper describes the greenhouse gases (CO2 and CH4) reanalysis as prepared for the Copernicus Atmosphere Monitoring Service (CAMS). Manuscript draft is well prepared for understanding of the work that is carried out. The method is well described but the results are not fully satisfactory yet. Many traditional inversions produced better statistics for model simulated CO2 and CH4 concentrations by optimising the surface sources and sinks (e.g., Chandra et al., ACP 2022). However, the approach holds different promises for 4D concentration product of CO2 and CH4 in near real time along with other meteorological parameters of ECMWF weather forecasting system. In general I recommend publication of the manuscript as "Technical note", after accounting for some of my comments below.

Line 48: are the SCIAMACHY and IASI data good enough for assimilation ? the growth rate may be fine but the spatial distribution are probably of poor quality.

I have found later that this is mentioned in the caveats. No action is needed but I am just making sure to mention the points of concern.

The rationale for the selection of the observations assimilated in the CAMS reanalysis and their quality will be emphasized in the revised version of the manuscript.

Figure 1: I have several doubts on these plots.

1) the total column CO2 values on Dec 2020 are of similar magnitude of CO2 at MLO (414 pm as per SIO flask data). I was thinking that global mean XCO2 will be a couple ppm lower than MLO value. Please confirm.

Figure 1 gives an overview of the CAMS GHG reanalysis dataset including global trends and spatial/temporal distribution at seasonal sales. The global mean has an error ranging from -0.7 to 3.5 ppm. Figure 14 shows that global mean error in 2020 is +1.35

ppm. This is consistent with the assessment of the reviewer. The problem of constraining the global mean and annual growth rate is documented in section 3.3 and mentioned in section 4 (point 6). The range of error in the global mean will be added in the revised version of the manuscript.

2) What is the advantage of showing 2003-2020 mean as opposed to just 2020 seasonal means?

The purpose here is to show the typical seasonal cycle. For this reason, using 2003-2020 mean as opposed to just 2020 seasonal means is better because individual years can be affected by the large inter-annual variability of biogenic fluxes (e.g. during el Nino years). This will be mentioned in the revised manuscript.

Figure 2: Should the two-way arrows between Forecast "Sphere" and Surface flxes "Maps" be one-way? From Fluxes to Forecast

The two-way arrows indicate the two-way coupling between the atmospheric model and the model of biogenic surface fluxes. The surface fluxes are affected by the forecast of temperature, radiation, humidity and soil moisture and the atmospheric CO2 in the forecast is affected by the surface biogenic fluxes. This will be clarified in the revised version of the manuscript.

Table 2: You could use FF-CO2 emissions EDGARv6.0 etc. or the GridFED by UEA group. Similarly many of the flux components of CO2 and CH4 should be revised by using the most recent flux data sources. I not saying for this paper but in the near future.

The current reanalysis cannot be changed as the reanalysis system requires a consistent configuration of the model and prescribed input fluxes throughout the whole reanalysis, but we will certainly use a newer version of the anthropogenic emissions in the next CAMS reanalysis planned for 2024. This will be mentioned in the revised manuscript.

Line 352: I think it would have been nice to add/subtract some CO2 flux to make the CO2 flux budget consistent with observed CO2 growth rate. At the very least the land biosphere + ocean exchange could be added/subtracted with an offset to balance all sources+sinks = 2.12 * Growth rate. Then the statistics may be look that bad.

This adjustment of the fluxes with the CO2 growth rate will be considered in the next CAMS re-analysis. There is the caveat that the growth rate will have to be adjusted in near-real time which might not be feasible. A note of this option will be made in the discussion section of the revised manuscript.

Figure 7: It would be nicer to use a discrete colour scale (like in Fig. 7). Otherwise, it is hard to distinguish between medium blue/red from dark blue/red

Also consider showing a Taylor diagram, like I see below for the TCCON sites

We will change Figure 7 to use a discrete colour scale and add a Taylor diagram in the revised manuscript.

Do you need this Figure 14 ? If needed I suggest a more detailed site information is shown before Fig. 7. The Surface, MBL, TCCON, NDAAC, AirCore etc. sites can be shown in different symbols

The map of the MBL sites is useful to be able to interpret the observed global mean produced by NOAA. A detailed list of TCCON, The NDACC, AirCore is provided in the Appendix. In the revised manuscript the map in Figure 14 will be updated to show the different types of observations with different symbols and placed before Fig. 7 as suggested by the reviewer.