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Interactive
comment

Interactive comment on "An inter-comparison of tropospheric ozone reanalysis products from CAMS, CAMS-Interim, TCR-1 and TCR-2" *by* Vincent Huijnen et al.

Anonymous Referee #2

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This paper intercompare four tropospheric ozone reanalyses against independent observations. Each reanalysis and the independent observations are relatively well described. The intercomparison is done between 2003 and 2017 over a large number of diagnostics covering different situation of tropospheric ozone chemistry. There are nevertheless many shortcomings in this manuscript. First, the four reanalyses are not independent (two – CAMS-iREAN and TCR-1 – are the ancestor of the two letters – CAMS-REAN and TCR-2) which is confusing. Moreover, TCR-1 seems to have changed since its published paper (Miyazaki et al., 2015) which is even more confusing. There is a lot of discussion on the impact of change in the observing system during the reanalyses but these are not clearly shown. Finally, the overall presentation is poor

– figures and text – which make the paper difficult to be recommended for publications after minor revision. Here below are my detailed comments on the paper where I provide direction for improving the manuscript.

Major comments.

There are several aspects of the study that should be revised before the paper be accepted in GMD which are listed below:

1. The paper uses four reanalyses which are by far not independent. CAMS-REAN has been built above CAMS-iREAN in order to solve some of its shortcomings. This is the same for TCR-2 vs TCR-1. For me, the authors need to refocus the study by comparing only CAMS-REAN and TCR-2. If they want to compare CAMS-REAN and CAMS-iREAN, this should be done in a separate section. For TCRs, such a section is necessary since no publication have done a dedicated comparison as it is the case for CAMS in Inness et al. (2019).

2. There is a large confusion between TRC-1 (Miyazaki et al., 2015, available here https://ebcrpa.jamstec.go.jp/ miyazaki/tcr) and the version used in this paper. First, two different names should be used for these two different products. TCR-1 being already used, I suggest TCR-M (for MIROC) or anything that would clarify the confusion. But TCR-M seems closer to TCR-2 than TCR-1, except for the model spatial resolution. Moreover, on the TCR-1 webpage, it seems that surface NOx has been updated from Miyazaki et al. (2015) so it is difficult to know what is really TRC-M. In the revised paper, and in the section comparing TCRs reanalyses as suggested above, the authors should compare TCR-2 and TCR-1, not TCR-2 and TCR-M.

3. The paper lack a dedicated section on the changes in the observing systems and its impact on the reanalyses which is largely commented throughout the paper. How does the time series of the Obervation-minus-Forecast statistics affected by

these changes? Or the χ^2-test, or the spread of the ensemble for EnKF systems, or the size of the analysis increments, or the number of relevant observations, or the comparison with a control run... This is essential for the users to know what they could expect – and what they can't – from these products. Regarding the use of the assimilated observations, the paper discuss ozone reanalyses in the polar region where TCRs are poorly constrained (no TES observations poleward 72°). What is not said in the paper is that CAMS reanalyses are probably not well constrained as well in the winter poles since the assimilated ozone column are from UV sensors which are blind during the polar night. In the revised manuscript, I suggest removing all the discussion related to the polar regions (thus removing these regions also from the figures).

4. The figures need to be improved. The resolution of all the figures are too small. Many readers, like me, will try to zoom into them in the PDF document, which is not possible with their current resolution. Please, increase them. For the line plots, add a grid in the background of the figure. In general, the fonts are too small, they must be increased, as well as the line width. The legends are not always complete, please, describe everything shown in the figure. E.g. in Fig. 4, what is the dashed line referring to the left y-axis (which I cannot read due to the small size of the fonts)? You must also write what is shown when biases are plotted: obs-reanalyses or reanalyses-obs. If normalized differences, what is the norm? In Fig. 5, the colour levels in the bias are not very well chosen because it appears that all of the reanalyses seems to be highly biased. Why not using a constant colorbar with large steps showing only relevant differences? To extract major signal from the time series, I am suggesting plotting moving average allowing to detect the major differences between the observations and the reanalyses. Also, their readability will be improved by plotting the values of CAMS-REAN and TCR-2 only.

5. Many aspects of the conclusions and in the abstract are not shown in the paper,

e.g. the impact of the change in the observing system or the differences between the forecast models. On the other hand, the performance of the reanalyses in different tropospheric layer, conditions and seasons – which what this paper discusses – is almost ignored. In the conclusions it should make clear of what are the findings of this paper and what are subjects for future research.

6. The writing lack of clarity. For example, I do not understand the first sentence of the introduction. A careful reread of the paper is necessary to improve its readability. See some example in the specific comments.

Other general comments

1. Tables 5-9 provides a summary of the performances of each reanalysis compared to independent observations. This information is important and the values in the tables are mentioned throughout the paper. I have two major concerns with these tables. First, extracting the comparison between the reanalyses is difficult and I suggest replacing the tables by bar-plots. Second, I suggest replacing the RMS with the standard deviation of the difference. The RMS combines a measure of the bias and the variability of the difference. Since the bias is already provided, the standard deviation will tell us by how much the differences are distributed around the bias. For these figures, TCR-1 and CAMS-iREAN could be compared with their updates versions.

2. Also regarding differences, how are them calculated: obs-rean or the opposite? When normalized, what is the norm?

3. In Figure 3, the authors define the tropopause in each product as the altitude where ozone exceeds 150 ppbv which means that the altitude of the tropopause change from a product to the other. I suggest taking a surface pressure as defining the upper level of the free troposphere, e.g. 200 or 300 hPa. By using 300

hPa, they will be able to remove Fig. 12, which I suggest. Also, why showing the number of stations and not the number of soundings?

4. Regarding the use of the observations and in addition to my major comment above, the Tables 2 and 3 need to be revised.

   (a) As far as I know, there is only one CCI product for SCIAMACHY/GOME-2 TC and MIPAS profiles. I thus recommend to remove "(BIRA)" and "(KIT)".

   (b) What version of SCIAMACHY CCI is used? Same for MIPAS CCI, and GOME profiles? (I understand that NRT products have version changing during the time but this should not be the case for scientific – or offline – products.)

   (c) Also, does CAMS-iREAN and CAMS-REAN both assimilated MIPAS ESA NRT and CCI profiles? Which seems to use twice the profiles of the same instruments? Please, clarify. I am also surprised to see that CAMS use MIPAS NRT, a product older than 15 years and which was reprocessed by ESA several times (the ESA offline v7 is now the latest validated version).

   (d) You also mention MLS V3.4 which does not exist (at least for the offline products) – this is it either V3.3 or V4.2 (or shortly V3 or V4).

   (e) I would also add the reference to each dataset in an additional column.

   (f) The MLS version used in TCR-1 and TCR-2 are not clear. Version 4 is mentioned in the text while Table 4 mention version 3. Please clarify. Also use the appropriate MLS data quality document when referencing a version.

5. The terminology of "error statistics" is misused in the paper. It is generally applied to the error statistics in the DA system (i.e. B and R matrix and model error if any). In the case of this study, it is applied to the differences between the reanalyses and the observations so I would use the "observation-minus-analysis" statistics instead.

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6. The authors use the inter-annual variability (IAV) and elsewhere deseasonalized anomaly, which seems to reflect to the same quantity. Could they clarify and use only one of those terminology?

7. I prefer the acronyms CIRA and CAMSRA, it is much easier when speaking than CAMS-iREAN and CAMS-REAN.

8. Many acronyms are undefined and should be.

Specific comments

L13-16: "Global tropospheric ozone reanalyses constructed using different state-of-the-art satellite data assimilation systems, prepared as part of the Copernicus Atmosphere Monitoring Service (CAMS-iRean and CAMS-Rean) as well as two fully independent Tropospheric Chemistry Reanalyses (TCR-1 and TCR-2), have been intercompared and evaluated for the past decade." This is not true. CAMS-iREAN and TRC-1 are not constructed using state-of-the-art satellite data assimilation systems since these systems have been updated for CAMS-REAN and TCR-2.

L18-20: "The improved performance can be attributed to a mixture of various upgrades..." This is not shown in the paper.

L21-23: "Meanwhile, significant temporal changes in the reanalysis quality in all the systems can be attributed to discontinuities in the observing systems." Idem, this is not shown in the paper.

L22-24: "To improve the temporal consistency, a careful assessment of changes in the assimilation configuration, such as a detailed assessment of biases between various retrieval products, is needed." Which is what this paper should have been shown.

L24-26: "Even though the assimilation of multi-species data influences the representation of the trace gases in all the systems and also the precursors' emissions in the TCR reanalyses, the influence of persistent model errors remains a concern, especially for

the lower troposphere." Again, this is not shown in the paper.

L31-32: "The global distribution of present-day tropospheric ozone..." I don't understand this sentence, please, rephrase.

L41: "...tropospheric ozone, but are generally..." => "...tropospheric ozone, which is generally..."

L45: "Tropospheric ozone is reasonably well monitored..." You are talking about surface ozone in this sentence so I would write "Surface ozone is reasonably..."

L50-52: This list of satellite dataset is incomplete (missing are e.g. OMPS and TROPOMI for the most recent instruments) so I would write "These observations are complemented with (combined) satellite observations from, e.g., GOME-2, ..."

L62-64: "Simultaneously international modelling initiatives..." I don't understand this sentence, please, clarify.

L77: "...individual measurements suffer..." Do you mean "...individual measurements which suffer..."?

L81: "...particular constellations of pollution..." What do you mean by "constellations"?

L85: "However, all of these applications presume that the reanalysis is sufficiently accurate,..." What matter is that reanalysis is well characterized more than accurate.

L118-119: CAMS-REAN and CAMS-iREAN acronyms are undefined.

L126: "NOx" => "NOx"

L129: "...changing constellation of ..." => "... the change in the observing system..."

Table 1: What are the output frequency of each product. Are the output snapshots or time averages?

L156: => "The meteorological model version is CY40R2."

L157: "In terms of ozone, observations from the following set of satellite instruments have been assimilated:. . ."

L159: "Limb observations are instrumental to discriminate. . ." => "Profiles from limb instruments (MIPAS and MLS) are used to discriminate. . ." Could you explain how does limb profiles are used to discriminate the tropospheric and stratospheric contribution of the total column observations?

L161-163: See my general comment above regarding MLS V3.4.

L211: Remove reference to Watanabe et al. since it is already provided 2 lines above.

L341: "In the TCR systems,. . ." Move this info in Sect. 2.3.

Figure 2: What is the difference between the part in page 14 and 15? It seems to be the same.

L376: ". . .both model and observations. . ." Which model? Do you mean the reanalyses? If yes, replace by ". . . the reanalyses and the ozonesondes. . ."

L377: same as above "modelled" or "analysed"?

L379-381: Is the poor correlation between reanalyses and observations due to the missing total column observations during the polar night? Since, as far as I know, none of the total column assimilated data are taken by emissions instruments thus failing to measure during the night?

L397-399: "During 2003 and 2004 both CAMS reanalyses. . ." Why? This is not related to GOME data since CAMS-REAN does not assimilate GOME.

L399: ". . .GOME observations. . ." => ". . .GOME nadir profiles. . .".

L400-403: Why does CAMS assimilate MIPAS NRT and not the offline reprocessed products delivered by ESA?

L412-413: "Also both the observations and reanalyses indicate an upward trend of

tropospheric ozone in the UTLS. . ." I don't see this from figure 4. Could you clarify?

L431-433: "From 2011 onwards the correspondence with observations improves remarkably, despite the lack of TES measurements in TCR from June 2011 onwards." Why?

L434-444: I do not agree with most of what is written. âĂć "In the tropics, . . ." This is not true for CAMS-iREAN which generally underestimate the ozonesondes. âĂć ". . . both CAMS reanalyses show a strong peak . . ." In fact, TCRs also show a peak. âĂć ". . .overestimation of up to 20 ppb." None biases are going up to -20 ppb. I would rather say -15 ppb. Do not omit the sign of the bias in the comparison. âĂć "This spike appears much weaker in TCR. . ." Does the reason not due to the fact that TCR also optimize surface emissions allowing the reduce the bias with observations? âĂć But the authors does not discuss the fact that CAMS-iREAN seems to have the best agreement with ozonesondes during the whole period and they should comment on the reason for this.

L449: "332 hPa" => "382 hPa"?

L467-474: I see other reasons for the seasonal variations in the bias time series than those mentioned in this §. For CAMS products, their troposphere is not constrained by any data during the polar night since all of the assimilated nadir instruments are measuring UV sun-scattered light. For TCR, TES ozone data are only available at latitude lower than +/-72°. Could the author comment on that?

L473: "332 hPa" => "351 hPa"

Sect. 5.2: "Figure 6 presents the temporal variability. . ." Well, figure 6 is a scatter plot without any time axis (on the x-, y- or any colorbar) so I would change this sentence. Moreover, all the discussion in this §related to seasonal differences are not supported by Fig. 6. I understand that Fig. S3 could support this discussion but as being part of the supplement, it cannot be used for new discussion.

L521: Here and at several other placed "R=0.89 – 0.96"? Do you mean "R between 0.89 and 0.96" or "RïČŐ[0.89,0.96]"? Or something else?

L541: "We compute the interannual. . ." Do you mean the deseasonnalized anomaly for each region? See also the general comments.

L652: Do you mean Fig. 12? So this is almost Fig. 3 without observations. Is it really the annual mean? It seems more to be a time series of monthly mean?

L669-670: The change in behaviour is clear above the SH polar latitude but less clear in SH midlatitudes.

Figure 13: âĂć Is it as Fig. 7 but for PC surface-300 hPa in south-east Asia and ENSO? âĂć "A 2-month smoothing". Do you mean a running mean or moving average? âĂć What is TSI?

L742: ". . .annual mean. . ." For which year?

Figure 15 is very interesting but I would add the ozone sonde values in order to assess the quality of the reanalyses against the best estimation of the truth (i.e. the sondes).

L767: "The changing constellation. . ." I would rather say "The changes in the observing system. . ."

L770: "This calls for a detailed evaluation of the capability of the current reanalyses of tropospheric ozone." Do you mean this is something to do in the future? Please, clarify.

L793-795: "In the TCR reanalysis, the chemical concentrations and precursor's emissions were simultaneously optimized through EnKF data assimilation, which was important in providing information on precursors' emissions variations (Miyazaki et al., 2014; 2017; 2019a; Kiang et al., 2018) and in improving the vertical profiles of ozone." Well, this is not shown in the paper so I would remove this comment from the conclusions.

L800-803: "Meanwhile, the analysis ensemble spread . . ." Well, again, the TCRs en-

semble spread are not shown in the paper. Also, what do you mean with "4D-var could be used . . ." Altogether, I don't understand the message in this sentence.

L413: The acronym UTLS must be defined.

L819: " . . . a careful assessment of changes in the assimilation configuration. . ." Which what this paper should have done.

L822: "The assimilation of multi-species data influence. . ." This has not been addressed in the paper.

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