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# Comment on acp-2022-458

Anonymous Referee #3

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Referee comment on "Assimilation of S5P/TROPOMI carbon monoxide data with the global CAMS near-real-time system" by Antje Inness et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-458-RC3, 2022

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In their paper, Inness et al descibe the assimilation of TROPOMI CO in the world-leading CAMS analysis system. The combined assimilation of TROPOMI, MOPITT and IASI CO is described in detail, and major advances are reported by using TROPOMI. The paper is well written, is presenting important results, and is complemented by validation results with independent observations. I am much in favour of publishing these results. Several questions came up when reading the paper, and I would ask the authors to answer those before the paper is published.

IAGOS data is used prominently in the paper so it seems reasonable to invite an IAGOS team member as co-author, or at least mention the programme explicitly in the acknowledgement.

The paragraph starting with “The differences between the observations and the model equivalents are called departures" (page 3) is general and rather long to my taste. The sensitivity of the departures to detect small changes is an important point to make I agree.

"Initial work about the use of early TROPOMI CO data in the CAMS system was published in an ECMWF technical memorandum (Inness et al., 2019c) but not in a peer reviewed publication."
The early paper of Borsdorff (a co-author) from 2018 already showed striking good agreement between early TROPOMI retrievals and the CAMS system. Reading this paper it is a bit surprising that "a more mature retrieval version" seems to be needed for the assimilation to be successful, as is suggested by the text of the introduction? The good initial comparisons are discussed on page 10.

"contains a lot of additional work," (page 3, line 18) If this is relevant, it should be

specified what the additional work consists of.

Could you please comment why data with LAT < 60S is blacklisted?

Equation 2, 3: I would expect to see error terms here to reflect measurement and retrieval uncertainties, and forecast errors, especially since the "true" atmosphere is introduced here. The equations suggest that the true state can be quantified which is not the case. I would recommend that the error terms are explicitly added to these equations to avoid confusion. "d" is resulting from these errors.

"we remove the influence of the a-priori profile" (page 5, line 9) In general the a-priori does not only appear explicitly in these equations, but also implicitly through the dependence of the retrieval "y" and retrieval error on the a-priori, since a badly chosen a-priori will generally lead to larger retrieval errors and larger d values.

"the thickness of the 1000-300 hPa layer," is a bias predictor (page 5, line 15) What is the physical relation between the bias and this quantity?

NEWBGE and OLDBGE. In many data assimilation applications the background error is scaled using chi-square statistics, which basically requires that the background errors are consistent with the computed "d" values and observation errors. Is something similar done in the CAMS system? Phrased differently: is there statistical evidence that the NEWBGE is more realistic than OLDBGE?

Also for equation 4 (page 10) I would suggest to explicitly add the error terms.

"with a mean difference between the data sets of 3.2±5.5 %" (page 10, line 21) Is CAMS higher or lower?

"9.22 ± 3.45% against standard TCCON XCO data and 2.45±3.38% against TCCON unscaled XCO " (page 10, line 27) Please explain why there are two numbers and which one is reflecting the actual bias more.

From the abstract I got the (maybe wrong) impression (I realise it is not explicitly mentioned in this way) that the retrieval upgrade to version 02.02.00 was quite important for the success. From section 2.2, however, it is not so clear how much the retrieval versions 1 and 2 differ. The validation results are discussed, but the versions are not separately reported. Is this information available? Is there a clear indication of

changes/improvements in version(s) 2 compared to retrievals before Summer 2021? It would be good to have some quantitative information of the differences before the comparisons with CAMS are presented.

"It is not clear yet if this is the direct result of the assimilation of TROPOMI data leading to increased CO in the upper troposphere, or it could be the result of convective transport. " (page 18) I guess this could be easily studied by looking at the vertical profile of the analysis increments.

"illustrating that the assimilation of TROPOMI CO improves the fit of the CAMS analysis to the IASI-BC data globally." (page 20 top) A really nice result!

The results for West Africa in Fig 16 could be linked to panel b in Fig. 10, and provides evidence I guess that the increments around 200 hPa are a good result. Please add a comment in the text.

Figure 16 caption: IASI should be IAGOS

What is the relation between "Marco Polo" and China surface observations in Fig 18? Should this be CNEMC?

The results suggest that the low biases in CAMS are linked to a low bias in emissions. Are there future approaches developed to improve this situation, e.g. a bias correction applied to the emissions or emission inversions? Please comment at the end of the conclusions section.